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THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

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THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

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VOLUME 51 SPRING, 1977 NUMBER 1

EAST OR WEST? THE GEOGRAPHIC ORIGIN OF THE BLACK DEATH*

JOHN NORRIS

The sudden onset of plague in Southeastern Europe and the Middle East in the winter of 1346–47 which began the epidemic known as the Black Death inspired among contemporaries and among historians down to the present day various attributions concerning the geographic origin of the outbreak. Based on evidence of varying degrees of reliability, and heavily interdependent, these attributions all have one thing in common: they place the point of origin well to the *east* of the first identified point of human outbreak in Southeastern European Russia. It is the contention of this paper that the Black Death did not originate in China, India or "Central Asia" as has been supposed, and was not "brought" westward by the Mongols or anyone else. It was probably moved *northward* over the course of centuries, by means of transmission among the wild rodent colonies, from Kurdistan and Iraq to Southeastern Russia, from where it spread to Europe, the Middle East, Turkestan, the South Siberian steppe and, eventually, to Mongolia; but not to China, which received the disease via India and Burma.

To demonstrate this thesis we must explain the fallacy of theories of rapid,

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* Presented at the annual meeting of the American Association for the History of Medicine, Galveston, Texas, May 14, 1976. This essay was prepared with the support of a Senior Research Scholarship from the Killam Foundation. I wish to express my gratitude to my colleague Dr Peter Harnetty for his painstaking criticism of the paper.

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extensive, and covert movement of the disease through the human population in sparsely-inhabited areas along the trade routes of Asia. We must also explain how the plague micro-organism is actually moved by the population dynamics of its hosts.

Of what help is the historical record in establishing the argument? Palaeontological and historical evidence suggest that plague has existed as a zootic disease since the late Eocene period,[1] and that it has been a human disease in the relatively densely populated areas of Southwest Asia for at least three thousand years.[2] The first great outbreak of which there is much record, the "Justinian" plague of the sixth century A.D., originated in the Middle East or, possibly, Ethiopia.[3] It spread throughout the Mediterranean lands, to Western Europe and perhaps as far as the British Isles. There were numerous recurrences in the Middle East, Eastern Europe and the Western Mediterranean in the sixth, seventh and eighth centuries.[4] It is generally assumed that these outbreaks originated from some Middle Eastern focus,[5] though there is no good reason to rule out local foci among the wild rodent populations as the source of at least some of them. On the evidence of repeated epidemics, plague appears to have been established in enzootic form at various times in Iraq, Persia, Syria and Egypt at least until the eleventh century, and possibly continuously until the outbreak of the Black Death in the mid-fourteenth century.[6]

Attributions of the Black Death's origin to China, India, Central Asia or Russia are all based on a few original accounts, to which subsequent interpreters have added their own glosses, of varying degrees of accuracy and hon-

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[1] V. N. Fyodorov, "The question of the existence of natural foci of plague in Europe in the past," *J. Hyg., Epidem., Microbiol & Immunol.* [Praha], 1960, *4*. 135-141.

[2] The first clear identification is that in 1 Samuel, chs. 5-6 (cf. L Fabian Hirst, *Conquest of the Plague* (Oxford: Clarendon, 1953), 7-8, W J. Simpson, *A Treatise on Plague* . (Cambridge: University Press, 1905), 3-5, Sir W P. MacArthur, "The occurrence of the rat in early Europe The plague of the Philistines (1 Sam., 5, 6)," *Trans Royal Soc. Trop. Med* , 1952, *46*(2): 209-212; *46*(4) 464, S. H. Blondheim, "The first recorded epidemic of pneumonic plague. The Bible, I. Sam. VI," *Bull. Hist. Med.*, 1955, *29:* 337-45.

[3] Evagrius Scholasticus, *Historiae ecclesiasticae* (ed. Paris, 1673), lib iv, pap, xxviii; J.-N. Biraben and J. Le Goff, "La peste dans le haut moyen-âge," *Annales: Economies, Sociétés, Civilisations*, 1969-70, *24* 1492-3, Michael Dols, "The Black Death in the Middle East" (Princeton, Ph D. thesis, 1971), 10-11; Dols, "Plague in early Islamic history," *J. Amer. Oriental Soc,* 1974, *94*. 372-3.

[4] Biraben and Le Goff, *op cit* (n. 3 above), 1493-1508; Raymond Crawfurd, *Plague and Pestilence in Literature and Art* (Oxford Clarendon, 1914), 104-106.

[5] Biraben and Le Goff, *op. cit.* (n 3 above), *passim*.

[6] Dols, "The Black Death in the Middle East," 19-27; Dols, "Plague in early Islamic history, " 374-81; Alfred von Kremer, *Ueber die grossen Seuchen des Orients nach arabischen Quellen* . . . (Wien, 1880), 125, 128, 133-5, 155; Simpson, *op cit*. (n. 2 above), 19-21.

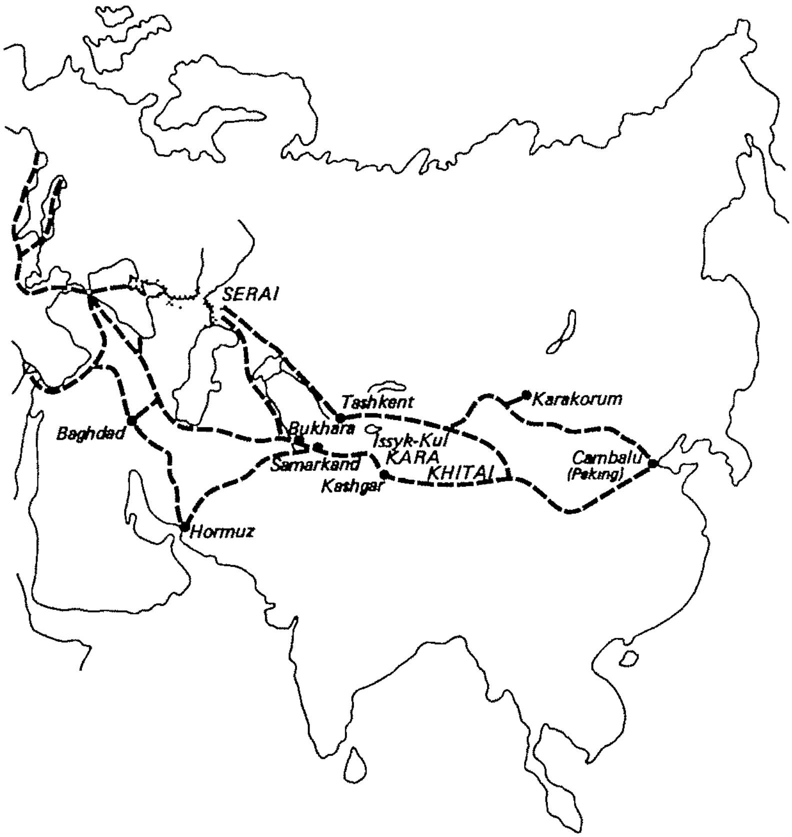

Fig. 1. Central Asian Tracks (& the Supposed Track of Plague Westward)

esty. None of the original accounts is, strictly speaking, contemporary, since all were written at least a few years after the event, and were coloured by the knowledge of the plague's ravages in Europe and the Middle East. The attributions may be grouped according to the supposed point of origin chosen.

1. *China:* Almost all modern Western interpretations which regard the plague as having come from China (Fig. 1) are based on a few European chronicles and on the summary of Chinese literature on the subject contained in the Chinese imperial encyclopaedia *Ku Chin T'u Shu Chi Ch'eng*, published in A.D. 1726. The nearly-contemporary sources include an anonymous Flemish epistolary chronicle, citing Louis Sanctus of Beeringen,

the friend of Petrarch;[7] the "Storie Pistoresi";[8] the "Chronicon Estense";[9] and Gabriel de Mussis Placensis' "Ystoria de morbo sive mortalitate quae fuit anno Domini 1348."[10] All record wondrous disasters in the East in the fifteen years or so preceding the outbreak of plague in Europe. The catastrophes included: earthquakes which destroyed whole mountain ranges; floods; and rains of fire, stones, toads, lizards, serpents and blood. All but de Mussis record poisonous miasmas from these commotions which spread the disease throughout Asia and on to Europe. These sources appear to be repetitive and perhaps are cumulative, each adding to the Flemish chronicle.

Ku Chin T'u Shu Chi Ch'eng records a series of unspecified "pestilences" from 224 B.C. in various regions of China. In 1333, 1334 and 1337 these were associated with natural disasters: floods, famines, droughts, earthquakes and plagues of locusts. Later there were localized "pestilences" without horrendous natural phenomena in 1344, 1345, 1352, 1354, 1356, 1357, 1358, 1359, 1360, 1361, 1362, 1363, 1369, 1380, 1402 and every few years thereafter down to the date of the encyclopaedia's publication.[11] This account of natural disaster was appended by the Abbé de Guignes to his discussion of the Black Death, published in his *Histoire général des Huns, des Turcs, des Mongols &c* . . . (1756-58).[12] De Guignes had been led to this source by an Arab historian[13] who had noted that the European plague came from Cathay or Tartary, where the smell of corpses had spread on every side, and that immediately previous to this onset China had been visited by disastrous floods that had left the country almost depopulated. The German historian Hecker, in 1832, reproduced de Guignes' note almost word for word, but embellished it from the European accounts to include a theory of "telluric concussion" causing progressive infection of the various zones of the earth by a poisonous miasma containing the germs of plague.[14] A variety of more recent historians—Sir Henry Howarth,[15] Braudel,[16] Ackerknecht,[17] Deaux,[18] and Ziegler[19]—have repeated the attribution in Hecker's account.

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[7] J. J de Smet, ed., "Breve chronicon clerici anonymi," *Recueil des chroniques de Flandres* (Bruxelles, 1856), III, 14-18

[8] "Storie Pistoresi," in L. A. Muratori, ed., *Rerum italicarum scriptores* (Milan, 1725-51), 11, V, 237

[9] "Chronicon Estense," Muratori, *op. cit.* (n 8 above), 15, III, 160

[10] Gabriel de Mussis Placensis, "Ystoria de morbo sive mortalitate quae fuit anno Domini 1348," in ed. Henschel, H. Haeser, *Arch. f d. ges. Med* , Bd 2 (Jena, 1842)

[11] *Ku Chin T'u Shu Chi Ch'eng* [Imperial Encyclopaedia] (Peking, 1726), vol. 45, 114 chuan. I am indebted to Mr. Shiu Yim Ts'e, Librarian of the University of British Columbia's Asian Library for a correct translation of the relevant passages of *Ku Chin T'u Shu Chi Ch'eng*.

[12] J. de Guignes, *Histoire général des Huns, des Turcs, des Mongols et des autres Tartares occidentaux* (Paris, 1756-58), V, 223-24.

[13] al-Marqrizi (*Suluk*, pt 2, vol 3, 772-91) according to Dols ("The black Death in the Middle East," 33, 35), Abd-el-Mahasin, according to Creighton and Simpson

This readiness to accept the attribution to China was encouraged in the nineteenth century by a combination of ethnic prejudice, a sense of Western technological and cultural superiority, and the fact that plague by this time seemed to be confined to technologically primitive regions like the Far East. The development of the germ theory further encouraged the association of plague with Asia and insanitary primitiveness, even though, by 1914, it had been determined that plague was not a "filth" disease. The fact that the third pandemic originated from Southwestern China seemed to clinch the matter: if plague originated in China in the nineteenth century, why not in the fourteenth as well?

Ku Chin T'u Shu Chi Ch'eng does not actually provide a *description* of plague until 1644 A.D. Wu Lien-teh, though he went so far as to interpolate a plague entry for 1346 in his summary of *Ku Chin T'u Shu Chi Ch'eng* which does not exist in the original,[20] nevertheless acknowledged that there was a surprising absence of references to plague in Chinese literature. He was able to find only two references to "malignant bubo" in early Chinese medical literature—in 610 and 652 A.D.—with partial descriptions. The first description of an epidemic is to be found in 1642 and probably refers to the same outbreak as that recorded in *Ku Chin T'u Shu Chi Ch'eng* for 1644.[21] In another work, Wu wrote that "pestilences" following in the wake of wars, famines, floods and other natural catastrophes had nothing to do with plague, but "were in all probability typhus and relapsing fever—diseases apparently rampant in China from the earliest times."[22]

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[14] J. F. C. Hecker, *The Epidemics of the Middle Ages. No. 1. The Black Death in the Fourteenth Century* (Berlin, 1832; trans. B. G. Babington, Philadelphia, 1837), 17-25.

[15] Sir Henry Howarth, *History of the Mongols from the Ninth to the Nineteenth Century*, Part II, Division I (London, 1880), II, 175.

[16] Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II* (Paris, 1946, 1964; New York: Harper Row, 1972), 64.

[17] Erwin Ackerknecht, *History and Geography of the Most Important Diseases* (New York. Hafner, 1965), 12.

[18] George Deaux, *The Black Death*, 1347 (London: Hamish Hamilton, 1969), 2.

[19] Philip Ziegler, *The Black Death* (Harmondsworth: Penguin, 1970), 13-14. Dols ("The Black Death in the Middle East," 35) states that by the end of 1346 A.D. "it was known at least in the major European seaports that an unprecedented pestilence was sweeping the Orient." This is based on Ziegler (p. 15), who misquotes Hecker (*op. cit.* [n. 14 above], p. 26) whose statement makes no mention of what was known in the European seaports.

[20] "1346. A plague began in China: it raged through Asia, and depopulated a great part of Europe. This pestilence was considered to be the progenitor of the Oriental plague. It originated after the terrible earthquakes and changes of the soil, after the world had been free from this scourge for 700 years" (Wu Lien-teh, J. W. Chun, R. Pollitzer and C. Y. Wu, *Plague, A Manual for Medical and Public Health Workers* [Shanghai: National Quarantine Service, 1936], 47).

[21] *Ibid.*, 10-12.

The definite proof of the presence of plague in China had to wait until the nineteenth century. Its appearance in eastern Yunnan after the Mohammedan rebellion of 1855 was followed by its transmission to the sea coast in the next thirty years, from where it spread to various parts of the world in the 1890s. This development of the so-called "third pandemic" led to a tracing-back which established that plague was present in eastern Yunnan in the 1840s, and in western Yunnan much earlier—perhaps in the seventeenth century, and certainly in the eighteenth. Local tradition was that the disease had come from Burma.[23]

2. *India:* For the attribution of the origin to India, no such early sources exist. The first definite indications of plague in India are instructions concerning precautions against it in the Hindu *Bhagavad Purana,* perhaps as early as the eleventh century. Thereafter it appears to have broken out periodically, though there is no reliable record of epidemics in the years immediately preceding, or during, the outbreak of the Black Death in Europe in the fourteenth century.[24] Early in the nineteenth century a Russian medical historian, Wilhelm Richter,[25] stated that the plague had come into Russia from India. More authoritatively, the great apostle of geographical epidemiology, August Hirsch, on the basis of a comment by Fracastorio, and because he regarded pneumonic plague (which was present in the Black Death's incursion into Europe) as a particular modification of the "Indian strain," located the point of origin on the upper Ganges, and regarded the Chinese outbreak as secondary.[26] Georg Sticker added further, erroneous, support to this by mistakenly attributing to the Andalusian traveller, Ibn-Battutah, the statement that he had witnessed the plague in India in 1332 and

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[22] K. Chi-min Wong and Wu Lien-teh, *History of Chinese Medicine* (Shanghai: National Quarantine Service, 1936), 506.

[23] Wu Lien-teh, "The Original Home of Plague," *Manchurian Plague Prevention Service Reports,* 1923-24, IV, 186-9; Wu et al., *Plague,* 12-16; Wong and Wu, *op cit* (n 22 above), 507-9.

[24] Simpson, *op. cit.* (n. 2 above), 40; R. Nathan, *The Plague in India* (Bombay, 1897), 42; Dols, "The Black Death in the Middle East," 36-7

[25] Wilhelm Richter, *Geschichte der Medicin in Russland* (Moscow, 1813), III, 215

[26] August Hirsch, *Handbuch der historisch geographischen Pathologie* (Erlangen, 1861) Translation *Handbook of Geographical and Historical Pathology,* trans by Charles Creighton (2nd ed., London, 1883), I, 540-4.

1344 and contracted it in the latter year.[27] Other, more recent, historians have supported the Indian attribution, notably Dr. J.F.D. Shrewsbury as recently as 1970.[28] They have usually done so from the conviction that the districts of Kumaon and Garhwal in India are ancient and "ultimate" plague foci.

3. *Central Asia:* The historical evidence for the Central Asian attribution is even sparser. There is a record of a million-and-a-half people dying of a disease in Samarkhand and Bokhara—perhaps in 1056, perhaps as late as 1347 (the dating is vague)[29]—to which later interpretation has attached the name of plague. There does not appear to be one first-hand description of plague or its symptoms, either in the meagre records of the Mongol hordes or in the accounts of the Western and Persian merchants and travellers who, according to modern historians, thronged the caravan routes of Central Asia during the *pax Tartarica*.[30]

Three Andalusian Arabic commentators, writing more or less simultaneously (c. 1349), place the origin in Central Asia. Ibn-al-Wardi recorded that he had learned from Muslim merchants returning from the Crimea to Syria that the epidemic had broken out in the land of the Uzbeks—that is, in the land of the people of Uzbeg, the recent reigning khan of the Golden Horde—in October-November 1346, causing great devastation, and spread from thence to the Crimea and Byzantium. Elsewhere he attributed its origin to the "land of darkness" (usually interpreted to mean the land of the Kipchak Khanate of the Golden Horde) about fifteen years previously, from whence it had spread to the land of the Uzbeks and thence to China, India, Transoxiana, Persia and the "land of the Khitai." Ibn Khatib and Ibn Khatimah placed the origin in the "land of the Khitai," and the latter claimed to have gathered his information from Christian merchants, but otherwise the accounts are substantially the same.[31]

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[27] Georg Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre*, I Band: Die Pest. Erster Teil: Die Geschichte der Pest (Giessen: A Toepelmann, 1908), 41-2 Sticker made a quadruple error. Ibn Battutāh did not describe the disease as plague, which disease he witnessed in Damascus in 1348, he did not come to India until 1333; he fell ill of a fever which he does not describe as plague, but Sticker does; and he found the disease in Mutra which is Madura in South India, not Muttra in North India, as Sticker supposed. (Ibn Battutāh, *Travels in Asia and Africa 1325-1354*, trans and selected by H. A. R Gibb [London, 1929; New York: A. M Kelley, 1969], 64-5; Charles Creighton, *A History of Epidemics in Great Britain* [Cambridge, 1894, New York: Barnes & Noble, 1965], I, 146-7).

[28] J. F D Shrewsbury, *History of Bubonic Plague in the British Isles* (Cambridge. University Press, 1970), 37

[29] Simpson, *op. cit.* (n. 2 above), 19-20; Dols, "The Black Death in the Middle East," 32.

[30] See esp Sir Henry Yule, ed., *Cathay and the Way Thither* (2nd ed., London: Hakluyt Society, 1914), I, Introduction; and Eileen Power, "The Opening of Land Routes to Cathay," in A P. Newton, ed , *Travel and Travellers in the Middle Ages* (London: K. Paul, Trench & Trubner, 1930), 125-6, 143-4.

The "land of the Khitai" has often been taken to mean China—a belief permanently embodied in the Russian language. In fact, between the eleventh and the fourteenth centuries it referred, specifically, to the area dominated by the Kara Khitai, that ephemeral empire of Turks and Uighirs led by refugees from the ruins of the Chi-tan dynasty in North China, whose brief victories over the Kwarismi Turks in the twelfth century startled the entire Moslem world and inspired among the hopeful Christians of the West the legend of Prester John. *Khitai* was the Persian transliteration of Chi'tan. When the Mongols irrupted into Western Asia a century later they were at first taken by the Moslems to be a further wave of the same group, and the name was applied to them generally. Thus all three authors were referring, vaguely, to the Mongol lands. The Persian and Arabic names for the Chinese were *Sin* or *T'in* from the third-century *Ts'in (Chin)*, brought from China by Persian merchants.[32]

Medieval Europe's identification of Central Asia as the source of plague was strongly motivated by the population movements from the East which had been threatening the frontiers of Europe since the days of Imperial Rome; and particularly by the relatively recent and destructive attacks of the Turks and Mongols. These established an ethnic antipathy in the context of which almost any unfavourable manifestation could be attributed to the region of Central Asia. In the nineteenth century this identification was to be reinforced, paradoxically, by a renewed interest in, and exploration of, Central Asia by Europeans which impressed on them the area's primitiveness, remoteness and, above all, mystery. For some, including Wilhelm Haeser and, later, Wu Lien-teh, it appeared that the lands of Central Asia were the original home of man, of rats and of plague.[33] In 1879 the almost simultaneous news of plague outbreaks in the widely-separated localities of Yunnan in Southwestern China, Kumaon in India, and Vetlyanka on the lower Volga, convinced August Hirsch that there were ancient and contemporary plague foci in the interior of Central Asia from which all these outbreaks had sprung (Fig. 2). "These centers," wrote Hirsch, " belong not

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[31] Dols, "The Black Death in the Middle East," 31-4

[32] 'Alā ud-Dīn 'Atā-Malik Juvainī, *Tā-rikh-i-jahān gushā* [History of the World Conqueror], trans J A Boyle (Manchester: University Press, 1958), Part 2, Chapter 10

[33] W Haeser, *Geschichte der Medicin*, Band III (Jena, 1882), 139; Wu, "The Original Home of Plague," 181, 198

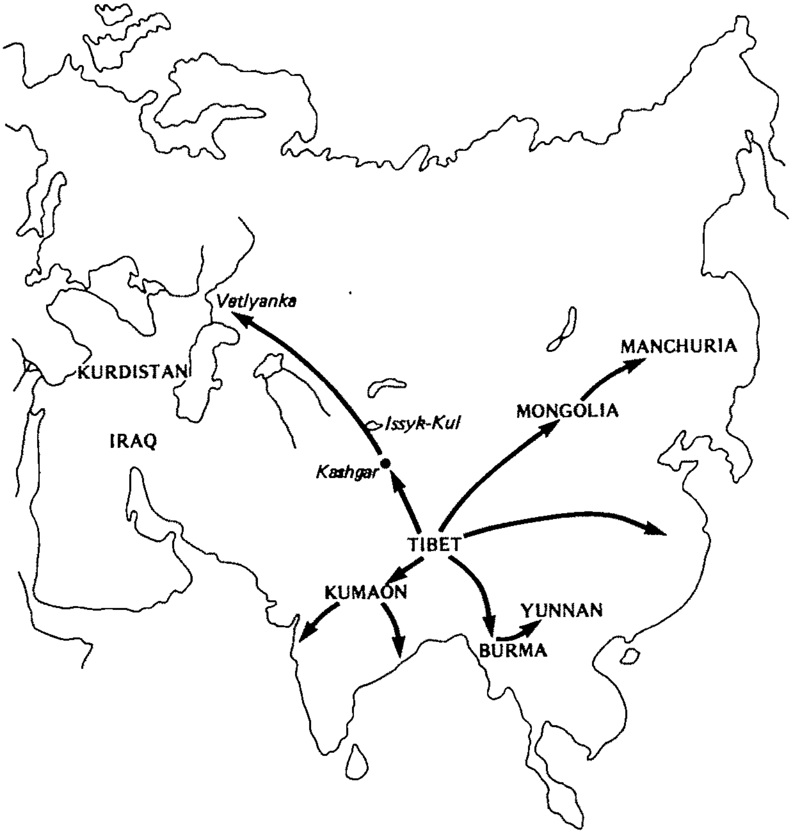

Fig. 2. "The Ultimate Reservoir," Hirsch, Haeser and Wu Lien-Teh

only to the past but remain unimpaired down to the present day,"[34] Adrien Proust wrote of the whole highland area from the Caspian to the Himalayas as having "never been free of plague outbreaks."[35] The identification of sylvatic plague among the wild rodent populations of southern European Russia and Turkestan at the beginning of the twentieth century also gave powerful support to this interpretation. Finally Wu Lien-teh, in the 1920s and 1930s, elaborated on Hirsch's theme: that plague perpetuates itself among wild-rodent reservoirs in socially-backward and sparsely-inhabited countries; and that the frequency of attacks stemming from Kurdistan, Russian Turkestan, Chinese Turkestan, Inner Mongolia, Kumaon, Garhwal and

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[34] Hirsch, *op cit.* (n 26 above), I, 533-34.

[35] Adrien Proust, *La défense de l'Europe contre la peste* (Paris, 1897), 96.

Yunnan argues for a common source which he located in Tibet. Plague spread from this "ultimate reservoir" along the caravan routes of Central Asia to the outside world.[36] This enthusiasm for the "ultimate reservoir" moved Charles Creighton to write:

> According to the dominant school of epidemiologists it is always enough to have traced a virus to a remote source, to the "roof of the world" or to the back of the east wind, and there to leave it, in the full assurance that there must have been circumstances to account for its engendering there, perhaps in an equally remote past, if only we knew them.[37]

One piece of archaeological evidence must also be included in the discussion of the attribution to Central Asia. In 1885 the remains of two large Nestorian cemeteries were discovered a few miles to the west of Issyk-kul on the northern borders of the modern Kirghiz Republic (Fig. 3). On about 330 headstones were recorded the names of more than 650 deceased, covering the years from 1186 to 1349. At least 106 of these names refer to deaths occurring in the years 1338 and 1339. On three headstones were recorded the names of *ten* people whose deaths in these two years were attributed to "pestilence" (in the original Syriac). This was translated as "plague" by the archaeologist Daniel A. Chwolsen. First Chwolsen, then the missionary-historian John Stewart, and still later the distinguished authority on plague, Robert Pollitzer, stated that the high mortality and the ten recorded "pestilence" deaths established the presence of plague in the region in 1338-39. Pollitzer went further: "It is certain, therefore, that plague was conspicuous in Central Asia for a few years before the Crimean ports became infected (1346) and the disease was carried thence by ship to Europe."[38] Subsequent historians have repeated these statements, though the evidence for a plague epidemic is by no means conclusive, and certainly does not support the identification of more than a sporadic outbreak.

Dols, who has made a thorough study of the Islamic literature on the subject, writes:

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[36] Wu, "The Original Home of Plague," 178-98; Wu Lien-teh, *A Treatise on Pneumonic Plague* (Geneva. League of Nationals Health Organization, 1926), 2; Wu *et al*, *Plague*, 6-17.

[37] Creighton, *op. cit.* (n. 27 above), I, 149.

[38] Daniel A. Chwolsen, "Syrisch-nestorianische Grabinschriften aus Semirjetschie . . . ," *Mémoires de l'Académie Impériale des Sciences de St. Petersbourg*, vii^{e} série, tome 37-8 (St. Petersburg, 1886), 81, 84, 85, 89; Chwolsen, "Syrisch-nestorianische Grabinschriften aus Semirjetschie, Neu Folge . . . ," *Ibid* (St Petersburg, 1897), 31-2, 34, 36, 28; J. Stewart, *The Nestorian Missionary Enterprise* (Edinburgh. T & T. Clark, 1928), 209; Pollitzer, *Plague*, 14; cf. Karl F. Meyer, "Plague," *Encyclopaedia Britannica* (1959 ed), 17, 114; Ziegler, *op. cit.* (n. 19 above), 26.

The only precise mention of plague in Central Asia known to me, which would suggest an endemic [sic] focus, is found in a 10/16th century plague treatise which refers to the magical practices in Kashgar (Shufu) during the plague epidemics.[39]

4. *Southern European Russia:* Finally, we must examine the evidence for the plague's appearance in South Russia, which is usually regarded as the proximate origin for the European outbreak of the Black Death. Two contemporary Greek historians identified South Russia as the source of the European Black Death. Nicephoros Gregoras wrote that the plague came "from Scythia and Maeotis and the mouth of the Tanais [Don]" in the spring of 1346. John Cantacuzenos (the Byzantine Emperor John VI) wrote that it began among "the Hyperborean Scythians."[40] More dramatically, Gabriel de Mussis, a Piacenzan who was not himself in the Crimea, gave the famous description of the outbreak in 1346 among the "tribes of Tartars and Saracens," which devastated the population of whole tracts of country at about the time when the Venetians were being driven from Tana and the Genoese were being besieged in Caffa by the Kipchak Khan, Jani Beg. According to de Mussis the disease attacked the Tartar army and was spread by them to the town of Caffa by means of catapulting infected corpses over the walls, from where they were thrown into the sea by the Genoese. "But soon the whole air became infected, and the water poisoned, and such a pestilence grew up that scarcely one out of a thousand was able to escape." From Caffa, according to de Mussis, the plague was carried by the fleeing Genoese ships to Genoa, Venice and the rest of Europe.[41]

Two leading modern historians of plague have suggested that the Black Death originated where it was first reported: in the lower valleys of the Volga and Don and in the Crimea. Charles Creighton wrote that the disease first manifested itself in this area, but he believed that its seeds had been sown in China, and that the disease returned to China.[42] Sir. W.J.R. Simpson, writing in a vein more in harmony with modern pathology, suggested that the epidemic may have had its origin in the established foci of Kurdistan and Iraq, and that "in its general extension, north, east, south and west, it reached the Volga by routes similar to those which it has taken in modern times." Commerce and the march of armies, he wrote, had carried it

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[39] Dols, "The Black Death in the Middle East," 34

[40] N. Gregoras, *Historiae Byzantinae*, XVI, J. Cantacuzenos, *Historiarum*, Vol III (Bk IV)—quoted in Christos B. Bartsocas, "Two fourteenth-century descriptions of the 'Black Death,'" *J Hist. Med*, 1966, *21*. 395-6

[41] De Mussis, *op. cit.* (n. 10 above), quoted in Simpson, *op cit* (n 2 above), 137-8

[42] Creighton, *op. cit* (n 27 above), 143, 147-8.

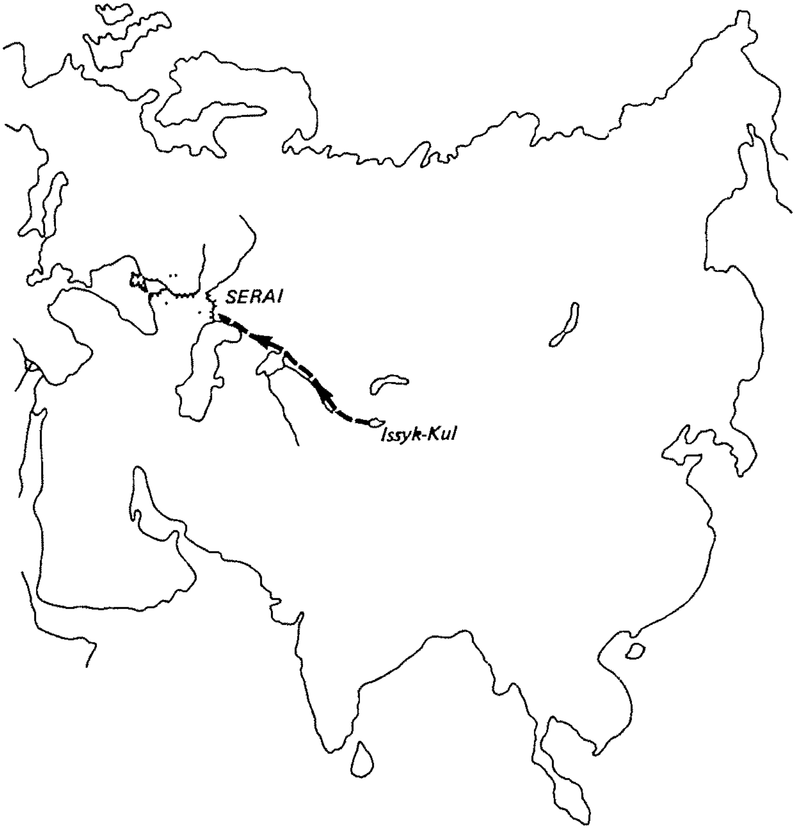

Fig. 3. The Possible Route of Plague from Issyk-kul to the Crimea

northward from Persia.[43] Contemporary local records that the Caucasus and the territory on both shores of the Caspian Sea were devastated by plague at the time of the Black Death tend to support this view.[44]

In summary, what does this historical record tell us that is reasonably certain? There was an undoubted outbreak in South Russia in 1346-7 from which, so far as can be determined, the disease spread to the Mediterranean, Europe and the Middle East. The plague was present in Iraq and Kurdistan in the eleventh century, and it was probably present in India in the eleventh century and later, though apparently not in the years immediately preceding the appearance of the Black Death in Europe. Plague may have existed in coastal China in the seventh century A.D., but the first epidemiological description dates only from the seventeenth century. There are no reliable first-hand reports of plague among the Mongols of the steppe immediately prior to the outbreak in the Crimea, though there may have been a sporadic, small-scale outbreak near Issyk-kul, characteristic of direct transmission from wild rodents to small, dispersed, human populations. It occurred about eight years before the outbreak of the Black Death, and it is doubtful if it was connected with that outbreak for reasons which will appear later in this paper. The historical record does not give much support to the belief that the disease was transmitted a great distance across Asia to cause the European Black Death in the fourteenth century.

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[43] Simpson, *op. cit.* (n. 2 above), 23.

[44] Cf. Josef, Freiherr von Hammer-Purgstall, *Geschichte der Goldenen Horde im Kiptschak* (Pest, 1840), 308; Howarth, *op. cit.* (n. 15 above), II, 175.

But this is not the only historical objection to long-range transmission. The political and religious circumstances among the successor states of the Mongol Empire in the second quarter of the fourteenth century (Fig. 4) make it unlikely that large numbers of traders were then travelling along the caravan routes of Asia and available to provide transmission for the plague. Mongol khans, in the heyday of Mongol power, had encouraged Persian and Christian (though not Chinese) merchants; but from the end of the thirteenth century the situation changed with the conversion of the Western- and Central-Asian successor states to Islam. In 1295 the Il-khans of Persia became Moslem, and with the change they became an Iranian nationalist dynasty. This sharpened their chronic conflict with the Kipchak Khanate of the Golden Horde in southern Russia which, however, in 1313 itself became Moslem. Finally, in 1326 the Chagatai Khanate of Turkestan turned to Islam. While these religious changes did not bring harmony among the khanates, they did cause breaks between them and both the Mongol empire in China and the Christians in the West. The result was that by about 1325 communication along the Central Asian tracks through the khanates was very difficult for Western, or even Persian, traders, and after about 1340 virtually impossible. Finally, in 1343 the Kipchak Khan, Jani Beg, moved to suppress entirely the Italian factories at Tana, Caffa and Serai, and European trade came to an end. It was during this last suppression of the factories that the Black Death is generally supposed to have appeared in Southern Russia.[45]

Political circumstances in the khanates also argue against the mass spread

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[45] Bertold Spuler, *History of the Mongols* (Berkeley· University of California Press, 1972), *passim* , esp 210-11; W. Heyd, *Histoire du commerce du Levant au moyen-âge* (Leipzig, Harrassowitz, 1936), II, 156-210, Power, *op cit* (n 30 above), 143-7, J J Saunders, *The History of the Mongol Conquests* (London. Routlege & K. Paul, 1971), 149, 164.

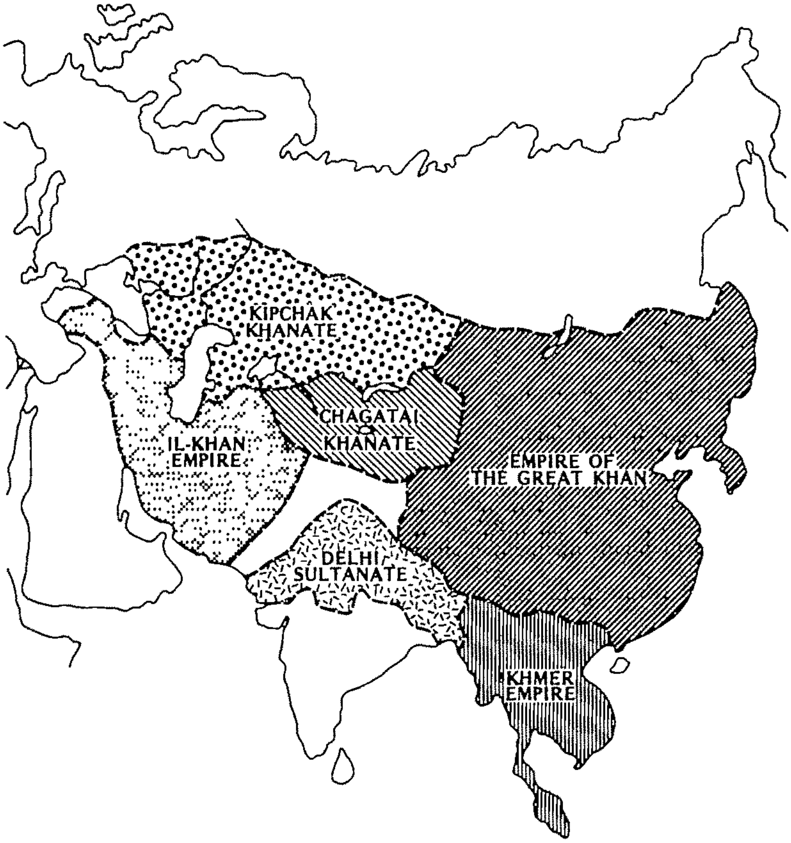

Fig 4. Mongol Khanates in the 14th Century

of the Black Death by the movement of armies, at least from east to west. De Mussis and, in the twentieth century, Wu and Simpson,[46] noted that the Black Death broke out among the Tartar army besieging Caffa and concluded that that army brought it from the Orient. But, in fact, in the fifteen years before the outbreak, there was no warfare and very little communication between the Khanate of the Golden Horde and any of the states to the east of it. Moreover, the internal state of the Khanate, generally prosperous, peaceful and culturally fruitful, was steadily reducing the nomadic element in the society and increasing the agricultural element, as well as increasing the population as a whole.[47] Indeed, conditions within the territory of the

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[46] Simpson, *op. cit.* (n 2 above), 21, 139; Wu, "The original home of plague," 196.

[47] Saunders, *op cit.* (n. 45 above), 160-61 I also wish to acknowledge gratefully the assistance of Dr E. G

Khanate were favourable for producing just that density of human and commensal rodent populations essential for supporting plague epidemics and epizootics. On another frontier, there was a chronic state of war between the Golden Horde and Persia during these years. The Il-khan dynasty was collapsing, the Persian state was in chaos, and various attempts were made by armies from the Golden Horde to conquer Tabriz and the rest of Azerbaijan. When one of the contenders for supremacy, Malik Ashraf, ravaged Tabriz in 1346-7, plague was present, and there is evidence that it was also present at Baghdad which he besieged in 1347. Ten years later Jani Beg captured Tabriz, but left the city almost immediately and died on his return to Serai. Dols interpreted both these cases as transmission by the armies from one center to another, and outbreaks in Mosul and Baghdad in 1349 as due to transmission by armies from the north to Azerbaijan and Kurdistan.[48] On the other hand, it is at least equally likely that the outbreaks in Kurdistan, Iraq and Azerbaijan, *and* in the Kipchak Khanate, originated in local wild-rodent foci.[49]

The most important technical objection to long-range transmission is the apparent difficulty of accounting for that transmission across vast, thinly-populated expanses of territory within, at most, a few years time, without there appearing any evidence *en route* of a human outbreak. The basis of this objection is in the dynamics of the various host populations which govern the transmission of the micro-organism from wild-rodent foci—where the disease is perpetuated in enzootic form—to areas of relatively dense human population—where the disease can become epidemic. By the 1950s studies of wild-rodent foci in Russia, the United States, Kurdistan and Southeast Asia had demonstrated the relative immobility of plague in its enzootic state, but also its slow movement in that state across continental land masses.[50] There

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Pulleyblank, Professor of Asian Studies, University of British Columbia, in determining this point

[48] Dols, "The Black Death in the Middle East," 38-9; cf. J D. Tholozan, *Histoire de la peste bubonique en Mesopotamie* . . (Paris, 1874), 2, 87: Simpson, *op. cit.* (n. 2 above), 23.

[49] Dr. J F. D Shrewsbury has suggested that transmission by armies is highly unlikely on the following grounds: (1) Armies do not go into areas where plague is known to be. (2) Though individual soldiers may occasionally carry in their clothing infected fleas from a plague focus, the stationary state of sieges is necessary for generation of elpizootics among commensal rodents as a prelude to human epidemics (3) Early modern armies did not have enough wheeled transport in which infected rodents could hide (Shrewsbury, *op. cit.* [n. 28 above], 142). Of these, only the second seems to me to be cogent.

[50] K. F Meyer, "The ecology of plague," *Medicine*, 1942, *21:* 143-74; Fyodorov, *op cit* (n. 1 above), 135-41; M. Baltazard, M. Bahmanyar, Ch. Mofidi and B. Seydian, "Le foyer de peste du Kurdistan," *Bull. World Health Organization*, 1952, *5.* 441-72; B. K. Fenyuk, "Experience in the eradication of enzootic plague in the north-west part of the Caspian region of the U.S.S.R.," *ibid.*, 1960, *23:* 263-73.

are two significant implications of this discovery: first, that the rapid "movement" of the micro-organism across large distances on land is impossible without human transmission of infected zootic hosts and the consequent likelihood of human outbreaks en route; and second, that plague may also be spread slowly by transmission of the micro-organism from one enzootic wild-rodent focus to another.[51]

In fact the micro-organism is transmitted through two channels: (1) Along the maritime trade routes by shipboard transportation of infected fleas on humans, rodents and merchandise. This is relatively rapid because it depends on the agency of humans who move much more rapidly and extensively than do other hosts. It has been the means by which each of the great pandemics has reached Europe. (2) Across land by means of transmission of infected fleas between wild-rodent colonies and, ultimately, into a center of human population, either directly from the wild-rodent foci to the commensal-rodent and human populations, or through lines of human communication after commensal rodents and/or humans have invaded the wild-rodent biotrope. This is the method by which the micro-organism has been spread eastward across the United States and from the Caspian region eastward across Russian Turkestan. It is relatively slow because it depends on the movement of fleas and wild rodents, neither of whose biotropes is extensive or fast-extending. Only when man invades these biotropes and provides the means of rapid movement does the pace of transmission quicken. Transmission across land cannot be simultaneously stealthy and rapid, because to be rapid it requires human agency, with all the risk of substantial human outbreak which that implies.[52]

These are the most *probable* means of transmission, and in discussing epidemics we must deal in probabilities. But we must also take account of three other *possible* means: interhuman transmission of bubonic plague by ectoparasites of humans, especially *Pulex irritans*; interhuman transmission of plague in pneumonic form; and transmission of bubonic plague by other animal hosts, notably camels. Interhuman transmission by ectoparasites of humans is possible, as the confined, sporadic outbreaks in Kurdistan in recent years may have demonstrated,[53] but because the eclectic flea, *Pulex irritans*, having harboured the micro-organism for thousands of years, is remarkably resistant to it, this particular insect is a relatively poor transmitter.[54] Hence there is, as yet, no good scientific evidence of an *epidemic* being sustained on a large scale by this form of transmission. This does not rule out transmission on a small scale, of the sort which produces sporadic outbreaks, as in Kurdistan. Though the first outbreak of the Black Death was characterized by a considerable incidence of pneumonic plague and Guy de Chauliac, for one, wrote that the pneumonic cases tended to *precede* the bubonic ones,[55] there is no modern scientific evidence to suggest that the differentiation characterizing pneumonic plague is determinant of, rather than dependent upon, human bubonic plague.[56] Finally, modern investigation suggests that camels have a low susceptibility to plague and that in the past plague in camels has played a very limited part in the occurrence of outbreaks of plague in human beings. Camels are frequently infected with plague during an epizootic among rodents, but in most cases they do not die of the disease, since it follows a mild course ending in recovery. The proportion of highly susceptible animals in a herd or caravan which die of plague is insignificant. People are infected only if they slaughter a camel suffering from plague, skin it and cut up the meat.[57] In such cases, the outbreak takes the form of sporadic cases rather than an epidemic.

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[51] Fyodorov, *op cit* (n. 1 above), 135.

[52] Meyer, "Ecology of plague," *op cit* (n. 50 above), 164, 169-71, M Baltazard, "Déclin et destin d'une maladie infectieuse: La peste," *Bull. World Health Organization*, 1961, *23*· 247-62.

[53] Baltazard *et al* "Le foyer de peste du Kurdistan" *ibid* , 1952, *5* 441-72.

There seems little doubt that the outbreak of the Black Death in the Mediterranean and European lands in the years 1346-52 was due to transmission by ships, along the lines of maritime trade, of fleas infected with a strain of the micro-organism to which the host populations in these regions were especially susceptible. But how did the micro-organism come to be in Southern Russia in the fourteenth century?

Probably plague had been enzootic in the wild-rodent foci of South Russia for some time before the outbreak of the Black Death, to become epidemic by the crossing of the biotropes of the wild rodents and a relatively dense concentration of commensal rodents and humans at a time when the disease had become epizootic in the rodent population. Apparently it was enzootic in Iraq and Kurdistan as late as the eleventh century. Human outbreaks there and in Azerbaijan in 1347 could have been carried by invading armies, but more probably these were the result of the same epizootic outbreak as occurred at the same time in the Kipchak Khanate. There was ample time for the slow enzootic transmission of the micro-organism through the wild rodent populations on either side of the Caspian to the northwestern shores of that sea and thence to the Don Valley and the Crimea. Here a necessary condition of a plague epidemic—a sufficient density of human (and commensal-rodent) population—had been secured by the development and prosperity of the Kipchak Khanate. The nearly contemporary outbreaks south of the Caucasus, at Tabriz and Baghdad, also occurred in centers of population density.

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[54] Joseph Hirsh, "Twin tales: A new look at the flea, and looking at it from the viewpoint of the louse," *Military Med.*, June 1960, *125*. 412; Albert L Burroughs, "Sylvatic plague studies. The vector efficiency of nine species of fleas compared with *Xenopsylla Cheopis*," *J. Hygiene*, August 1947, *45*· 390-3; H E Ewing and Irving Fox, *The Fleas of North America* . (Washington: Government Printing Off , 1943, Harold Stark, *The Siphonaptera of Utah* (Atlanta U S. Public Health Service, 1958), 218-19; Hirst, *op. cit.* (n 2 above), 236-9; C. J Martin, "The transmission of plague by fleas," Lecture II of the Horace Dobell Lectures on Insect Porters of Bacterial Infections. Delivered before the Royal College of Physicians. *Brit. Med. J* , Jan. 11, 1913, *I* 63-4.

[55] Guy de Chauliac, *La grande chirurgie*, composée en l'an 1363. (Ed. E Nicaise (Paris, 1890), 167-70

[56] Wu, *Treatise on Pneumonic Plague*, 3-4, 6-8, 162-4, 165-78; Pollitzer, *Plague*, 505-8, 510-13, 515-16, Hirst, *op. cit* (n 2 above), 34.

[57] V N Fyodorov, "Plague in camels and its prevention in the U.S.S R.," *Bull World Health Organization*, 1960, *23*. 275-81

The infection of such centers and, indeed, of all the centers of population in Europe for the next three centuries, was made relatively easy by the fact that, unlike modern cities, they were not yet widely separated by large stretches of cleared arable pasture and developed urban sites from the uncleared, unbroken land which harboured wild rodents. Early modern towns were "farm towns" such as exist in Southeast Asia today; the countryside came close to their gates and to the edge of the limited arable fields and pastures of villages. This countryside harboured ample wild rodents to maintain plague in its enzootic form, and the wild rodents could readily exchange infected fleas with commensal rodents and humans during an epizootic. The early modern population, moreover, would more readily invade the wild rodent biotrope than would, for example, the population in the twentieth-century Western United States, where the established wild-rodent foci of plague are at present spatially and socially remote from the centers of population. In Europe such centers became isolated from the countryside only with the large-scale clearing of land and destruction of woodland fauna accompanying, or slightly preceding, the onset of industrialization.[58]

Wherever it occurred, the epidemic outbreak of the Black Death, like all epidemics of bubonic plague, required the achievement of a composite epidemiological threshold determined by: (a) an optimal *density* of susceptibles in the flea, wild rodent, commensal rodent and human populations; and (b) an optimal infectivity of the micro-organism, as determined by: (i) abundance of the micro-organism, (ii) capacity of fleas to be infected by the micro-organism, (iii) capacity of fleas to infect zootic and human hosts, and (iv) susceptibility of zootic and human hosts.[59] Given these circumstances, it required only the outbreak of an epizootic among the wild rodents and transmission to the commensal rodents, linking with a relatively dense human population, for an epidemic to occur. The siege of Caffa provided an intensification of this crowding of human population both among the besieged and the besiegers.

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[58] Fyodorov, "The question of the existence of natural foci of plague in Europe in the past," 135-41. Cf Folke Henschen, *The History and Geography of Diseases* (1962, New York: Delacorte, 1966), 23-4.

Two further modern pieces of evidence lend support to this explanation. The first is embodied in a map of the Soviet Union's modern plague foci (Fig. 5). In the absence of large-scale human invasion of wild-rodent biotropes and destruction of wild-rodent colonies, the foci remain largely unchanged for long periods of time—merely expanding slowly. Thus, though the focus north of the Caspian Sea, which has been subjected to campaigns of mass extermination of wild rodents, has largely disappeared in the last forty years,[60] those east of the Caspian and extending across Turkestan and into Mongolia have grown somewhat since they were first charted in Tsarist times, but otherwise have remained largely unchanged.[61] The important feature to note is that the foci are highly concentrated in the Caspian Basin, where the largest concentration of rodents is, and that they thin out the further east they extend, as though the micro-organism had been transmitted slowly eastward from an established focus in the Caspian area.[62]

The second piece of modern evidence provides support for the proposition that the micro-organism was transmitted *northward*, probably from the Iraq-Kurdistan area. In 1951 R. Devignat, on the basis of studies of plague strains from Central Africa, Southeastern European Russia, Central Asia and East Asia (Mongolia and Manchuria) defined three regional varieties of the micro-organism, *Yersinia pestis*, according to their capacity to ferment glycerine and to reduce nitrates to nitrites, thereby producing nitrous acid. These three were:

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[59] The development of a mathematical threshold theorem for plague is a principal feature of my current work on the retrogression of the plague from Western Europe in the seventeenth century. Existing literature on the threshold theorem is summarized in: Robert E. Serfling, "Historical review of epidemic theory," *Human Biology*, Sept. 1952, *24:* 145-66; N. T. J. Bailey, *The Mathematical Theory of Infectious Diseases and Its Applications* (London: Charles Griffin; New York: Hafner, 1975. First published in 1957 under the title *The Mathematical Theory of Epidemics*); and Klaus Dietz, "Epidemics and rumours: A survey," *J. Roy. Statistical Soc.*, Series A, 1967, *130:* 505-28.

[60] Fenyuk, *op. cit.* (n. 50 above), 263-73.

[61] R. Pollitzer, *Plague and Plague Control in the Soviet Union: History and Bibliography through 1964* (New York: Institute of Contemporary Russian Studies, Fordham University, 1966), xiv, 1-56 *passim*.

[62] *Ibid.*, xiv.

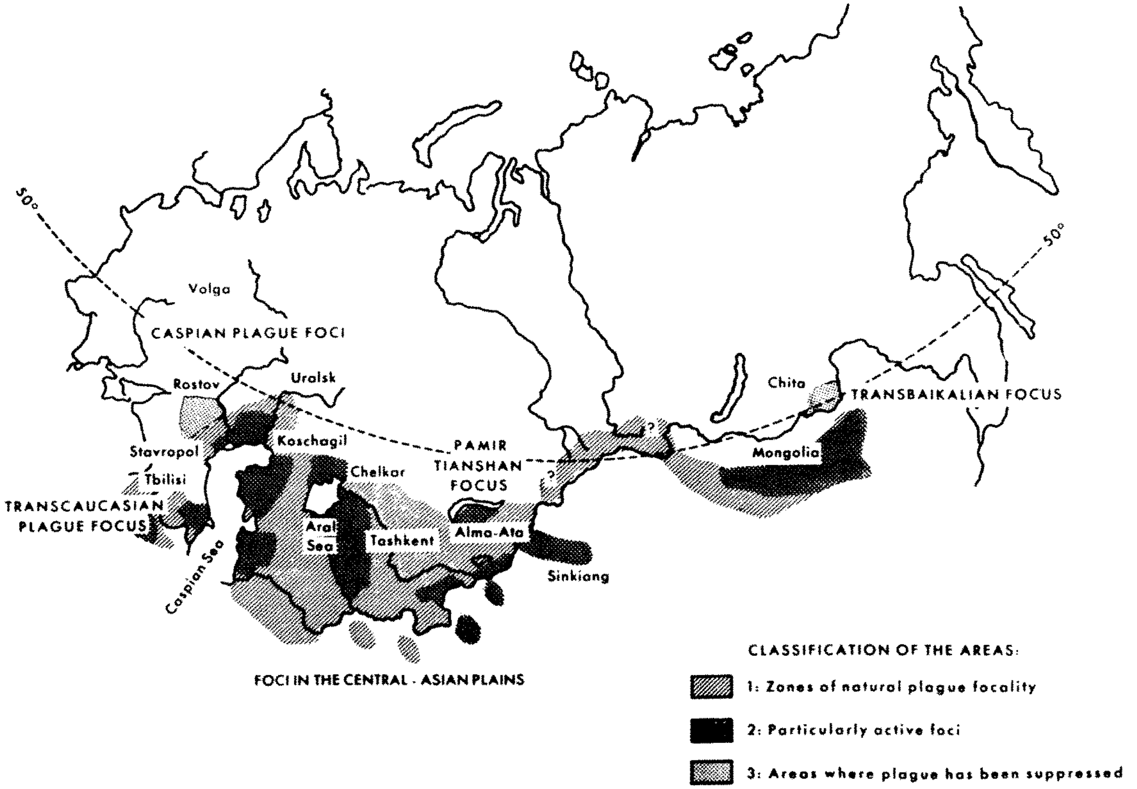
50°
Volga
CASPIAN PLAGUE FOCI
Rostov
Uralsk
Stavropol
Tbilisi
TRANSCAUCASIAN PLAGUE FOCUS
Caspian Sea
Koschagil
Chelkar
Aral Sea
Tashkent
PAMIR TIANSHAN FOCUS
Alma-Ata
Sinkiang
?
?
Chita
TRANSBAIKALIAN FOCUS
Mongolia
50°
FOCI IN THE CENTRAL - ASIAN PLAINS
CLASSIFICATION OF THE AREAS:
▨ 1: Zones of natural plague focality
■ 2: Particularly active foci
▒ 3: Areas where plague has been suppressed

Fig. 5. Sketch Map of the Natural Plague Foci in the Soviet Union and Adjacent Countries (R. Pollitzer, *Plague and Plague Control in the Soviet Union: History and Bibliography through 1964* (New York: Institute of Contemporary Russian Studies, Fordham University, 1966), xiv; after Yu. M. Rall ["Geography and some peculiarities of the national foci of rodent plague"], *Zh. Mikrob.* [1958], *29*: 76, 77).

(1) A variety to be found in the twentieth century in "Central Asia and Central Africa," which could ferment glycerine and produce ntirous acid.

(2) A variety which is supposed to have been a transformation of the first, found in Southeastern European Russia. This variety fermented glycerine but did not produce nitrous acid.

(3) A third variety, found in India, Burma and South China, is the strain which spread around the world after 1894 to such places as Australia, California, South Africa and Hawaii and caused the current serious outbreak in Vietnam. It did not ferment glycerine, but did produce nitrous acid.

Devignat associated each of the varieties with one of the great historic pandemics. He regarded the second variety as the result of the slow transformation of the first. This second variety established itself in Southeastern Russia by the fourteenth century then spread outwards to the whole of Europe as the Black Death. He called it *pestis medievalis*.[63] It was found in Turkey and Kurdistan as well as Russia, it had no chemical action on protein (unlike the other two varieties), it had a greater ability to proliferate and a greater vitality than the other two, and penicillin had no antibiotic effect on it. It also appeared to have greater pulmonary tropism and a greater tendency to produce septicaemia with pulmonary complications than did the other two.[64]

While there are many more sophisticated methods of classifying plague developed since 1951,[65] the glycerine-fermentation-and-production-of-nitrous-acid classification remains useful for identifying regional characteristics. For the purposes of this paper, however, we need only note the significant fact that the variety of plague found in Kurdistan, Turkey and Southeast European Russia has the capacity to ferment glycerine, whereas that found in India, Burma and South China does not.[66]

The significance of this capacity to ferment glycerine is explained by the

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[63] Devignat, "Varietés de l'espèce Pasteurella pestis: Nouvelle hypothèse," *Bull. World Health Organization*, 1951, *4:* 247-63.

[64] Devignat, "Comportement biologique et biochimique de *Pasteurella pestis* et de *Pasteurella pseudotuberculosis*, *ibid.*, 1954, *10:* 463-94. Cf. M. Baltazard *et al.*, "Recommended laboratory methods for the diagnosis of plague," *ibid.*, 1956, *14:* 457-509, esp. 466, 470-1.

[65] Bruce W. Hudson and Martin I. Goldenberg, "Isolation of *Yersinia pestis* of unusual protein content obtained from central Java," *ibid.*, 1970, *43:* 917-19; M. J. Surgalla, E. D. Beesley, and J. M. Albizo, "Practical applications of new laboratory methods for plague investigations," *ibid.*, 1970, *42:* 993-97; I. L. Martinevsky, "Materials for typing natural foci of plague according to the genetic properties of plague bacillus strains," *J. Hyg., Epidem., Microbiol. & Immunol.* [Praha], 1973, *17:* 272-78.

[66] This conclusion contradicts that come to by Wu Lien-teh in the 1920s that there was no difference between Western and Oriental plague ("The original home of plague," *op. cit.*, 184).

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Russian plague researchers, N. I. Kalabukhov, Yu. M. Rall, V. I. Federov, V. M. Tunanskii and I. Kh. Sultanaev in a series of papers published between 1957 and 1965.[67] It arises from the polyhostal nature of wild-rodent foci which, in turn, arises from the variety of ecological circumstances in which the micro-organism interacts with the various hosts, and the consequent unequal susceptibility of different species of wild rodents to the micro-organism. On the initial establishment of the micro-organism in a regional biotrope most species are about equally susceptible, and the micro-organism establishes itself widely, usually at the ecological boundaries where the numbers of animal species which could be carriers of infection are greatest.[68] This is the case in the natural plague foci in the Caspian region, the plains of Western Transcaucasia and Mongolia. But gradually a differential susceptibility asserts itself within the main focus which helps to maintain the focus through the interaction between resistant and susceptible groups of hosts. Gradually, too, at the periphery of the focus, the micro-organism adapts itself to only one species of rodent.[69] Specialization of this sort was found in the strains of plague bacillus isolated from susliks on the right bank of the Volga in the Astrakhan region, the marmots in the mountains of Central Asia, and the voles in the mountains of the Western Caucasus. Such secondary reservoirs can become the main reservoirs in circumstances where the "main" hosts are diminished in numbers by natural changes in the composition of the biocenoses or, temporarily, by large-scale extermination by a predator.

The development in the plague micro-organism of the capacity to ferment glycerine is part of the adaptation to hibernating rodents. Hibernation among rodents developed with the lowering of temperatures and the deterioration of living conditions for mammals at the beginning of the Quaternary period. Hibernating wild rodents which are hosts of plague, such as marmots and susliks, either acquired the infection from their nonhibernating ancestors or became part of the circulation of plague later when they came into contact with non-hibernating rodents living in the semi-arid borders of the steppe.[70] In any case, hibernating rodents developed in their fat a large concentration

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[67] N. I. Kalabukhov, "The structure and natural dynamics of natural foci of plague," *J. Hyg., Epidem., Microbiol. & Immunol.* [Praha], 1965, *9:* 147-59 and bibliography.

[68] P. A. Petrishcheva, *Prir. ochag. belezn cheloveka i. kraevaya epidemilogia* (Moskva: Medgis, 1955), 36-49.

[69] Yu. M. Rall and V. I. Federov, "The physiosocial evaluation of rodents as plague carriers and the monohostal character of natural plague foci," *J. Microbiol., Epidem. & Immunol.* [London], 1961, *31:* 228-35.

[70] N. I. Kalabukhov, *Zhurnal Mikrobiologi.* 1958, *9:* 91-7.

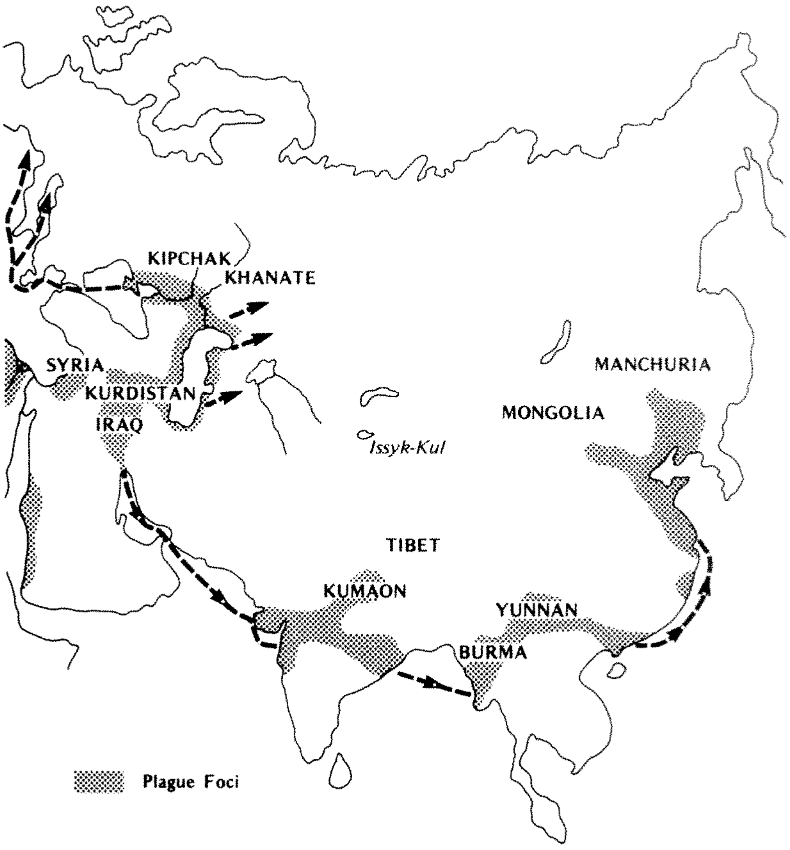

Fig. 6. Probable Route of Plague:
Middle East to Kipchak (9th to 14th centuries)
Middle East to China (6th to 17th centuries)

of glycerine, and it was to overcome this in the process of cell-necrosis that the micro-organism developed the biochemical property of fermenting glycerine. This property is absent in the plague bacilli isolated from among the non-hibernating rodents such as gerbils and rats in Southern Asia and Africa.[71] In the Iraq-Kurdistan, Caucasus and Caspian steppe regions, therefore, it is probable that plague became a disease of rodents *first in populations of non-hibernating rodents* such as gerbils, and *later among those of hibernating rodents* such as marmots and susliks.[72] This change manifested itself as the plague was transmitted from a region of temperate winters to one of cold winters—that is, generally, *as the plague was transmitted northward.*

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[71] V. M. Tumanskii, *ibid.*, 1957, *6:* 3-8.

[72] I. Kh. Sultanaev, *Zool. Zhurnal*, 1960, *39:* 118-36.

Thus the development by the plague micro-organism of the capacity to ferment glycerine, which is absent in the strain of plague current in South and Southeast Asia and China, but present in that in Southeast Russia, and which manifested itself as the plague was transmitted northward, suggests that the Black Death came to southeast Russia from a region *to the south,* rather than to the east, of that place of original human outbreak (Fig. 6).

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The argument for the proposition is now complete. Historical records are such as to cast strong doubt on the various attributions to China, India or Central Asia as the source of the Black Death in the fourteenth century. Population dynamics of the disease's various hosts also argue against long-range, rapid and covert transmission of the micro-organism. The modern plague map of the U.S.S.R. suggests that the micro-organism was transmitted *eastward* from the Caspian focus to form other, less concentrated, foci on the Central Asian steppe. Finally, the classification of plague by the capacity to ferment glycerine appears to differentiate sharply the Southern Russian strain from the Oriental one, and to establish that the former acquired its capacity to ferment glycerine as the micro-organism was transmitted northward, slowly, from one wild-rodent focus to another. This occurred probably in a period of centuries before the beginning, in the Crimea in 1346, of the terrible human disaster of the Black Death.

PHYSIOLOGY AND THE MECHANICAL PHILOSOPHY IN MID-SEVENTEENTH CENTURY ENGLAND

THEODORE M. BROWN

Introduction

In 1901 the famous Cambridge physiologist Michael Foster ventured beyond his discipline and published ten *Lectures on the History of Physiology in the Sixteenth, Seventeenth and Eighteenth Centuries.*[1] Lively and informative, Foster's *Lectures* soon became a major reference work for students of the history of physiology. Scholarly articles, general books, and collections of source materials all took Foster as an authority and guide.[2] Few seemed to notice, or if they noticed none seemed to care, that Foster wrote history as a good scientist would. He selected problems of greatest interest to his own generation, awarded credit or accorded blame to predecessors for their "correct" or "incorrect" ideas and techniques, and, generally, saw physiologists in the period he studied as searching for what he identified as "right method" and "distinctly physiological questions."[3] Thus, Foster considered Vesalius deficient for being too strictly "anatomical," while Descartes, like Herbert Spencer of his own time, failed by aiming at the creation of philosophy rather than the establishment of facts.[4] Harvey, however, approached perfection with his "strictly physiological method" that led him to test the implications of anatomical facts by "experiments on living animals."[5]

Today Foster's history would be dismissed as "Whiggish" or, at least, his approach would be very seriously questioned. He would be rightly chided for his insensitivity to proper intellectual context and his disrespect for older but historically legitimate standards of scientific explanation. It could easily be shown that Foster allowed little room for the possibility of a close interac-

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[1] Published by Cambridge University Press, 1901.

[2] A few books and articles influenced by Foster are Kenneth J. Franklin, *A Short History of Physiology* (London: John Bale, Sons and Danielson, 1933); John F. Fulton, *Selected Readings in the History of Physiology* (Springfield, Ill.: C. C Thomas, 1930); and L G., Wilson, "The transformation of ancient concepts of respiration in the seventeenth century," *Isis*, 1960, *51*: 161-172.

[3] Foster, *op cit.* (n. 1 above), pp 55-56.

[4] *Ibid* , pp. 55 and 57-58

[5] *Ibid.*, p. 55

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tion between philosophical assumptions and empirical discoveries, whereas more recent scholars have exhibited that interaction time and again.[6]

Now, three quarters of a century after Foster's *Lectures*, a reassessment of physiology during the Scientific Revolution and the eighteenth century is getting underway. Philosophical assumptions are being more seriously scrutinized, and new "facts" are being consistently understood as only pieces of more complicated patterns of thought.[7] The work of René Descartes in particular, which Foster regarded as directly contributory to philosophy rather than physiology, is beginning to receive a closer scrutiny for its scientific content and influence. Scholars as various as Thomas S. Hall, Gustav Scherz and Luigi Belloni have begun to pay serious attention to Descartes' radically new, mechanical world view and its immediate, positive influence on his own and his contemporaries' physiology.[8] Enough evidence has already accumulated to suggest that the Cartesian mechanical philosophy was a major, and perhaps even *the* major, influence on the course of physiological development in England in this period.[9] Italy quite possibly showed similar trends.[10] Few scholars would now deny that Cartesianism was among the forces shaping physiological ideas from the mid-1660s to at least the 1720s, whether, ultimately, for good or ill. Just how influential and constructive it was—compared, say, to the improvement of experimental technique—must be determined by further studies. But it is safe to venture that future scholarship will pay far more attention to the impact of Descartes and other mechanical philosophers than Foster even vaguely imagined necessary.

The present essay is meant as a contribution to and a reinforcement of the recent trend to study closely the role of the mechanical philosophy in seventeenth-century physiology. It treats the first introduction of the Cartesian philosophy in the physiology of England, where the period of initial invasion stretched, in a sense, from 1641 to 1664. Soon after the latter year

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[6] For this point in general, see Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd edition (Chicago: University of Chicago Press, 1970). For a major example in the work of a recent scholar, see Walter Pagel, *William Harvey's Biological Ideas* (New York. Hafner Publishing Company, 1967).

[7] Karl E. Rothschuh's sensitivity to this point of view is evident in the discussion of seventeenth-century physiology in his *History of Physiology*, trans. & ed. Guenter B. Risse (Huntington, N Y.: Krieger, 1973).

[8] Recent contributions by these authors relevant to this theme include. Thomas S. Hall, "Descartes' physiological method. position, principles, examples," *J Hist. Biol* , 1970, *3*. 53-79, Gustav Scherz, *Niels Stensen, Denker und Forscher im Barock* (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1964); Luigi Belloni, "De la théorie atomistico—mécaniste à l'anatomie subtile," *Clio Medica*, 1971, *6* 99-107

[9] Theodore M. Brown, *The Mechanical Philosophy and the "Animal Oeconomy"*, unpublished Ph. D. dissertation (Princeton, 1968).

[10] Rothschuh implies this, *op. cit.* (n. 7 above), pp. 82-83

English writers took a plunge into the waters of the mechanical philosophy, which until then they had been testing uncertainly. Before the former year there was no mechanical philosophy in England to speak of and therefore none to intrude into the territory of physiology.[11] Between 1641 and 1664 the philosophical landscape changed markedly, but even during the heady days of the Civil War and the Restoration—which cut through this period—the intrusion of the "new philosophy" into physiology was more like an inconclusive and ambivalent incursion than a decisive and massive invasion. The new Cartesian philosophy was at first present only implicitly. Later, even after its arrival had been explicitly acknowledged, it was denied admittance in one quarter and allowed it only on special terms in another. Even where it entered there were curious attitudes. To understand the first interactions between the Cartesian mechanical philosophy and physiological thought in the one country where later interactions were the most evident in Europe, we must be prepared, therefore, to follow a complicated course along several different paths.

The 1640s

Cartesianism entered English physiology in the 1640s, but only in the most implicit and indirect way. Nowhere was its presence explicitly acknowledged, and in only two works could its faint imprint be seen at all. The first and more important of those two works was the *Apologia pro circulatione sanguinis,* published by George Ent in 1641. Ent was the son of a well-to-do Belgian merchant who had fled to England to avoid religious persecution in the Low Countries.[12] After obtaining his Cambridge A.B. and A.M. in 1627 and 1631, Ent studied for five years at Padua, where he took his M.D. in 1636. Upon completing his studies, Ent remained for a while on the Continent, where Descartes in 1637 published his much anticipated *Discourse on Method.* It was doubtlessly while on the Continent for his extended period of study that Ent acquired the ideas, so strange for an English physician of his day and in several ways so resonant with Descartes, that he expressed in his *Apologia*.

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[11] Robert S. Kargon, *Atomism in England* (Oxford: Oxford University Press, 1966), Chapters II-V, describes the private flirtations with atomism of such figures as Thomas Harriot and Francis Bacon in the period before 1640. Since, however, these sympathies with ideas that were later to be generalized into the mechanical philosophy by Gassendi and Descartes were known only in small private circles, it can safely be said that were was no *public* mechanical philosophy in England before the 1640s and 1650s. Kargon's comments on Bacon, p. 52, are telling. "During his lifetime and for many years afterwards, the earlier works favoring the atomic theory were not published . . . the works of Bacon's mature period were all unfavorable to atomism. The major atomistic works of 1603-12, the *Cogitationes* and the *De Principii,* were not published until 1653."

[12] Biographical details from Charles Webster, "George Ent," in *Dictionary of Scientific Biography,* vol. IV (New York: Scribner's, 1971), pp 376-377

The *Apologia* is a treatise of 284 pages.[13] Written primarily as a rebuttal to Aemilio Parisanus' attempted refutation of Harvey's theory of circulation, the *Apologia* for the most part proceeds by taking up and then ridiculing each of Parisanus' arguments. In a large set of numbered paragraphs, Ent repeats Parisanus' reasoning, then uses classical and contemporary textual citations, careful logic, appeals to "experience" and heavy doses of sarcasm to rebut the rebuttal and thus re-establish Harvey's case and reputation. A typical paragraph begins this way:

> Your [Parisanus'] claim is that in a medium-tight ligature the vein becomes tumid below the band, because the blood above, in no way impeded by the ligature, is drawn there on account of the pain.[14]

The paragraph continues with a vigorous refutation of this opinion and concludes with a proud vindication of Harvey.

Woven through these pointed rebuttals, however, is a series of extremely interesting "Digressions." Their principal interest derives from Ent's incorporation in them of ideas which are not drawn from or, indeed, seem even compatible with Harvey's. In one Digression, for example, Ent considers the problem of "Attraction." He denies that there is any such thing in nature as the supposed "drawing power of the void"; the "horror vacui" is meaningless causally, because a nonentity (the void) cannot serve as an efficient cause.[15] Ent likewise denies that purging medicines, as followers of Hippocrates and Galen claim, can attract suitable humors by an imagined "similarity" or "familiarity" of parts.[16] Ent was here referring to the neoclassical doctrine that certain substances, both within and of importance to living bodies, have special appetent and even percipient properties which ordinary physical matter does not.[17] Moreover, Ent directly disclaims belief in the various bodily "faculties," which classical and neoclassical medical theory invoked to explain the attraction and retention of aliment and nutritive juice.[18] Where nutriment seems to be specifically "attracted" into a particular living vessel or vital tissue, it is in actuality absorbed in the manner of fluid soaking into a sponge.[19] Faculties in general are, in Ent's view, nothing more than the theatrical "deus ex machina" invoked everywhere to cloak our ignorance of true physiological processes.[20]

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[13] The full title is: *Apologia pro circulatione sanguinis, qua respondetur Æmilio Parisano Medico Veneto* (London, 1641).

[14] Ent, *Apologia*, p. 212 "Tua sententia est; in mediocri ligatura venam infra vinculum intumescere, quia superior sanguis, nihil impediente ligaturâ, illuc prae dolore trahitur."

[15] *Ibid.*, pp. 28 ff.: " . quia non entis nulla operatio Quod enim non est, non potest agere: cùm actus secundus praesupponant primum. . . . Dico autem, à vera philosophia alienum esse, finalem causam pro efficiente assignare "

[16] *Ibid.*, p 33.

[17] We will return to the question of "similary attraction" later in this essay, when we discuss Francis Glisson's *Anatomia hepatis* (1654).

In other Digressions Ent introduces equally interesting ideas. He argues often that the heart has no innate pulsatile faculty, as Harvey supposed. Instead, the flame-like heat of the heart ignites the combustible materials in the blood, thus producing a sanguinary effervescence which pushes out the walls of the heart and, when bubbling over into the aorta and attached vessels, distends the arterial walls as well.[21] A related opinion Ent also advocates is that the blood does not nourish the parts but merely serves as the vehicle for the heat and spirits which do properly vivify the body.[22] Yet Ent conceives heat and spirits in remarkably physical and chemical terms. The cardiac heat often appears to act only according to the simple principles of chemical combustion.[23] Moreover, when fresh aliment acquires the characteristics of blood, Ent seems to think that it does so in the manner of mere physical mixture, a view specifically denied in neoclassical doctrine.[24]

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[18] Ent, *Apologia*, pp. 40ff. For an account of what is called "neoclassical physiology" in this essay, see Lester S. King, "The transformation of Galenism," in Allen G. Debus (ed.), *Medicine in Seventeenth Century England* (Berkeley: University of California Press, 1974), pp. 7-31

[19] Ent, *Apologia*, p. 44

[20] *Ibid.*, p 40: "Si quaeras, unde cibi concoctio, chyli in venas distributio, ejusdem in sanguinem mutatio, & c. statim, a facultate aliqua id fieri, audies. Quod quid aliud est, quàm Deos advocare in theatrum, ut solvant nodum fabulae?"

[21] *Ibid.*, p. 49: "Veram autem hujus pulsûs causam arbitror, ipsius cordis primum impulsum. Nimirum sanguis in sinistro cordis ventriculo ebulliens, (ut postea explicabitur) sese in aortae ostium ingurgitat, unde dilatatur ipsius orificium, idémque motus ad extrema corporis perpetuatur: (quod nemini mirum videri debet qui motus celeritatem in corporibus tensis & continuis non ignoraverit) postea, in systole suâ sponte subsidit " Ent credits this notion to Aristotle, but it probably owes more to Descartes or writers of the Cartesian persuasion. For Descartes' views on heartbeat and their proximity to ideas in the Aristotelian tradition, see Étienne Gilson, *Études sur le rôle de la pensée médiévale dans la formation du système cartésien,* 3rd edition (Paris: J Vrin, 1967).

[22] *Ibid.*, pp. 69-71· "Eundem verò sanguinem nutrire, sive partes novas extruere, aut deperditas instaurare, haud verisimile videtur. Neque enim cum iis sum, qui *vitam & nutritionem, non reipsâ, sed ratione tantùm aliqua differre* existimant. Verbo dicam: Sanguis totum corpus animalis peragrat, tum ut tantae vitalis balsami copiae excipiendae locus sit, tum etiam ut partes singulae à cordis calore, cujus sanguis vehiculum est, vivificentur."

[23] See, for example, *Apologia*, pp. 96 ff

[24] *Ibid.*, pp. 241-242. For a neoclassical denial see Jean Fernel, *Universa medicina,* Part I, Book VI. A passage of particular note, from p. 238 of the 1593 Frankfurt, is as follows: "Quem igitur chylum ope venarum mesenteri praeparatum, & rudi quadam sanguinis forma adumbratum iecur accepit, hûc per eam moram &

Finally, digestive concoction is not the substantial alteration of foodstuff taking place under the instrumentality of innate heat and guided by an appropriate set of faculties; rather, a chemical fermentation induced by an acid humor activated by heat constitutes the whole process of digestion.[25]

What all these novel ideas in Ent's Digressions add up to, of course, is an episodic critique of neoclassical physiology, still dominant in the early 1640s. For in place of directive teleology, special vital principles, and unique and appetent living substances, Ent substituted a philosophical rejoinder to explanatory teleology, deliberately unqualified physical analogies, and straightforward applied chemistry. The unnamed source for many of these innovations could well have been Descartes, for in the introduction to his treatise Ent warmly praises Henry Regius, one of the best-known Cartesian disciples of the day.[26] Ent's ideas, at any rate, seem to echo several of Descartes', both in their particulars and in their general thrust.[27] And even if Ent may not have acknowledged all of his sources, his *Apologia* contained much that was new and, therefore, much that was potentially disturbing.

Despite their originality in the English setting, Ent's implicitly Cartesian views were greeted largely in silence. Physiological ideas of the early seventeenth century apparently continued in force, little if at all affected. The Professor of Physick at Gresham College until 1642 was Thomas Winston, a man of standard and static views; from 1643 to 1652 it was Paul de Laune, a minor figure who left no surviving literary or philosophical remains.[28] At Oxford in the early 'forties business largely proceeded as usual. The Regius Professor of Medicine was Thomas Clayton, an interesting and energetic

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contagionem, vera & expressa sanguinis specie ornat & perficit: cuius operis non quaevis illius sine delectu particula, sed quae duntaxit illius propria est caro atque substantia effectrix est, quáquam reliquae omnes in id usum quendam & commoditatem habent. Dum ea caro quae substantia concretum & coactū sanguinem exprimit, allapsum ex ventre chylum sensim entitur in propriam naturam cōmutare, sibique prorsus assimilare (est enim hic scopus unus in quem unaquaeque particula caloris sui munus dirigit atq; refert) primum quidem crassiorem reddit & colore rubrū: quûmque iam ad quandam mediocritatem accessit, sanguis verè est & appellatur, tantò à iocineris substantia deficiés, quantò ventris chylum superat. Ex quo percipi potest sanguinis confectionem, non modò caloris beneficio, sed maximé iocineris ingenita proprietate compleri ."

[25] Ent, *Apologia*, pp. 240-241.

[26] *Ibid.*, Opening remarks to Harvey " . sciréмque viros alios Medicinae consultissimos hanc cum illo litem agere, inter quos *Henricus Regius,* haud è trivio philosophus, validissimos ejus arietes nuper retudit . ." For direct evidence of Regius' relationship with Descartes, see Renè Descartes, *Oeuvres*, ed. C. Adam & P. Tannery, new edition (Paris, 1969), t. 2 pp. 305-306, 527, 616 & t.3, pp 65-68.

[27] For general accounts of Descartes' physiology, see Thomas S Hall, *op. cit.* (n. 8 above) and Theodore M. Brown, "Descartes: Physiology," in *Dictionary of Scientific Biography,* vol. IV (New York: Scribner's, 1971), pp. 61-65.

[28] John Ward, *Lives of the Professors at Gresham College* (London, 1740), pp. 266-269 For one of Winston's typically "standard and static views," see his *Anatomy Lectures at Gresham College* (London,

man.[29] But two questions argued in Vespers in 1640 testify to the overwhelming force of tradition.

Whether a variety of food conduces to health? Neg..
Whether the veins sanguify? Aff.[30]

Later in the 'forties there was a brief flurry of activity when William Harvey settled in Oxford for a few years, but Harvey's presence was important not for what it stimulated or affected in the 'forties but for what was remembered of it in the very different decade of the 'fifties.[31]

For Cambridge there is somewhat more specific information, and most of it points in the same continuously traditional direction. The holder of the Regius Professorship there from 1636 to 1676 was Francis Glisson, a notable figure who will prove increasingly important as this essay unfolds. Born in 1597, Glisson received his A.B., A.M., and M.D. degrees from Cambridge in 1621, 1624 and 1634.[32] He was elected Fellow of the College of Physicians in 1635. Sometime after he won appointment as Regius Professor, although it is not now known exactly when, he moved back to Cambridge, at least for a while, and assumed his lecturing duties. From manuscript notes surviving early lectures apparently delivered in the late 'thirties and early 'forties, Glisson seems to have been a fairly conservative young man, little influenced by the sorts of ideas Ent espoused. Among the issues he argued publicly as Regius Professor in these years are the following:

Humors are electively attracted to ones similar to themselves.
Sanguification is a similary action.
Nutrition is performed through similary attraction.

1659) [posthumous publication], p. 62: " . . . the *Fundus* . . . is almost seated in the middle Region of the *Epigastrion*, of the bottome of the stomach: It is not the most fleshy part . . yet it is the seat of *chylosis*, which is from the inborn property and specificall form, and innate heat of the stomach . . . ."

[29] There were two Thomas Claytons, who are sometimes confused. Thomas Clayton the older stepped down as Regius Professor in 1647 and turned the chair over to his son, a much less distinguished man. Clayton senior is given high praise by A. H. T. Robb-Smith, "Medical education at Oxford and Cambridge prior to 1850," in F. N. L. Poynter (ed.), *The Evolution of Medical Education in Britain* (London. Pitman Medical Publishing Co., 1966), pp 30-33.

[30] *Acta Congregationis. Oxon.*, vol. 16, f. 181.

[31] For Harvey at Oxford during the Civil War, see Geoffrey Keynes, *The Life of William Harvey* (Oxford: Clarendon Press, 1966), pp. 291 ff. For later "remembrances" of Harvey's work, see latter portion of this essay.

[32] For accounts of Glisson's career, see "Francis Glisson" in the *Dictionary of National Biography*, vol. VII (Oxford: Oxford University Press, 1963-1964), pp. 1316-1317 and William Munk, *The Roll of the Royal College of Physicians of London*, vol. I (London, 1878), pp. 218-221.

The natural faculty does not flow in [to a part].
Purgation is performed by the similitude of substance.[33]

If these titles are properly representative, as they seem to be, Glisson's views would appear to have remained well within the bounds of neoclassical orthodoxy.

At Ent's own College of Physicians the situation was somewhat different, though not radically. The College seems to have been intellectually fluid or, perhaps rather, ambivalent in these years. William Harvey, George Ent, Francis Glisson and Thomas Winston were all well regarded Fellows at this time. Moreover, the College tentatively experimented with aspects of "modernity," in 1648 appointing an official chemist who was responsible for the preparation of new sorts of medicaments.[34] Yet at the same time, the Fellows in 1647 adopted several new statutes but retained the old one requiring a rigorous examination in Hippocrates and Galen of all future members.[35] Introduced into this context, the sorts of ideas Ent espoused would be received mostly in silence, but they would also, it would seem, begin to win some sympathetic hearing.

The evidence for an at least partially sympathetic reception comes from lectures delivered by Baldwin Hamey in 1648. Hamey was an active member of the College in these years and a personal friend of William Harvey.[36] Elected Fellow early in the 1630s, Hamey rose steadily in the ranks of the College in the next two decades. In the 1640s he served as Censor and in 1647 was selected to give the College anatomy lectures—in January, 1648. In these public lectures Hamey expressed many views which were incompatible with the neoclassical doctrines he himself had previously written and which, therefore, seem to exhibit some sympathy for the new directions in which Ent had begun to move.[37]

The format of Hamey's *Praelectiones* is the standard seventeenth-century anatomy format.[38] He begins with the skin and musculature of the lower belly and works up to the head and brain. At the termini and in between he exhibits the detailed structure of the parts, cites notable classical and Renaissance authorities where relevant, and occasionally intersperses his descriptive accounts with physiological explanations. When he treats the liver, heart, or kidneys, he breaks off his description and citations to offer a theory of organic function. It is in these theoretical excursions that Hamey seems to show something of Ent's innovative spirit.

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[33] British Museum Sloane MSS. 3309 & 3310 *passim*.

[34] The College's decision to appoint a chemist is recorded in its official *Annals*, vol IV, p 7. This citation refers to a typewritten transcript and translation of the original MS *Annals* kept in the Library of the Royal College of Physicians, London. For an account of the College's appointment, see also Sir George Clark, *A History of the Royal College of Physicians of London*, vol. I (Oxford Oxford University Press, 1964), p. 286.

[35] See Clark, *op. cit.* (n. 34 above), pp. 278 ff for a discussion of 1647 Statutes, and pp. 410-411 for a transcription of the Examination Statute.

[36] On Hamey, see John J. Keevil, *The Stranger's Son* (London G Bles. 1953).

[37] The previously written neoclassical doctrines referred to here are from Hamey's *Universa medicina* of 1624, a manuscript of a never published treatise now in the Library of the Royal College of Physicians, London.

Hamey's description of kidney function is most clearly representative of the new directions in which his ideas were moving. After explaining at some length that Galen reasoned his way to an innate attractive faculty in the substance of the kidneys by first assuming that the blood did not and could not circulate, Hamey, an advocate of circulation, offers his own detailed and contrary account of renal action:

> Now then . . . [since] our kidneys were made to separate water from blood, and that we conceive all our blood must at one time or other passe by them to affect it, let us at present see of what use all the particulars be in their Structure, for the making out of the same. With Galen, the Temper of a part is usually the principall cause of its action, and this temper is its IDIOSYNCRASIS; but here we need not urge any hidden property, since we gave a manifest quality, w^{ch}, I conceive, to be, a more moderate heat, than is found in the adjacent parts: for, if the blood w^{ch} floweth with strong pulsation and ebullition from the Aorta by the emulgents, were not by its diversion to the kidneys somewhat qualified, then will all separation of urine be inconceavable; and, for this cause, we have grounds enough to imagine, that the kidneys were thrust aside, as farre as might be, and there covered with abundance of fat, the fittest Vestment for a moderate temper, by keeping of both heat and cold too, whose extreames doe equally hinder separation. And of this we have some evidence upon letting of blood, from w^{ch} the Serum is not secerned, but upon a . . . declination, from a boiling, to a moderate heat, before it comes to the other extreame of coldnesse: for, if at first it be received in a large cold Vessel, or in a small quantity, or by drops in any thing whatsoever, the blood will seeme good . . . [but] because it had no considerable time to change in, from one extreame to the other; for it is heat, according to that [opinion] of Aristotles . . . that separates heterogeneous bodies, but it must not be too fervent . . . .[39]

This explanation, based on the simple physical effects of heat and cold, was obviously formulated as an alternative to the neoclassical notion of the kidney's innate attractive faculty. In fact, just how heterodox Hamey's account was can be seen by comparing it to the explanation of renal function William Harvey had offered to the College in his own anatomical *Praelectiones* of 1616:

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[38] Hamey's *Praelectiones* is also an unpublished manuscript now in the Library of the Royal College of Physicians, London. Its full title is *Praelectiones anatomicae habitas in Collegio medico, 22, 24, 25 Januarii 1647/8 Baldwin Hameo.*

[39] The pages of Hamey's MS are unnumbered. This quotation is from a section entitled "De renibus, uteribus, et vesica."

> . . . urine is excreted in the same manner as the others through its proper channels like the excretion of gall and of sperm . . . . As to whether it moves by attraction or explusion etc., the answer in one word is that the originator of everything in the body is innate heat; it exists in everything even in all the excrements while they are governed by Nature. Innate heat as it concocts both attracts and expels. For this reason just as the food designed to nourish the parts is partly conveyed to them by the digestive faculty and partly attracted to them and partly seeks them spontaneously, so the excrements partly seek a way out, partly sink down to wherever there is a way out and are in part attracted and in part expelled. Unnatural excrements which are not governed by Nature are productive of disease and by their malignity seek different parts of the body as the bones, the legs etc., and the lungs. Just as it is in the world so is it in the microcosm, all things are moved of their own accord to their proper place. Wherefore whosoever says they are driven or attracted implies that they will be moved by an accord that is other than their own. Compare Aristotle's remarks in *De generatione*, Book II, ch. 4. And indeed excrement is discharged in accordance with its own nature. Movement by means of attraction, however, does exist in the body, for example, heat, pain and a vacuum all attract, the one lust, the other a purgatory and the third something to chew.[40]

Set in contrast, these two quotations clearly suggest that while Harvey in 1616 and Hamey in 1648 may have conceived themselves as moving in the same basically "Aristotelian" direction, Hamey obviously moved much farther towards a "mechanical" or physicalist explanation. His account of renal action is more consistently based on simple physical operations and his language here is free of references to such neoclassical entities as "innate heat," "attraction" and "faculty." Hamey quite possibly moved this far because, like Ent and perhaps under the same unnamed philosophical influence, he put much more stock than Harvey in the operation of simple physical principles within the body.

Hamey's apparent resonance with Ent, however, was not everywhere so strong. In several other of his physiological explanations, he shows little sympathy for the more daring kinds of ideas contained in Ent's *Apologia* and its "Digressions." He nowhere seriously entertains the chemical and mechanical analogies Ent introduced, and he is often content to retain neoclassical vocabulary and conceptions. Thus, one prime action of the stomach is "Chylosis," the manufacture of chyle from food, for which "the substance of the Ventricle i.e., the stomach . . . may be judg'd Causa principalis Chylificationis."[41] And similarly, the mixing of blood and chyle together in the liver "may, in some sense, be cal'd sanguification, though truly, it be a necessary preparation onely therunto; w^{ch} very thinge, Galen himselfe . . . confesseth."[42]

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[40] William Harvey, *Praelectiones anatomae universalis*, ed. & trans. Gwenneth Whitteridge (London: E & S Livingstone, 1964), pp. 163-165.

Hamey was thus both traditional and untraditional at the same time. His anatomical *Praelectiones* contain certain definite hints of modern influence and of a general progressiveness (he clearly and consistently defended Harvey's theory of the circulation of the blood), but they also show unmistakable signs of neoclassical residues. Neoclassical residues in a generally forward-looking set of lectures can be accounted for in terms of an understandable and almost predictable conceptual inertia, and in this regard Hamey very likely spoke for many others at the College as well. The College of Physicians was ambivalent in the 1640s; for every Ent there was a Winston, and Hamey fell in between. Given this environment in what was then England's most progressive medical institution, it is no wonder that the 1640s saw no fundamental change in England's pre-mechanical physiology.

The 1650s and Early 1660s

By 1650 the general situation in England was very different from what it had been ten years before.[43] New ideas percolated in very recently formed intellectual circles, and there was suddenly a host of new scientific and philosophical personalities. Political and social life was extraordinarily unsettled. England was in the midst of a revolution and had just witnessed the rise to power of the Army within the Parliamentary ranks, the execution of King Charles I, and the establishment of the Commonwealth. Parliament itself was frequently attacked by Oliver Cromwell and his more radical Army followers, and several of England's other established institutions were subject to thorough, often scurrilous abuse by violent elements in society suddenly given license by the tumultuous turnings of events.[44] In the late 'forties several of the political radicals had already tried to strengthen their position by aligning themselves with new intellectual currents, so that by the early 'fifties the atmosphere was so multiply tense that William Harvey could express his reaction to contemporary philosophical tendencies only in outraged, bitter terms. According to John Aubrey, Harvey in 1651

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[41] Hamey, *op. cit.* (note 39, above), from the section entitled "De peritonaeo."

[42] *Ibid.*, from the section entitled "De intestinis."

[43] For a recent general characterization of England during the Puritan Revolution, see Christopher Hill, *God's Englishman. Oliver Cromwell and the English Revolution* (New York: Harper and Row, 1970).

[44] For accounts of some of the contemporary literature, see Allen G. Debus, *Science and Education in the Seventeenth Century* (New York: American Elsevier, 1970), pp 15-56 and Charles Webster, "English medical reformers of the Puritan Revolution," *Ambix* 1967, *14*: 16-41.

> . . . bid me goe to the Fountain head, and read Aristotle, Cicero, Avicenna, *and did call the Neoteriques shitt breeches.*[45]

Times and tempers were clearly changing.

One immediately significant development of these strange times was the sudden ready availability of all sorts of books with every kind of new and striking idea. Old printing and publishing restrictions had recently been altered as a consequence of Civil War,[46] and coming in the wake of these alterations were scores of treatises and tracts which gave expression to highly articulated alternatives to traditional academic philosophy and neo-classical medicine.[47] The works of such famous foreign authors as Paracelsus, van Helmont and Gassendi were suddenly available in England and even in English.[48] But much more significantly, Descartes' *Discourse on Method* and *Passions of the Soul*—full and free-standing accounts of the dramatic Cartesian physiology founded on the principles of Descartes' equally dramatic mechanical philosophy—appeared in English editions of 1649 and 1650.[49] Here were explicit statements that the human body is nothing but a machine or engine, a physical contrivance whose organs, vessels, fluids and vapors are identical in composition and operation to the parts of clocks or other automata. This was a radically new, completely physicalist and fully "mechanical" approach to the "animal oeconomy" that the guardians and spokesmen for English physiology had to reckon with.

It was against this background or, at least, in this atmosphere, that much of the physiology of the 'fifties and early 'sixties was produced. At Oxford, Cambridge, the College of Physicians and, in a sense, at Gresham College too—the well-established loci of such English physiology as then existed—activity proceeded to some extent along lines previously drawn during the first five decades of the century. Scholastic exercises continued in the Oxford and Cambridge medical faculties, and the classically-based examination statute remained in force at the College of Physicians.[50] But because the circumstances of the 'fifties were in many ways so striking and strange, physiology itself became different too. In various, not always subtle ways environing circumstances and the erosion of traditional conceptions caused by the active presence of the several new and antagonistic philosophies exercised a palpable influence on physiological thought and practice.

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[45] *Aubrey's Brief Lives,* (ed.) Oliver Lawson Dick (Ann Arbor: University of Michigan Press, 1957), p. 129. For some of the unnamed "neoterics" to whom Harvey may have been referring, see Richard Foster Jones, *Ancients and Moderns,* 2nd edition (Berkeley: University of California Press, 1965), Chapter 5. For a more recent account, see Hugh Kearney, *Scholars and Gentlemen. Universities and Society in Pre-Industrial Britain, 1500-1700* (Ithaca, N. Y.: Cornell University Press, 1970). Chapters 6 and 7.

[46] For a detailed discussion, see Frederick S Siebert, *Freedom of the Press in England 1476-1776* (Urbana, Ill.: University of Illinois Press, 1952), pp. 165-233

[47] See n. 44 above.

[48] English editions of Paracelsus, van Helmont and Gassendi were as follows: Paracelsus, *Medicina diastatica, or sympatheticall mumie* (London, 1653) and *Of the supreme mysteries* (London, 1656); van Helmont, *Deliramenta catarrhi* (London, 1650) and *A ternary of paradoxes* (London, 1650); and Gassendi, *Institutio astronomica* (London, 1653), *The mirror of the true nobility* (London, 1657) and *The vanity of judiciary astrology* (London, 1659).

[49] The exact titles of the English editions were, respectively: *A discourse of a method* (London, 1649) and *The passions of the soule* (London, 1650).

Even in the new decade, however, changes came neither abruptly nor uniformly. Institutions at which physiology was pursued varied, had subtly different traditions and recent histories, and possessed unique constellations of personalities. Thus, those interested in physiology at the College of Physicians during the 'fifties were prepared to respond differently from those at Oxford. Indeed, the differences in response are quite as interesting and important as the commonality of the stimulus.

Let us consider first the London College of Physicians, which by 1650 had already begun to feel rumblings of change within its own ranks. It was at this time that a small but energetic group of Fellows tried with some success to import the sorts of ideas and activities with which they had recently become involved in meetings at Gresham College, where, a few years earlier, an extramural scientific club had crystallized.[51] Often consisting of the College's Drs. Jonathan Goddard, George Ent, Francis Glisson and Christopher Merret, along with such prominent figures of Civil War London as John Wilkins, John Wallis and Theodore Haak, the Gresham group functioned as an informal but high level discussion club which met regularly to consider reports of recently advanced ideas and discoveries. The purpose of these meetings was to exchange new and general scientific information; specific topics in anatomy and physiology held considerable interest for the medically-biased group of participants.

By 1649, however, the Gresham discussion club had disbanded. Several of its members went to Oxford (where we will follow their activities later in this essay), but three of the physicians in the original group remained in London and turned their attention to organizing their colleagues within the College of Physicians into a Bacon-style collaborative research club.[52] They were perhaps motivated in part by a half-conscious and quasi-political desire to defend their College by pursuing experimental activities and thus forestalling the attacks of certain radical critics. In any case, as we noted earlier, the College appointed an official chemist in 1648, and his laboratory became one main locus of activity and interest. In addition, a group of eight, including Glisson and Goddard, studied the "new disease," rickets, and in 1650 Glisson published their collective findings in a volume entitled *De rachitide*.[53] Interest in the next few years widened to include research into anatomical and physiological subjects, and in a short time the prevailing mood at the College was such that the annual lecturers felt free to abandon their prescribed general anatomy format, to use their appointments instead as something like research posts.

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[50] On conservatism at Oxford and Cambridge in the 1650s, see Kearney, *op. cit* (n. 45 above), pp 120ff, for the College of Physicians, see above, n. 33.

[51] See Margery Purver, *The Royal Society: Concept and Creation* (Cambridge, Mass . MIT Press, 1967), pp 161 ff.

In this self-consciously "Baconian" atmosphere of the College of Physicians investigations were undertaken into the functioning of the parts of the "animal oeconomy," a term then coming into popular usage. The most important studies were pursued by Francis Glisson, still the Regius Professor of Medicine at Cambridge but in the 'forties and 'fifties a semi-permanent resident of London.[54] Glisson's research culminated in the *Anatomia hepatis* of 1654, a work notable both for its solid scientific achievements and for the insights it yields into deep-seated neoclassical attitudes towards physiological explanation maintained by even the most progressive members of the mid-century College of Physicians.

The subject of Glisson's investigations was the structure and function of the liver, one of the three most important organs in neoclassical physiology. According to Galen and his later-day followers, the liver performs the crucial task of fashioning blood out of the partially digested food brought from the intestines through the mesenteric veins.[55] The hepatic parenchyma possesses a special faculty which transforms chyle into venous blood while bile is produced as a waste product. But in the light of Harvey's and other seventeenth-century discoveries the liver could not be seen any longer to perform these functions. The theory of circulation required that blood be already formed *before* it reaches the liver on its course through the body. Harvey also reported embryological evidence that revealed the liver first appearing *after* the blood was fashioned.[56] Moreover, recent discoveries by Continental anatomists created further doubts about the traditional hepatic function. In 1622 Gaspar Aselli discovered the chyle-absorbing lacteals, and about twenty-five years later Jean Pecquet found that these vessels delivered chyle not to the liver, as Aselli had supposed, but through a separate vascular system into the vena cava and then the heart.[57] Pecquet's discovery demanded an immediate adjustment of the traditional theory of hepatic function, and Pecquet himself tried to supply a "modern" alternative. A traveller in philosophical circles where Descartes' ideas were popular, he argued that the liver, instead of fashioning blood, acted primarily as a filter to separate bile mechanically from the circulating blood mass.[58] A large quantity of bile-containing blood regularly soaks into the hepatic parenchyma, but then a dual excretion mechanism, depending on the size and shape of both the blood particles and the sieve-like openings in the liver, operates to filter out bile from the sanguinary fluid.[59] Bile passes into the gall-bladder and purified blood continues on its circuit. Pecquet's theory and original discoveries were published in the early 'fifties, when they contributed to a bitter Continental debate over hepatic function.[60]

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[52] See Charles Webster, "The College of Physicians. 'Solomon's House' in Commonwealth England," *Bull. Hist. Med.*, 1967, *41*: 393-412.

[53] An English translation by P. Armin appeared in 1651 under the title *A Treatise of the Rickets*. For some account of this treatise, see Edwin Clarke, "Whistler and Glisson on rickets," *Bull. Hist. Med.*, 1962, *36*: 45-61 On "new diseases" generally, see Lloyd G. Stevenson, " 'New diseases' in the seventeenth century," *Bull Hist Med.*, *1965*, *39*: pp. 1-21.

[54] On Glisson's peregrinations, see n. 32 above. Other important studies, though not in Glisson's class, were undertaken by Thomas Wharton, also of the College These were published as *Adenographia· sive glandularum totius corporis descriptio* (London, 1656). The general conclusion about Glisson's intellectual position presented further on in the text applies also to Wharton

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[55] See, for example, Galen, *On the Natural Faculties*, trans. Arthur J. Brock (Cambridge, Mass.. Harvard University Press, 1952), pp. 215 ff.

[56] For Harvey's report, see the edition of his *De generatione animalium* published in *The Works of William Harvey*, trans. and ed. Robert Willis (London, 1847), pp. 415 ff. Glisson refers to Harvey's observations in his *Anatomia hepatis*, pp. 359 ff. of the 1659 Amsterdam edition.

[57] See Nikolaus Mani, *Die historischen Grundlagen der Leberforschung*, Vol. II (Basel: Veröffentlichungen zur Geschichte der Medizin, Fasc. 21, 1967), pp. 82 ff.

[58] *Ibid.*, p 89. See also Pierre Huard & Marie-José Imbault-Huart, "Jean Pecquet," in *Dictionary of Scientific Biography*, vol. X (New York: Scribner's, 1974), pp. 476–478

[59] *Ibid.* Cf. N. Mani, "Jean Riolan II and medical research, "*Bull. Hist. Med.*, 1968, *42*: 133 and 136. Also, pp. 141-142 of the "Riolan" article, Mani describes Pecquet as "not only a brilliant experimental physiologist but also a pioneer of iatromechanical thought." This description underlines the self-consciousness with which Pecquet assigned the liver the function of filtration. The principal Latin passage in Pecquet's writings on which this judgment is based is quoted by Mani in *Die historischen Grundlagen* (n. 57 above), p. 431, n. 41 as follows: "Ita cribralium demeaculorum varietas, et colandorum corporum diversa configuratio fluidis hoc transitu interdicit, illac autem concedit effugium. Hic per renes in vesicam urina secedit una cum bile, quae intra massam sanguinis subtilissima est; aciditas abit in lienis nutrimentum; subtilior e iecore bilis in cystidem fluit, crassiore in meatum hepaticum derivato. Uno verbo dicam, per sole sibi destinata ostia, praeclusis aliorum omnino posticis, excrementa posse divertere."

Glisson started where this debate left off. Thus, to test Pecquet's notion that the liver merely separates excremental bile from circulating blood in a mechanical fashion, Glisson devoted considerable attention to exploring in exquisite detail how the facts of hepatic microstructure might suit the liver to a sieve-like secretory function.[61] He began by uncovering the complicated network of vessels inside and outside the liver. Using the best techniques of recent Continental anatomy and proceeding with great caution, he made the intricate vascular connections beautifully clear. By excarnation, inflation and injection he shows that the two major biliary vessels, the *porus biliarius* and the *meatus cysticus,* are distributed along with the branches of the portal vein and the vena cava throughout the hepatic parenchyma.[62] Capillary roots of the bile-containing vessels are always found associated with capillary roots of the blood-containing vessels. Reflecting a major concern of William Harvey, Glisson shows, however, that there are no direct vascular anastomoses.[63] This can only mean, he reasons, that blood carrying bile is brought into the liver, where it discharges its contents into parenchymatous spaces; the bilious portion then enters the biliary vessels, which in turn transport the separated bile back outside the liver either into the intestines or into the special bile receptacle, the gall bladder. In general,

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[60] For the Continental debate, which also involved Bartholin, Riolan and Rudbeck, see Mani, *Die historischen Grundlagen,* pp. 90 ff.

[61] *Ibid.,* pp. 104-120. Mani calls Glisson's more than five hundred page work "eine grosse Monographie" and comments, p. 105, "Dieses Werk wurde für zwei Jahrhunderte grundlagen und richtungsweisend."

[62] Glisson, *op. cit.* (n. 56 above), p. 267: "Vasa haec . . . in eo consentiunt, quod eorum quodlibet ramusculos capillares in unamquamque parenchymatis partem distribuat. Nullam scilicet hepatis partem, quae sit alicujus momenti, reperias, ad quam venae cavae. Portae, atque pori bilarii capillaria vasa non pertingant."

[63] Harvey had been concerned to establish this point beyond any doubt as recently as 1649 in his *Exercitatio de circulatione sanguinis* (Cambridge, 1649). See especially pp. 137-138 of the Everyman edition (London, 1963) for a passage that may have influenced Glisson: "I myself, with what diligence has been mine to command, have sought hard and have expended not a little of both time and effort in anastomosis exploring. But I have never succeeded in finding vessels, namely, arteries with veins, mutually connecting by mosculation . . . [For] there is [in fact] no anastomosis at all in the liver or spleen or lungs or kidneys or any other viscus. When these had been boiled until the whole of the parenchyma had been rendered friable and a sort of powder had been prepared and needled out from all the vascular fibres, I should have been able to see all the fibre of any division and all the capillary threads, had such existed. I dare, therefore, boldly assert that there is no anastomosis of the portal veins with the cava, or of arteries with veins, or of the small capillary branchlets of the bile duct, which are dispersed through the whole moulding of the liver, with the veins. This can be seen in a fresh liver only, all the branches of the vena cava, creeping through the gibbous portion of the liver, have coats riddled with innumerable small holes, as if in a sink constructed for the reception of the blood in its descent. The branches of the portal vein are not similarly arranged, but are divided into offshoots; and both distributions of these vessels, one in the flat part, the other in the gibbous, run everywhere without anastomosis to the portal fissure of the viscus."

> The office of the liver is no other than some depuration of the blood. It is indeed very probable, that the blood is purified in various manners. Namely, the kidneys discharge salt and terrestrious excrement mixed and diluted with the urine; the pancreas similarly evacuates other material . . . and several other parts are endowed with this peculiar kind of function. Thus, if we regard the liver as if it were the part designated for the depuration of the blood, and also if we consider the gall bladder annexed to it and the biliary vessel distributed throughout the hepatic parenchyma, then we ought to say that the liver has been instituted by Nature in order to render the blood cleansed of bile.[64]

By concentrating all this attention on the hepatic vessels and by emphasizing the liver's secretory role, Glisson would seem, like Pecquet, to be driving primarily at a mechanical, sieve-like function for the organ. Suddenly impressed with recent Continental philosophy as well as anatomy, it might appear, he was departing from his prior, strongly-held commitment to the neoclassical physiological tradition. But this was not the case, at least not in important ways. For though Glisson sometimes referred to the liver as a "sieve," he neither spoke of the blood as a purely hydraulic fluid nor thought of the liver and its vessels as a mechanical device. As far as blood was concerned, Glisson maintained along with Harvey and other, less daring predecessors, that it was a living and architectonic fluid infused with vital heat.[65] In fact, the blood is first created neither by the liver, the heart, nor the veins, but by the *vital spirit* residing in the original seminal matter of the ovum.[66] And as for the supposedly sieve-like operation of the liver, Glisson was particularly careful, as Pecquet was not, to distinguish between mere mechanical filtration and the special, vital secretion of bile that actually takes place in the hepatic parenchyma.

Glisson based his explanation of biliary secretion on the notion of "similar attraction." This idea was both a part of the neoclassical tradition and a conception closely related to "faculties." The neoclassical scheme maintained that certain parts of the body were endowed with special, quasi-percipient or appetent powers which allowed them to attract, retain, expel and alter suitable materials These were properties which only living tissues could possess. For example, although a hollow bladder can attract matter by the physical principle of "horror vacui," only the walls of the mesenteric veins can attract chyle from the material in the intestines and only the flesh of the kidneys can selectively absorb urinous matter from the mass of the blood.[67] Glisson located an equivalent, distinctively vital, non-mechanical sort of attraction in the hepatic vessels within the liver.[68] He also claimed that the hepatic parenchyma itself helped separate bile from blood not by a sieve-like filtration but by a selective and "similar" attraction which acted by an "actual exertion to mutual intercourse."[69] Thus,

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[64] Glisson, *op. cit* (n 56 above), p. 411: " . . officium illi aliud nullum superest; praeter aliqualem *sanguinis depurationem*. Est quidem verissimum, sanguinem variis modis depurgari. renes nempe salsum & terrestre excrementum cum materia potulenta mistum ac dilutum exonerant: pancreas similiter materiam aliam a nemine hactenus explicatam evacuat: aliaeque complures partes peculiaria hujusmodi officia nactae sunt. Verum si hepar spectemus ceu partem sanguinis alicui depurgationi designatam: simulque vesiculam felleam eidem annexan, & porum bilarium per illius parenchyma distributum intueamur, dicedum profecto fuerit, hepar in eum finem a Natura institutum esse, ut sanguinem a bile defaecatum reddat . . ."

[65] *Ibid.*, p. 412· "Calor vitalis perpetua sanguinis coctura in ipso amarulentum sive felleum ejusmodi humorem necessario producit." Cf Mani, *op. cit.* (n 57 above), p. 113 & p. 445, n. 78

[66] Glisson, *op. cit.* (n. 57 above), p. 359: "Si vero accurate pensitemus, quidnam sanguinem primitivum efficiat; liquido constabit, non hepar, neque cor, nec etiam venas, sed *spiritum vitalem* in prima seminali materia hospitantem, illius auctorem esse. Nam in ovo incubato sanguinem reperias, antequam cor, hepar, venaeve efformatae fuerint; imo vero priusquam in ipso puncto saliente vel minimum sanguinis extiterit."

> The capillary vessels minister to the parenchyma, and distribute the bilious blood into all the parts equally . . . . This part of the separation is *mechanical.* But the *similar* parts pertain primarily to the parenchyma of the liver, although they are also closely related to both kinds—biliary and sanguinary—of vessels. For it is indeed probable, that the parenchyma is constituted of different kinds of parts, and, because of this, that certain humors are joined to one and certain others to another. Thus, the humors apply themselves to those parts with which they have the greatest similitude and affinity. And it is thus that secretion begins. Specifically, the particles of the parenchyma which have the greatest familiarity with the biliary humor, attract it to them and direct it to the opening of the biliary duct. Likewise, those particles which are most like pure blood, attract it to the capillary extremities of the vena cava. In the meantime, nevertheless, those roots of both the biliary duct and the vena cava, assist the parenchyma in this task by their own *similar* attraction.[70]

Hepatic secretion, clearly, is not entirely sieve-like. It depends on similar essences and their mutual (anthropomorphic) perception. By insisting on this similar attraction, Glisson deliberately and carefully built a non-mechanical operation right into the liver's principal function.

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[67] See Brock's edition, *op. cit.* (n. 55 above), pp. 275 ff.

[68] Glisson, *op. cit* (n. 56 above), 246: Dixi . . . attractionem *similarem,* ut ab organica sive *mechanica* distinguerem. Nam licet attractio hujusmodi in corporis organis reperiri possit, illa tamen haud organice perficitur; sed a simulitudine, familiaritate, atque affinitate corporis attrahentis cum attracto, promanat; eademque similitudo in constitutione partium *similari,* non autem *organica,* fundatur. Ac propterea opinor, actionem hanc latissime acceptam, commode dici posse *attractionem similarem.*"

[69] The phrase is from the following sentence, *Ibid.,* pp. 246-247: "His ita praemissis, concessoque posterum, corpora similaria semel excitata, *actuali conatu ad mutuum congressum* endere; quae jam dicta sunt, praesenti negotio applicabimus."

[70] *Ibid.,* pp. 472-473: "Vasa enim capillaria parenchymati proxime ministrant, parque eorum distributo

It would thus appear that one of Glisson's important aims in the *Anatomia hepatis* was to show how careful, empirical studies pointed to actions and properties unique to living things and irreducible to physical explanation. Contrary to Descartes and his assorted advocates, Glisson seems to have wanted to show where the mechanical philosophy failed and broke down, rather than where it succeeded or opened up interesting new possibilities. This attitude was certainly evident at several other places in his book. At one point, for example, he ridicules a popular corpuscular type of explanation,[71] and at several other places he deliberately resorts at length to the vocabulary and conceptions of neoclassical physiology (including the invocation of "faculties") to supplement the fluid mechanics of bile storage and distribution.[72] The recently intruded mechanical philosophy had no place, or at least no important and unchallenged place, in Glisson's physiology.

Glisson's reasons for maintaining this essentially conservative position were doubtlessly multiple and various. He was trained in and himself taught neoclassical medicine at Cambridge, and this helped keep his physiological conceptions traditional. Furthermore, he was a colleague, associate and admirer of William Harvey, who in his latter years became increasingly pugnacious in his opposition to the "neoteriques." Glisson was also an important figure in the London College of Physicians at the same time that Baldwin Hamey was especially prominent; as we have seen above, Hamey

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sanguinem biliosum in omnes ejus partes aequabiliter deponit . . . Estque ista separationis hujus pars *mechanica*. Partes ejusdem *similares*, primario ad hepatis parenchyma, proximo autem loco ad utrunque vasorum auferentium genus spectant. Verisimile equidem est parenchyma e partibus diversimodis constitui; aliasque humorum excernendorum uni, alias alteri conveniere. quo fit, ut humores illi se ipsis intercedit estque istud secretionis eorum initium. Particulae nempe, quibus maxima cum *humore felleo* est familiaritas, eundem ad sese alliciunt, inque pori bilarii extrema capillaria dirigunt. Similiter, particulae illae, quibus cum *sanguine puro* potissimum res est, hunc ad se attrahunt, posteaque ad capillares radicum cavae extremitates deducunt. Interim tamen radices illae, tum pori bilarii, tum venae cavae, similari sua attractione parenchymati in hoc negotio opitulantur "

[71] *Ibid.*, pp. 241 ff., where Glisson rejects the effluvial explanation of magnetic attraction

[72] *Ibid.*, pp. 165-166: "Idque factum videtur, quia bilis in vesicula ista copiose accumulari debuit, ac propterea exitum illius per meatum cysticum plurimum arctari par erat, ope fibrosi anuli, qui in ipsa eorum connexione adnascitur; quemadmodum in proximo capite luculentius constabit. Alias enim, si vesicula fellea (ut saepe fit) ad ejiciendam bilem vehementer extimularetur; quoniam iter rectum est, bilisque copia semper adest, fieri non posset, quin eadem, summo cum partis illius, in quam vas illud fertur, periculo, refluerat. Irritatio enim, fibrarum omnium contractionem adfert; unde idem usu venit, quod a compressione aliter contingeret." Cf. Owsei Temkin's important study, "The classical roots of Glisson's doctrine of irritation," *Bull. Hist Med.*, 1964, *38* 297-328. Temkin, pp. 325-325, concludes that, "Glisson's doctrine of irritation has been shown to be deeply rooted in the classical tradition, which means the teaching of the schools prevailing during the first half of the seventeenth century. . . . Though Galen did not conceptualize 'irritation' as Glisson did, both dealt with the same kind of phenomena in a very similar way." Temkin also points out, pp. 310-311, that Glisson continued to refer to explusive "faculties."

was anything but an undaunted "modernist." Finally, in the 1650s Glisson was also a public "Baconian," and this possibly seemed to him a politically suitable and personally sufficient concession to philosophical modernity.[73] In any case, whatever his precise complex of personal reasons and institutional motivations, Glisson's mental set was the one that dominated the College of Physicians well into the 1660s. With Glisson holding the neoclassical line and with no mechanically-inclined philosopher in its ranks to offer an alternative, the mechanical philosophy hardly entered there into the domain of physiology.

Quite different tendencies were evident at the universities. Here the traditional, medically-circumscribed and scholastic form of physiology continued in part, but new sorts of activity and a new body of physiological ideas, sometimes of the Cartesian kind, were also becoming evident. Research at Cambridge proved limited and evanescent, but by the end of the 1650s Oxford had become the home of a sophisticated group actively pursuing several sets of physiological investigations while being definitely committed at the same time to aspects of the mechanical philosophy.[74]

The Oxford group—including such members as Robert Wilkins, Seth Ward, Christopher Wren, John Wallis, Robert Boyle, Robert Hooke, Thomas Willis and Richard Lower—undertook an extensive series of experiments into all sorts of problems. Sprat specifically mentions chemistry and mechanics in his retrospective *History of the Royal Society* (1667), and in addition there is evidence for investigations into astronomy, pneumatics and optics.[75] Also prominent among the areas of inquiry were matters of deep physiological significance. One principal source for these physiological concerns was William Petty, who was with the group briefly.[76] Another can be traced back to William Harvey, who in the 1640s spent a few years at Oxford during the English Civil War.[77]

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[73] Glisson's role in the College of Physician's public "Baconianism" has been referred to above. Moreover, he scattered several Baconian phrases throughout the *Anatomia hepatis* For example, on p 50 he makes the following telltale remark: "Hic solum addam, (quod ad poros particulatim spectat) si liquor injectus, partis alicujus habitum penetrans, eundem totum *aequaliter* tinxerit; videtur adesse *Instantia Crucis*, quâ indicatur, fluxum illum non fieri per poros, sed per ipsam partis substantiam· sin autem *discretim* innumerabilia duntaxat veluti puncta infecerit; clarum indicium est, poros ibidem reperiri."

[74] Oxford physiology will be described in the pages below. The most important, though transitory work at Cambridge was done by Henry Power. On Power and his research efforts, see Charles Webster, "Henry Power's experimental philosophy," *Ambix*, 1967, *14*. 150-178, and F J. Cole, "Henry Power on the circulation of the blood," *J. Hist. Med.*, 1957, *12:* 291-324.

[75] Purver, *op. cit.* (n. 51 above), pp. 100 ff. & Barbara Shapiro, *John Wilkins, 1614-1672: an Intellectual Biography* (Berkeley: University of California Press, 1969), Chapter 5 There were also suggestions of concern for microscopy, automata, architecture, mathematics, navigation and husbandry.

[76] For Petty, see Charles Webster, "Henry More and Descartes: some new sources," *Brit. J. Hist. Sci.*, 1969, *4*. 359-377.

Once the Oxford experimental group gathered momentum by the mid-1650s, physiological investigations picked up to a considerable extent where Harvey had left off. The composition and functional significance of the blood was much discussed;[78] heartbeat was actively examined by vivisectional techniques;[79] and experiments were even tried on something like transfusion.[80] Robert Boyle, the premier *virtuoso* of the Oxford group, offered evidence in his later writings for earlier Oxonian interest in these matters.[81] Another major concern of Harvey during his Oxford days, respiration, was also frequently examined by members of the Oxford scientific group. Ralph Bathurst, brother of a Harvey associate of the 'forties, was a major figure in Oxford science in the 'fifties; he delivered three lectures on respiration in 1654 as part of the public performance for his M.D.[82]

Robert Boyle picked up these themes a few years later.[83] With the help of his talented young assistant, Robert Hooke, Boyle developed an air pump with which he explored the properties of atmospheric air and the phenomena of fire and respiration.[84] Results of the Hooke-Boyle collaboration at Oxford were reported in 1660 in Boyle's *New Experiments Physico-Mechanicall, Touching the Spring of the Air, and Its Effects.* Among the respiration experiments and conjectures recounted were the following:

> To satisfy ourselves in some measure about the account upon which respiration is necessary to the animals that nature hath furnished with lungs, we took . . . a lark . . . which being put in the receiver, did divers times spring up in it to a good height. The vessel being hastily, but carefully closed, the vacuum pump was diligently plied, and

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[77] For Harvey at Oxford, see Keynes, *op cit.* (n. 31 above), pp 291 ff.

[78] See, for example, T. S Patterson, "John Mayow in contemporary setting," *Isis*, 1931, *15*: p 60 Cf. *Acta Congregationis. Oxon.*, vol. 17 (1648-1659), ff 150r & 151v, e.g , for instances when the composition of the blood was publicly discussed

[79] Robert Boyle, *Works* (London, 1772), vol. 2, p. 69; this is a passage from *Some Considerations Touching the Usefulness of Experimental Natural Philosophy,* which Boyle wrote in sections from about 1650 onwards and published in 1663

[80] See Purver, *op cit.* (n. 51 above), p. 117 & Shapiro, *op. cit.* (n. 75 above), p. 133.

[81] See n. 79 above as an example.

[82] For Ralph's brother George Bathurst as a definite Harvey associate, see Keynes, *op cit* (n. 31 above), pp 312-313 Ralph Bathurst's "Praelectiones tres de respiratione. Habitae in Schol. Medicin. Oxon A.D. 1654 Pro gradu M.D." are published in Thomas Warton's *The Life and Literary Remains of Ralph Bathurst, Literary Remains* (London, 1761), pp. 127-210.

[83] There is good evidence that Boyle in fact knew Bathurst's lectures, even though they were delivered before he settled in Oxford; see T. Warton (n. 82 above), *Life*, p. 163

[84] Marie Boas, *Robert Boyle and Seventeenth Century Chemistry,* (Cambridge: Cambridge University Press, 1958), pp. 43-44.

> the bird for a while appeared lively enough; but upon a greater exsuction of the air, and very soon after was taken with as violent and irregular convulsions, as are wont to be observed in poultry, when their heads are wrung off . . . .[85]

The gentleman-amateur Boyle, without medical degree or formal institutional position, was clearly pushing out the boundaries of respiratory physiology, a subject to which William Harvey, Royal and Collegiate physician, had given the greatest prior impetus.

In addition to Harveian concerns, however, other physiological problems likewise drew consistent attention from the Oxford scientists. Among the most important of these were questions of neurological control and cerebral function. Ralph Bathurst, Robert Boyle and a group led by Thomas Willis were the Oxonians most intimately involved with these matters. Bathurst introduced neuro-cerebral issues in his lectures on respiration,[86] and Boyle included extensive reports of vivisectional experiments among his physiological observations.[87] Boyle's activity notwithstanding, it was Thomas Willis who explored neuro-cerebral function most extensively.

Willis was a physician who had spent several productive years in Oxford as a medical practitioner and an eager general scientist.[88] After receiving his M.B. degree in 1646, he associated with Petty, Wilkins and other members of the Oxford scientific circle in the early 1650s. Later in the 'fifties he established a close relationship with Richard Lower, a talented younger man who served him much as Hooke served Boyle. Willis also enjoyed the scientific services of Christopher Wren, Savilian Professor of Astronomy after 1661, and profited from the friendship of Thomas Millington, M. D. A professed Anglican in Puritan Oxford, Willis had to await the Restoration before receiving a professorial appointment. He was rewarded in August 1660 when university authorities designated him Sedleian Professor of Natural Philosophy. In October he received his Oxford M.D.

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[85] Boyle, *Works, op. cit.* (n. 79 above), vol 1, p. 97.

[86] Bathurst, *op. cit.* (n. 82 above), *Literary Remains*, pp. 166-167: "Saepe etiam occurrunt et alii, qui quamvis e lecto non surgant, brachia tamen et pedes in somnis jactant, tanquam luctantes aut cursitantes; clamant, rident, plorant, disputant, versus faciunt, et quid non? Atque haec omnia praestant motu musculorum et spirituum, longe magis difformi et vario quam ad respirationem requiritur. Haec tamen voluntarie fieri quis negat? Cum tantillum ad voluntatem intersit, sive a phantasiae sive intellectus dictamine ad actiones obeundas incitetur Et verisimile quidem est, animam adeo nunquam opus suum intermittere, ut ne in somnis quodem non semper aliquid meditetur: Novi enim aliquammultos, qui licet, mane experrecti, nihil se somniasse dejerabunt. postquam tamen diurnarum rerum occursus memoriam ipsis refricaverit, multorum reminiscuntur quae per somnium ante cogitabant. Adde quod in affectibus ubi cerebrum laborat, tanto semper deterius se habet respiratio, quanto plus a voluntatis imperio decedit. Hinc phrenetici rarum et magnum et inordinatum spirant; quia nempe mens jam effraenis ad alia acrius fertur, actionum vero animalium fere obliviscitur, ita ut neque cibum capere curent; neque alvum aut vesicam levare, nisi plurimum urgente necessitate, sustineant Utile, autem hic fuerit ut, cum Galeno, notemus voluntariorum operum alio quidem liberiora esse; alia vero affectibus corporis quodammodo inservire . . ."

[87] See, for example, Boyle, *Works, op. cit* (n. 79 above), vol. 2, pp 69-70: " . . . having opened . . . [a frog] alive, and carefully cut out his heart, without closing up the orifice of the wound . . the frog notwithstanding leaped up and down the room as before, dragging his entrails (that hung out) after him; and, when he rested, would upon a puncture leap again, and being put into the water, would swim, whilst I felt his heart beating betwixt my fingers."

[88] For biographical details here and below on Willis and his associates, see Hansruedi Isler, *Thomas Willis* (New York: Hafner, 1968), pp. 3-44

In the years following his appointment as Sedleian Professor Willis organized Lower, Wren and Millington into something like a research team that helped him produce a large, general and serious work on neurocerebral physiology. Published early in 1664 as *Cerebri anatome,* the book covered approximately two hundred and fifty octavo pages of Latin text and was illustrated with plates contributed by Wren and Lower. It is organized into two main sections.[89] The first, about two-thirds of the book, is "The Anatomy of the Brain"; the second is "The Description and Uses of the Nerves." Both sections are anatomically founded but functionally oriented.[90] Among Willis' principal contentions about function—all based on carefully presented if later questioned anatomical evidence—are that the animal spirits are manufactured from arterial blood in the cerebral and cerebellar cortex by a distillation-like chemical process;[91] that the wavelike movement of the animal spirits in various medullary tracts is the immediate cause of mental and sensory acts;[92] and, specifically, that inwardly directed undulations of the spirits are associated with sensation, whereas outwardly directed ones (i.e. from the convolutions of the cerebral cortex through the medullary substance of the cerebrum and out into the medulla oblongata and the nerves) are connected with memory, will and appetite.[93] Willis also discusses the vessels of cerebral circulation, describes in intricate detail the distribution of the principal nerves, and offers hypotheses for the manner of muscular motion.[94]

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[89] On editions of *Cerebri anatome,* see H. R Denham, "A bibliographic survey of *Cerebri anatome,*" in *Thomas Willis. The Anatomy of the Brain and Nerves,* ed. William Feindel, vol. 1 (Montreal: McGill University Press, 1965), pp. 67-83.

[90] For more extensive accounts of the content and principal conclusions of *Cerebri anatome,* see Feindel, "The contents of *Cerebri anatome,*" in Feindel, *op. cit* (n. 89 above), pp 48-59; Isler, *op cit.* (n. 88 above), pp. 88-112; and Christopher Duncan Bown, *Mid-Seventeenth Century Natural Philosophical Thought about the Brain,* Princeton University Senior Thesis (Princeton, 1973). See also Alfred Meyer and Raymond Hierons. "On Thomas Willis' concepts of neurophysiology," *Medical History,* 1965, *9.* 1-15, 142-155 and K D. Keele, "Thomas Willis on the brain," *Medical History,* 1967, *11* 194-200.

[91] Feindel edition (n. 90 above), vol 2, pp. 87-88.

[92] *Ibid.,* p. 91.

[93] *Ibid.,* p. 96.

Perhaps Willis' most important concern, however, was to distinguish between voluntary and involuntary functions in man and in the different behaviors of higher and lower animals. To make these distinctions, Willis differentiated sharply between the respective roles in the "animal oeconomy" of the cerebrum and the cerebellum. The cerebrum is the prime seat of the rational soul in man, of the sensitive in animals; it is also the source of voluntary movements and ideas and the locus of imagination, memory and thought. By contrast, the cerebellum controls and regulates—through its separate store of animal spirits and attendant nerves—automatic and involuntary motions such as heartbeat, respiration and peristalsis.[95] There are some actions, however, which are mixed or crossed. That is, extreme mental agitation may affect the heartbeat or breathing pattern, and unusual discomfort in the digestive tract may afflict the mind. Willis explains:

> The reason of these is, partly because the Nerves of either Government communicate variously among themselves with shoots sent forth one to another, so that often times the offices of the one are drawn into the parts of the other. But besides . . . the sensible impression being inflicted on the parts of the involuntary Function, forasmuch as it is vehement, like a strong waving of waters, passing through the Cerebel, affects the Brain it self. In like manner it may be thought concerning the motion which belongs to those parts, *viz.* that made after the ordinary manner, that it is performed by the command of the Cerebel. Notwithstanding some more severe Edicts of the Brain, by the by-passage of the Prominences, belong also to the Cerebel, and determine the Offices of the Inhabitants of it to be performed at the beck of the Appetite.[96]

On this basis Willis could explain both reflexive action in man and the characteristic differences between men and lower animals. Unconscious reflex behaviors occur in man when moderately strong incurrent undulations of the animals spirits are reflected into a motor pathway before they reach the cerebrum and, thus, consciousness.[97] In lower animals, the cerebellum and other parts of involuntary function are invariably well developed in comparison with the cerebrum and its nerves, so that almost all functions of such animals can be performed automatically with the greatest efficiency.[98]

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[94] *Ibid*, pp 129 and 135 for the latter.

[95] *Ibid*, p. 111.

[96] *Ibid.*, p. 120, cf pp. 108-109

[97] *Ibid.*, p. 115 for afflictions in the praecordia or viscera which are automatically regulated in the cerebellum Cf. p 118 for the automatic erection of the ears and widening of the eyes on hearing a sudden noise.

[98] *Ibid.*, p. 112.

The difference between man and lower animals therefore reduces to a matter of differential organization and complexity in their brains and nervous systems. The behavior of brutes, largely automatic, can be considered machine-like; man, with his unusually well-developed cerebrum and intricate interconnections between the voluntary and involuntary control systems, is a far more complex contrivance. Thus,

> . . . Brutes are like Machines framed with a more simple furniture and with less workmanship, and therefore furnished with a motion of one kind only, or determined for the doing still the same thing. But in Man divers series of motions, and as it were complications of wheels within wheels, appear.[99]

This passage is extremely interesting. For it not only conveys Willis' meaning with apt language and particular forcefulness, it is also representative of several other mechanical metaphors scattered throughout *Cerebri anatome* and derived from Descartes. Thus, when Willis wishes to describe the recurrent regularity of those actions under cerebellar control, he uses the following verbal device:

> . . . the Spirits inhabiting the Cerebel perform unperceivedly and silently their works of Nature without our knowledge or care. Wherefore whilst the Brain [Cerebrum] is garnished as it were with uncertain Meanders and crankling turnings and windings about, the compass of this [Cerebellum] is furnished with folds and lappets disposed in an orderly series; in the spaces of which, as in designed Orbs and Tracts, the animal Spritis are expanded according to the Rule and Method naturally impressed on them. For indeed those in the Cerebel, as it were in a certain artificial Machine or Clock, seem orderly disposed after that manner within certain little places and boundaries, that they may flow out orderly of their own accord one series after another without any driver, which may govern or moderate their motions.[100]

This, unmistakably, is Descartes' vocabulary. Although, later in the text, Willis makes only oblique and passing reference to Descartes' "Doctrine of the Passions of the Soul," this passage and many others can only be understood in the context of Willis' undoubted familiarity with and warm praise for Cartesian physiology.[101]

Willis' use of Cartesian analogies and metaphors, however, is not quite so dominant a feature of *Cerebri anatome* as the two passages above would seem to suggest. For though he does rely on mechanical (and chemical) references at certain important places, most of his text, unlike Descartes' *Passions of the Soul,* is taken up with the detailed description of the parts, reports on human dissections, references to comparative anatomy and occasional allusions to vivisection experiments.[102] Indeed, Willis even stops to note explicitly that comparative anatomy proves the pineal gland to be merely a gland like any other, and unable to serve the high hypothetical purposes Descartes assigned to it.[103] Moreover, while Willis turned all his investigative techniques to the purpose of functional explication, he defines and derives the essential categories of function by other than mechanical means. That is, he bases the fundamental differences between voluntary and involuntary action, which play so large a role in *Cerebri anatome,* on generalizations from manifest behavior and on long-standing medical and physiological distinctions, rather than on newly-minted mechanical language and references. Willis therefore uses Descartes-like mechanical analogies to enhance certain meanings and to sharpen a few distinctions, but he readily acknowledges that these meanings and distinctions existed both logically and chronologically prior to the invention of the mechanical philosophy.

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[99] *Ibid.*, p 162

[100] *Ibid.*, p. 111

[101] For the reference to Descartes' "Doctrine of the Passions of the Soul," see Willis, *ibid.*, p. 162. For one of Willis' explicit, contemporary references to Descartes, one of the "most Learned men," see "Of Feavers" (1659) in Thomas Willis, *The Remaining Medical Works* (1681), p. 65.

What characterized Willis' work likewise characterized that of other Oxford physiologists. Robert Boyle, who clearly knew Descartes' physiological writings, employed mechanical allusions strikingly similar to Descartes' as an occasional interweave in his otherwise-inspired and directed physiological essays and investigations. As one example, Boyle pauses briefly in his *Usefulness of Experimental Natural Philosophy* to offer the following reflections:

> . . . if chymical experiments and mechanical contrivances were industriously and judiciously associated by a naturalist profoundly skilled in both, and would make it his business to explain the phenomena of a human body . . . divers of them [phenomena] . . . might be by artificial engines (consisting as the pattern not only of solid, but liquid and spirituous parts) not ill represented to our very senses: since a human body it self seems to be but an engine, wherein almost, if not more than almost, all the motions common to men with other animals are performed mechanically.[104]

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[102] For vivisection experiments, *ibid.*, p. 152.

[103] *Ibid.*, p. 106: "Below the Chambers of the Optick Nerves in a common Valley which lyes between the tops of these and the Buttock-form Prominences, is placed the Pineal Glandula or Kernel in form of a Pine-apple, called also *Conarum;* this is not only found in Man and four-footed beasts, but Fowls and Fishes also are endued with the same. Wherefore, although from hence it may be concluded, that this is of necessary use; yet we can scarce believe this to be the seat of the Soul, or its chief Faculties to arise from it; because Animals, which seem to be almost quite destitute of Imagination, Memory, and other superior Powers of the Soul, have this Glandula or Kernel large and fair enough."

[104] Boyle, *Works, op. cit.* (n. 79 above), vol 2, pp. 75-76. See n. 106 below for one of Boyle's explicit references to Descartes.

After these momentary musings, Boyle quickly returns to his detailed, largely uninterpreted reporting of anatomical and chemical experiments. Thus, like his fellow Oxonian Willis, Boyle showed a familiarity and receptivity to the Cartesian mechanical philosophy, but, at least in his early work, he confined his unmistakable references to the periphery instead of allowing them to enter the central core of his physiology.

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Why this dual attitude towards the Cartesian mechanical philosophy in the physiology produced at the English universities in the 1650s and early 1660s? Several explanations are possible. First, there is the momentum of tradition and the phenomenon of historical staging. However different their work may have been in certain respects from that of their predecessors and contemporaries, Boyle and Willis both owed something to the neoclassical tradition of medicine and physiology. Unlike Boyle, Willis had a university M.D., and this meant that he had spent several years absorbing the essential ideas and explanatory assumptions of Galen, Fernel and similar authors. But even Boyle, though not an M.D., was widely read in Galen and the neoclassical writers, as many references in his works make clear.[105] Immersion in or, at least, thorough exposure to these traditions worked to assure that the mechanical philosophy would not suddenly, at its very introduction, occupy center stage in productions of men deeply conscious of prior tradition and to some extent carried along by its momentum. Secondly, Harvey's work also had a considerable influence over all four men in question, and Harvey was anything but a force pushing in the direction of the mechanical philosophy. Indeed, the more closely one studied Harvey's works and pursued his methods of comparative anatomy and vivisection, the more one tended to question the simple physiological mechanisms of Descartes. This was apparent in Boyle's investigations of heartbeat.[106]

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[105] See, for example, *Works*, vol 2, p 52: " . . when he [Galen] comes to consider particularly the exquisite structure of a human body . . [he] breaks forth into very elevated, and even pathetical celebrations of God, and tell[s] us, that in his books, *De usu partium*, he composed hymns to the Creator's praise."

[106] See, for example, Boyle, *Works*, *op cit.* (n. 77 above), vol. 2, p. 69. "If you dexterously take out the hearts of vipers, and of some smaller fishes, whose coldness make them beat much more unfrequently and leisurely, than those of warm animals, the contraction and relaxation of the fibres of the heart may be distinctly observed, in order to the deciding or reconciling the controversy about the cause and manner of the heart's motion, betwixt those learned modern anatomists that contend, some of them, for Dr. *Harvey's* opinion' and others, for that of the Cartesians. Towards satisfying my self in which difficulty, I remember, I have sometimes taken the heart of a flounder, and having cut it transversely into two parts, and pressed out, and with a linnen cloth wiped off, the blood contained in each of them, I observed, that for a considerable space of

A third possible explanation is that the philosophical influences affecting Boyle, Willis and others were quite complex and somewhat mixed. William Petty, for example, was a major philosophical source for the Oxonian physiologists. This former associate of Thomas Hobbes almost certainly introduced Descartes-like mechanical ideas along with his experimental and anatomical concerns. But Petty, as some recently published letters make clear, was careful to balance interest in Cartesian principles with the highest regard for experiment.[107] Within physiology, enthusiasm for experiment meant central respect for anatomy and vivisection and only qualified, if nonetheless definite, interest in mechanical hypotheses.

Fourthly, there may have been a political motive, whether conscious or unconscious, that contributed to the mixed attitude towards mechanism we have seen in the Oxonian physiologists. For the university scientists had to walk a narrow tightrope in order to distinguish themselves from their recent, bitter and radical critics while not appearing reactionaries themselves. As has been suggested earlier, the militant university critics of the Puritan Revolution had expressed unqualified and largely thoughtless support for the new ideas of Descartes and other mechanical philosophers.[108] Members of the beleaguered Oxford community, by contrast, would have to express some interest in the new ideas or seem hopelessly out-of-date. Yet at the same time they could moderate heated enthusiasm with cool reflection. To endorse but simultaneously to qualify the new ideas, and by doing so to exhibit their critics as uneducated and foolish men, was the effective strategy John Wilkins and Seth Ward adopted in 1654.[109] Their university

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time, the severed and bloodless parts held on their former contraction and relaxation. And once, I remember, that I observed, not without wonder, that the severed portions of a flounder's heart did, not only after the blood was drained, move as before, but the whole heart observed for a pretty while such a succession of motion in its divided and exsanguious pieces, as I had taken notice of in them, whilst they were coherent, and as you may with pleasure both see and feel in the intire heart of the same fish."

[107] Charles Webster, "Henry More and Descartes," *op. cit.* (n. 76 above), pp. 367-369.

[108] See above for general context, and for one example of "unqualified and largely thoughtless support," see John Webster, *Academiarum examen* (London, 1654), pp. 104-107 *passim:* "That the *Galenical* way of the medicinal part of *Physick* . . . may not be the prison that all men must be inchained in, and ignorance, cheating and impostorage maintained by Lawes and Charters; but that the more sure, cleer and exquisit way of finding the true causes, and certain cures of diseases, brought to light by those two most eminent and laborious persons, *Paracelsus* and *Helmont,* may be entertained, prosecuted and promoted . . . . The next is *Metaphysicks,* to help which I shall only offer this expedient. that it might be reduced to some certain grounds and principles . . . and so that the most sure way of *Renatus des Cartes* may be brought into use, and exercise. . . ."

[109] The Wilkins/Ward strategy is apparent in their rejoinder to Webster, *Vindiciae Academiarum* (Oxford, 1654). For a developed version of this "cooptation" model of university response to its 1650s critics, see my

colleagues and associates could then easily follow their lead in physiology, which is just what they seem to have done. They could pay homage to the mechanical philosophy, but unlike some of their critics they would not become its witless devotees.

Finally, it is even possible that popular opinion and common theology contributed to restraining enthusiasm for mechanism within the physiology produced at the universities. The 'fifties and 'sixties were the first decades of outraged reaction to the recently published views of Thomas Hobbes.[110] The author of *Leviathan* (1651) and *De corpore* (1655) was immediately castigated for his atheism, anticlericalism, ethical relativism and determinism. At the heart of all these intolerable views, it was believed, lay Hobbes' materialism, according to which there was no place for immortal and immaterial spirits anywhere in the universe. In the body of man there were only matter and motion, for sensation, memory and thought were mere "corporeal motions."[111] University men played a major role in responding to Hobbes. Henry More of Cambridge wrote *Antidote Against Atheism* (1653) and *The Immortality of the Soul* (1659) largely against Hobbes.[112] Seth Ward of Oxford wrote *A Philosophical Essay Towards an Eviction of the Being and Attributes of God* (1655) and *In Thomae Hobbii philosophiam* (1656) likewise against Hobbes.[113] Even Robert Boyle, in an essay on physical matters of 1662, *An Examen of Mr. T. Hobbes his Dialogus Physicus De Natura Aeris,* paused to reflect that,

> it might possibly prove some service to higher Truths than those in Controversie between him and me to shew that in Physicks themselves his Opinions, and even his Ratiocinations, have no such great advantage over those of some Orthodox Christian Naturalists.[114]

In this climate it is hard to imagine that other university men would thoughtlessly and unqualifiedly speak of the "corporeal motions" within human and animal machines; instead, they would have to worry about public reaction and the unintended identification of their views with Hobbesian materialism. This, no doubt, was still another reason why Willis and Boyle

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"The Rise of Baconianism in Seventeenth Century England," in the forthcoming *Festschrift* for Edward Rosen to be published by the Polish Academy of Sciences.

[110] For an excellent general account, see Samuel I. Mintz, *The Hunting of Leviathan* (Cambridge University Press, 1962).

[111] *Ibid.*, p. 63 ff.

[112] *Ibid*, p. 84 ff.

[113] *Ibid.*, p 55.

[114] Quoted *ibid.*, p 87.

showed something less than unabashed support for mechanism within the animal oeconomy.

Conclusion

The net result of the varied and sometimes cautious reactions we have traced in the 'forties, 'fifties and early 'sixties was a multiply ambivalent incursion. Throughout the 'forties new attitudes towards physiological explanation generally made little headway. In the 'fifties, in the College of Physicians, Francis Glisson kept the mechanical philosophy beyond the pale. He retreated to "similary attraction" as a way of defending neoclassical physiology from the invasion of philosophical modernity. In the universities, leading students of the animal oeconomy granted a definite place to mechanism within physiology. They refused to retreat or retrench in the face of Descartes, and they either gently substituted chemical and mechanical metaphors for the old "faculties" or simply ignored the need for explicit explanation. Yet even within university physiology enthusiasm was qualified to a considerable degree, and the role of the mechanical philosophy was carefully circumscribed. But however complex, an initial reaction to mechanism within the animal oeconomy had been formulated.

Over the next twenty years this initial, ambivalent reception was to change to a fuller, less qualified enthusiasm. Science and the circumstances of its practice were to alter markedly, and in these altered conditions physiology changed too. The path was cleared for full invasion, and once the territory was open the mechanical philosophy would remain entrenched for at least sixty years.

THE PEOPLE'S HEALTH: MEDICAL THEMES IN 18th-CENTURY FRENCH POPULAR LITERATURE*

WILLIAM COLEMAN

Popular literature poses as great a problem as the people's health. Were we to inquire into the medical condition of, for example, Voltaire, a celebrated hypochondriac, or seek to establish the pattern of health and disease in the French royal family during the latter years of the old régime, we would enjoy an inestimable advantage. Here written records exist pertaining to definite personalities. To be sure, these materials are incomplete and, more importantly, because of changing medical terminology and understanding, are to be interpreted with great caution. Nonetheless, they often do exist and by existing give witness to two cardinal facts: the direct availability of medical literature to the upper orders of society, and access by the same to trained medical personnel.

It is notorious, however, that such literature was well beyond the ken of the great mass of society and that formal medical attention for most of the lower orders was virtually non-existent. Yet these so-called lower orders, comprising in late eighteenth-century France perhaps ninety per cent of a population estimated minimally to have been twenty to twenty-two million, also lived, took sick and died. They, too, constituted a potential medical clientele but one which, in the large, was not to be satisfied by the regular medical trades. Recent years have turned historians, folklorists and anthropologists increasingly towards this world of peasants, vagabonds, villagers and naissant urban proletariat.[1] Social history has taken hold of such persons by means of

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[1] Representative studies are presented in *Biology of Man in History*, ed. R. Forster and O. Ranum, trans. E. Forster and P. M. Ranum (Baltimore: Johns Hopkins Univ. Press, 1975). Comprehensive assessment of rural problems is given by O. H. Hufton, *The Poor in Eighteenth-Century France, 1750-1789* (Oxford. Clarendon, 1974); on urban manifestations see J. P. Gutton *La société et les pauvres L'example de la généralité de Lyon, 1534-1789* (Paris: Société d'édition "Les Belles Lettres," 1971). The demographic situation, an object of intensive recent study, is concisely assayed by Goubert in *Histoire économique et sociale de la France*, ed. F.

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reconstituted vital statistics, demographic movement, price levels, employment opportunity, patterns of land tenure and exploitation, available diet and the like. The incidence of disease among this population has also been explored.

Such studies hold the promise of providing a far clearer view of the hard realities of man's biological existence in pre-industrial Europe. They hold the promise of revealing the essential condition of a nation's health and the prospects of its potential medical consumers. These studies derive principally from parish records, official reports, oral and written accounts of popular belief and practice, and assorted medical materials. They do not draw directly upon the experience of the poor as recorded by the poor, for the simple and sufficient reason that this ninety per cent of the French population was itself overwhelmingly illiterate. The social historian thus constantly faces the dilemma of recreating the lived experience of a large mass of mankind which left only minor written testimony to its experience.

The poor in France appear nonetheless to have possessed a literature peculiarly their own. Distributed by peddlers, and presumably read aloud to assembled listeners by the occasional lettered person of the farm or village, this was the celebrated *bibliothèque de colportage*. Here was a truly popular literature. It was designed to appeal to the common people and it most certainly did so, if the asserted production figures of thousands, hundreds of thousands, even millions of copies are to be accepted. I shall examine below the medical bearings of this popular literature. Erwin Ackerknecht has emphasized that the history of medicine has traditionally been written from the perspective of the physician or the learned observer.[2] This has had the effect, he suggests, that we continue in ignorance regarding both concrete matters of medical practice and the actual medical experience of the profession's clientele. It may be added that we are little better informed regarding medical knowledge, if any, available to and utilized by those large segments of the population left unserved by regular physician or surgeon, such professionals enjoying the witness of at least rudimentary formal medical training. One approach to the medical awareness of the poor is through popular literature. It promises to provide one measure—and there are others—of the scope and special terms of the medical advice available to the lower orders of society.

Braudel and E. Labrousse (Paris: Presses Universitaires de France, 1970), II, 9-84. A splendid summary statement of current questions and conclusions is provided by J P. Gutton, *La société et les pauvres en Europe (XVI*^{e} *- XVIII*^{e} *siècles* (Paris: Presses Universitaires de France, 1974)

[2] E. H. Ackerknecht, "A plea for a 'Behaviorist' approach in writing the history of medicine," *J Hist. Med.*, 1967, 22. 211-214.

This literature at best presented a highly traditional and debased view, often a grotesquely debased view, of the doctrines of orthodox or academic medicine. It provided, moreover, a distinctive and, in my opinion, necessary appreciation of the medical objectives and capacities of the poor. Their lot demanded that they seek not health, an abstract good accessible only to the leisured, but efficacious means to restore the ailing when stricken. In essence, therefore, medical advice in popular literature was activist, not expectant.

French popular literature of the eighteenth century is characterized by several features, first among which are format, printing history and mode of distribution.[3] Most items in the collection—some ten thousand titles have been claimed and firm evidence exists for perhaps fifteen hundred—are miserable scraps of crude printing on coarse paper. Hard bindings were rarely if ever employed and most titles were issued in rough wrappers of blue, from which has probably arisen the vernacular designation for the entire genre, the *bibliothèque bleue*. These were frequently compilations or extracts; authorship was unstated. Format varied but was usually small; pages varied enormously in number. Compactness was at a premium, for these were peddler's wares, to be carried on the back along with other merchandise, notably the small necessities of sewing. The *bibliothèque bleue* was first issued towards

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[3] The essential introduction to the *bibliothèque bleue* is R. Mandrou, *De la culture populaire aux 17^e et 18^e siècles La bibliothèque bleue de Troyes* (Paris: Stock, 1964), supplemented by the important studies of G Bollème: *Les almanachs populaires aux XVII^e et XVIII^e siècles. Essai d'histoire sociale* (Paris—The Hague. Mouton, 1969); *La bibliothèque bleue La littérature populaire en France du XVI^e au XIX^e siècles* (Paris Julliard, 1971) [an anthology of texts plus introduction and commentary]. See also Bollème's "Littérature populaire et littérature de colportage au 18^e siècle," in *Livre et sociètè dans la France du XVIII^e siecle*, ed. F. Furet (The Hague: Mouton, 1965), I, 61-92. An indispensable research tool is the recent *Catalogue descriptif de la bibliothèque bleue de Troyes (Almanachs exclus)* by A. Morin (Geneva: Droz, 1974). From the voluminous older literature must be cited the querulous C. Nisard, *Histoire de livres populaires ou de la littérature de colportage* (Paris, ed. 2, 1864), 2 v. Mandrou and Bollème (*Almanachs*) list additional studies.

I have consulted items in the *bibliothèque bleue* principally at the library of the Musée national des arts et traditions populaires and the Municipal Library at Troyes; also some items at the Bibliothèque nationale. Given the frequent absence of bibliographical clarity in the citation of these works, and the fact that it is difficult to be assured that any two copies bearing the same or similar titles are typographically or textually identical, I follow Bollème's practice of indicating by library (ATP—Musée national des arts et traditions populaires, BMT—Bibliothèque muncipale de Troyes; BN—Bibliothèque nationale) and *cote* the particular copy which I have examined. Where applicable I include the appropriate number from Morin's catalogue of the *publications troyennes*.

The notion of *populaire* is central to an appreciation of this literature. On the one hand, popular literature may suggest a literature composed by the people, an oral tradtion recorded by the written word; on the other hand, the expression refers to works published for a popular audience but whose contents most probably are derived from the learned world Medical themes in the *bibliothèque bleue* clearly fall into both categories. For a discussion of the general issue, see M. Soriano, *Les contes de Perrault* (Paris: Gallimard, 1968), 479-491. On the search for historical assessment of the thought and value-systems of large groups, see G. Duby "Histoire des mentalités," *L'histoire et ses méthodes*, (Paris: Gallimard, 1961), 937-966.

1650 in Troyes, an important city in the cloth trade, and this city remained the outstanding center of production until the demise of the collection in the 1850s under the dual assault of a new, cheap popular press and the censorship of the Second Empire. The pages of each copy were usually secured by a loop of thread passed through the fold, producing thereby a flimsy semblance to a small volume. Time and ungentle readers have treated the *bibliothèque bleue* severely. In short, items in the *bibliothèque bleue* were indeed ephemeral, being startlingly low in price (a sou or thereabouts), no objects of beauty to a collector, and subject to ready destruction through cheap fabrication and intense use. The initial problem for close study of this literature has been, quite literally, gaining access to it.

Robert Mandrou, fortunately, has surveyed this diffuse corpus with an eye to its more general features.[4] Probably the most distinctive writings within the collection are portrayals of the noble ideal. Some forty titles were devoted to relating the great romances of the later medieval period. These focused upon the heroic deeds of Charlemagne, his companions and descendants. Behind a record of adventure, mayhem and death, they celebrated the ultimate triumph of Christian Europe over the threatening Saracen. Such romances—Huon of Bordeaux, the Four sons Aymon, Valentin and Orson and others—had disappeared from the regular book trade but obviously constituted the persistent core of the historical mythology by which the French rural masses defined their nation.

Works of pious intent occupied a major place in the collection. Here a broad spectrum of possibilites was available. Gatherings of canticles and catechisms and books of devotion occur frequently. The lives of the saints were often recorded, including a significant number of healing saints. All of these works proffered a very direct Christianity. Their inspiration was the New Testament; their repeated exemplar the birth and passion of Christ; their common message our frailty and sinful ways and the certainty we face of stern and final judgement.[5] Such works were naturally congruent with the devotion of their readers and auditors.

A third major division of the collection embraces the domain of the occult. Apparently few in numbers, they seem not to have been so in popularity. Here one meets "secrets" without end and, repeatedly, that celebrated name, Albert. In the *bibliothèque bleue* circulated numerous editions of the *Grand Albert* and the *Petit Albert*, each a stupefying assemblage of pronouncements, predictions, formulae, operations and miraculous effects. Here, too, is found a sizeable prediction literature, ranging from the barest simplicity of a calendar to disorganized but diverting compendia of anticipations of all manner of untoward events. Perhaps here also may be listed the almanach, one the most striking, useful and best studied components of the entire *bibliothèque bleue*. The early almanach stands astride a line separating the real and the magical. Its roots, of course, are astrological. The stars maintained their sway throughout the eighteenth century but did so apparently with diminished authority, as may be seen from almanachs devised for an urban audience.

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[4] Mandrou, *De la culture populaire*, *passim*.

[5] *Ibid.*, 77-78.

All in all, as Mandrou indicates, the *bibliothèque bleue* perpetuated the values of old France into just those years, ca. 1650-1850, when a radically new nation was coming into being. A well-ordered society, clearly divided into levels of wealth, power and privilege; respect for and submission to authority, secular or religious; unquestioning adherence to Gospel Christianity and a keen awareness of the possibility of sin and its fearsome consequences; a sense of wonder and perhaps no little anxiety before the overwhelming forces of nature—such traits were declared by the recently-arrived *philosophe* and his sympathetic audience to belong to another era. They were inappropriate, his polemic proclaimed, to an enlightened France, to a nation whose economic order and social forms were beginning to change and change radically. Yet the *bibliothèque bleue* survived the Revolution and attained its greatest popularity in the early nineteenth century. Quite obviously, its audience was not one of entrepreneur and advanced thinker.

A mention of audience raises the most difficult question regarding the history and significance of the *bibliothèque bleue*. Who read these books? Who could read them? Recent study of the collection has begun on the premise of a truly popular audience, probably rural and presumably illiterate. Acting upon this premise one examines the collection as an expression of the mentality of the masses. Not unexpectedly, one finds that the *bibliothèque bleue* expresses just this mentality. Unfortunately, no other procedure seems available: one imputes, whatever the risks, which are many, readership from text. Lacking other relevant evidence we simply state and develop the consequences of our premise. While it is probable that the *bibliothèque bleue* reached a popular audience, that audience, the "people," was not, even in eighteenth-century France, an undifferentiated mass. It may be a safe guess that this literature reached the rural small holder (*laboreur*) and his dependents and various personages of village and town. There are indications, too, that the *bibliothèque bleue* found readers in the popular quarters of the growing cities. Almost certainly the consumer of the *bibliothèque bleue* would not have access to the regular book trade, whose cheapest editions were not, relatively speaking, inexpensive. Users of the latter, drawn from the upper reaches of society, it is known disdained these popular publications. In the following discussion I have generally followed the conventions of others who have consulted this literature. Items which obviously do not belong in the *bibliothèque bleue,* despite their inclusion by some students, will not be exploited (see note 7 below). Moreover, various other items which I and others do utilize must be regarded with suspicion, or at least suspended judgement. So long as there are doubts concerning the true limits of the *bibliothèque bleue* it will be premature and precarious inflexibly to infer readership from text.[6]

Where, within this vast miscellany, are found texts treating medical themes? Practical treatises, including those of a medical nature, are quite rare.[7] Instead, medical advice of one kind or another is found scattered throughout the collection. Almanacs provide a primary source. Catechisms and the lives of the saints offer occasional advice and frequent admonition. There is much in the prediction literature and, of course, astrological and magical texts often bear on questions of health and disease. Death is a recurrent theme in this literature and has its special relationship to medical affairs. To my knowledge, only two items in the extant collection are explicitly and fully devoted to medicine, the first being a highly simplified herbal and the other a much reduced French version of the School of Salerno.[8] Both date from the mid-eighteenth century.

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[6] The paradox must be faced directly although at present it cannot be resolved in a satisfactory manner: all studies of the *bibliothèque bleue,* including my own, accept the "fact" that this literature, conveying and conveyed by the written word, reached an illiterate audience. How could it be so? Mandrou (*De la culture populaire,* 18) explains the matter as follows It was the custom in France to gather in family or communal groups each evening after work; this was especially true in winter and occurred in both town and country. The consumers of the *bibliothèque bleue* were probably constituted by this audience "It is within the framework of this evening gathering *(veillée)* that we must situate the public reading of the *bibliothèque bleue;* everywhere, even in the poorest village, there would always be found someone, a curate, verger, or soldier returned home, who knew how to read " The *bibliothèque bleue* would thus function on several levels: read by some; heard by others; repeated orally, with frequent distortion, beyond the *veillée.* This solution of the problem might well hold for certain texts in the *bibliothèque bleue* (romances, lives of the saints) but seems altogether improbable for others (herbals, almanacs). There exists also the possibility that this literature passed only into the hands of the literate. If true, then all analysis thereof as an index to the thinking and belief of *le menu peuple* would be seriously threatened. The possibility, however, is that and no more; virtually all other evidence pertaining to the production, distribution and consumption of the *bibliothèque bleue* denies it.

[7] A major trilogy, *Le médecin charitable* (BMT B. bl. 46, Morin 750), *L'apoticaire charitable* (BMT B. bl. 706, Morin 22) and *L'opérateur des pauvres* (BMT B. bl 707, Morin 850), all from the seventeenth century, and a double volume of the 1750s, *La médecine et la chirurgie des pauvres* (ATP 1°R 465), have commonly been included in the *littérature de colportage* Their format (full calf binding), probable price and, notably, editorial expressions make this exceedingly unlikely *The Charitable Apothecary* (3) writes for an audience embracing religious communities, "great families," lords and ladies of town and country, military men and students of medicine, the *Charitable physician,* while offering much instruction regarding the preparation and use of drugs, stresses (iii) that the reader do nothing without the advice of a physician. Such views are hardly consonant with the capacities of the presumed audience of the *bibliothèque bleue*. In fact, these works constitute reeditions of and derivations from the great charitable works of seventeenth-century physicians, notably those by Philibert Guybert: *Le médecin charitable, enseigant la manière de faire et préparer en la maison avec facilité et peu de frais les remèdes propres à toutes maladies, selon l'advis du médecin ordinaire* (Paris, 1625 or earlier); *L'apoticaire du médecin charitable* (Paris, 1625 or earlier), and by Paul Dubé: *Le médecin des pauvres* (Paris, 1669); *Le chirurgien des pauvres* (Paris, 1669). Dubé's work continued to appear under his name (the first four editions were published anonymously; the fifth of 1678 carried, following the author's death, the identification "Par M. Dubé, Docteur en médecine") in an edition of 1738. Guybert's publication reached a twentieth

It is an important fact, therefore, that French popular literature does not present systematized medical advice. This contrasts strikingly with the abundant medical production of the regular book trade. There one encounters a truly astonishing number of elementary therapeutic and hygienic manuals, these being commonly the product of practising physicians writing for a literate and comfortable audience intent on self-preservation, improvement and charitable acts. By contrast, medicine in popular literature was incidental to the larger objectives of this literature, namely, calendrical reckoning, amusement, reassurance and edification. Nonetheless, there appeared in popular literature a general attitude towards health and disease which, while neither deliberately cast by the publisher nor consciously called for by the consumer, proved altogether suited to the medical capacities and social condition of the latter.

Intrinsic to the *bibliothèque bleue* was a clear perception of man's place in nature. Certain assumptions were made, and conclusions drawn or implied, which largely established the scope of human action. These assumptions are most clearly seen in the almanacs and, above all, in perhaps the most impressive single title in the entire collection, the *Calendar and Compost of Sheperds.*[9] The *Compost*—first issued as a sumptuous folio in 1493 and

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edition by 1649. The supposed *colportage* editions identify themselves as his work; a (trade ?) edition was issued in 1645 by Nicolas Oudot, founder of the greatest dynasty of *imprimeurs troyens*

[8] *L'escole de Salerne. En vers burlesques* (no date, *permission* of 19 December 1714: BMT B. bl. 367, Morin 249; also ATP, edition of 1719, O'R 1 300), *Histoire générale des plantes et herbes, avec leurs propriétés, par M Leonard Fucus* [sic] (1735: ATP 1° R. 387, also BMT B bl. 268; Morin 609).

[9] The *Calendar* first appeared as *Le compost et kalendrier des bergiers* (Paris, 1493); an English edition was published in Paris in 1503 The latter was reissued in facsimile, with an invaluable historical and bibliographical introduction by H O. Sommer, as *The kalender of shepardes*, ed. H. O Sommer (London, 1892). An English edition of ca. 1518 was reprinted with modernized orthography as *The kalendar and compost of sheperds* (London: P. Davis, 1931), all of my citations below are from this edition (texts in the English and French editions differ only slightly). Numerous copies of *éditions de colportge* exist (for example, BMT B. bl 653, 654, 698, 699). On French almanacs see Bollème *Almanachs populaires*, 11-21; E. Socard *Etude sur les almanachs*

introduced into *colportage* towards 1650, wherein it enjoyed continued republication—was, as its title indicates, in large part a manual for calendrical reckoning. It also included from the outset a concise formulation of numerological relations, astrological principles and simplified medical doctrines. It may be fair to claim that the *Compost* provided the model upon which all subsequent almanacs have been cast.

Man, our unknown author declared, should know that living and dying are from God. He should also realize that God has given man a natural term, *viz.* seventy-two years. For thirty-six years man grows in "beauty, length, breadth and strength;" for another thirty-six years, he gradually declines into his grave.[10] All men want a long life and the shepherd, our author, has developed a knowledge of this matter. He claims to relate only what is evident to every observant "natural wit and understanding" and to eschew idle clerical talk and mere bookish learning. In truth, of course, the *Compost* is a compilation of previous oral and written traditions. Long life was an appropriate goal and perhaps a fair expectation for those who determined to live in a frugal and orderly manner. The shepherd, however, urged caution, for longer still was eternity and that was a matter exclusively in the hands of God. We are God's creatures; He has placed us in this world. Unceasingly exposed to the temptations and dangers of the temporal scene, quite properly concerned about our salvation and fully aware of our frail constitution, we must none the less live out our mortal term. This was God's will, he had set our natural term. The shepherd's advice presumed, therefore, to bear on how we might best pass our seventy-two years.

At this point in the discussion the increasingly abundant hygienic literature created by physicians for the regular book trade would have introduced complex variations on perennial themes of Greek medical doctrine, on diet, exercise and the proper use of the six things non-natural. Such advice was not altogether absent from the *bibliothèque bleue,* but was there exceedingly rare and incomplete and was presented in desultory and usually chaotic manner. The shepherd, for example, preferred guidance by the stars to sustained hygienic or preventive action.

The powerful astrological themes in the *bibliothèque bleue* strongly suggested a rigid celestial determinism. If so, then human action, whether based on rational procedures or on other grounds, must necessarily be without result. The shepherd and others would have written in vain. On the other

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et calendriers de Troyes (1497-1881) (Troyes, 1882); J Grand-Carteret, *Les almanachs français; bibliographie-iconographie . . . 1600-1895* (Paris, 1896; reprint, Geneva: Slatkine, 1968)

[10] *Kalendar and compost of sheperds* (1931), 3.

hand, the context of these discussions was predominantly Christian. No Christian was unaware that the creator had given man not only existence but freedom. It was man's obligation to exercise his free will and so direct himself towards good or evil. Above man there stood always God. His goodness could assure health and long life but His "justice," as the shepherd succinctly put it, always threatened man with an early demise.

Compromise seemed to be in order. The heavenly bodies might "steer a man both to good and evil without doubt of a surety" but man, by exercising his free will and utilizing his astrological knowledge, could withstand this celestial influence. God, however, dominated man. *The Mirror of Astrology,* an eighteenth-century item reportedly the work of one Sinibal of Spacidime, astrologer to the state of Milan, stated the matter with clarity and force: "Free will is [man's] own will. By its exercise we may protect ourselves from harmful matters and this both by the grace of God, who is above all things, and by reason. So it is that nothing can be determined with [true] certainty, and whoever asserts the contrary deserves to be chastized."[11] In effect, no resolution of the conflict between the powers of stars, man and God was developed in this literature. God's power, certainly, was not restricted; miracles were both possible and real. The possibility of healing both body and soul through Christ and the church was ever-present in the *bibliothèque bleue*. Regarding the powers of human capability and astral influence, however, all remained uncertain. One text will present a seemingly firm determinist position, another will proclaim human freedom. In general, the collection did not promulgate a hard line of astrological determinism but did recognize and emphasize the reality and immediate importance of celestial influences.

As to the nature of the object of these influences, that is, man's bodily frame, popular literature was singularly unrevealing. Explicit and systematic anatomical and physiological description is not to be expected in such writings and, with one exception, will not be found. That exception, not surprisingly, is the *Calendar and Compost of Sheperds*. Man, of course, was the microcosm: "Some Sheperds say that a man is a little world by himself [and this] for the likenesses and similitudes that he hath of the great world, which is the aggregation of the nine skies, four elements and all things in them contained."[12] Specific connections between the planets and body parts were spelled out by word and picture. Anatomy was dealt with summarily: a list of the names of 248 bones in the human body was provided, without further comment whatsoever, and 22 veins deemed suitable for opening were located on a figure but not named. Physiological discussion *per se* was simply non-existent, although a rude outline of principles was given.[13] These principles were drawn from long familiar themes of Greek medical tradition and in the form no doubt given them by medieval medical compilers. The qualities hot and moist are associated with air, spring and youth; hot and dry with fire, summer and early maturity; cold and dry with earth, autumn and full maturity; cold and wet with water, winter and decrepitude. To these conditions are bound, respectively the four temperaments: sanguine, choleric, melancholic and phlegmatic. Health represents an "accord" amongst these four qualities; sickness results when one quality "destroys" the others. The death of the body and release of the soul may then ensue. Warning is made briefly as to the unusual dangers presented by a change of seasons. This modest instruction continued to appear in the *Compost* throughout its existence, and indistinct elements of such principles may be found elsewhere in the *bibliothèque bleue*. Never, however, did they rise to the level of presenting a coherent doctrine of bodily function and rarely was an attempt made to found hygienic or therapeutic advice upon them. These facts are indicative of the character of the practical components of this literature. Abstraction and theoretical principles were poorly developed and concrete advice, whatever may have been its true utility, was presented in concise and peremptory manner. I turn now to this advice and inquire as to what measures of the healing art found a place in popular literature.

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[11] *Le mirroir d'astrologie ou le passetemps de la jeunesse; traitant l'Inclination de l'Homme et de sa nativité, suivant tous les mois de l'année* (Troyes, n.d: *permission* of 19 *mai* 1739), 2-3 (ATP 1° R. 306).

[12] *Kalendar and compost of sheperds* (1931), 100.

The roster of afflictions noted in the *bibliothèque bleue* is long but obscure. Reference was made to the *peste*, that is, to infectious diseases in general, to toothache, fevers, assorted pains, hemorrhages, the stone, ulcers and others.[14] Surgical procedures other than venesection were rarely mentioned. It should be emphasized, however, that in popular usage, as in contemporary medical terminology, only a small fraction of these terms would have reference to clearly defined and accepted clinical entities. Popular literature not infrequently cited diseases by name but could do so with neither the intention nor the capacity truly to specify a given disease condition. Indeed, it is much more appropriate to examine medical themes in this literature from a therapeutic rather than a diagnostic viewpoint.

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[13] *Ibid.* 108, 110-113.

[14] Bollème, *Almanachs populaires*, 51-52, provides further specification E Lombard, examining German-language Swiss almanacs, particularly those of the nineteenth century, has discerned and expounds with a wealth of detail a broad range of general disease characterization in these publications. See his *Der medizinische Inhalt der schweizerischen Volkskalender im 18 und 19 Jahrhundert* (Zurich: O Füssli, 1925)

Healing advice in the *bibliothèque bleue* presents both natural and supernatural aspects. It also offers much regarding the role of astrology in the cure of disease. Naturalistic healing encompassed essentially but two measures, drugs and venesection. Nowhere in the literature will there be found theoretical discussion of these measures; their presence is marked only by varied instances recommending their application. Supernatural and natural procedures often stood in close connection to one another; astrological texts repeatedly combined the two.

An excellent illustration of these several points is provided by the famous almanac, the *Messager boiteux* of Vevey, published annually since 1707 and still current.[15] The *Messager* combined diverse practical information with primitive reporting of noteworthy current events. Practical advice ranged from zodiacal stations and lunar phases to saints' days, regional fairs and suggestions regarding bleeding, cupping and purging. Most of this information is compressed into twelve monthly lists, a set of conventional signs being used to indicate the proper time for purging and bleeding. There are good reasons, in addition to common sense, for believing these indicators to have been printed according to no guiding principle whatsoever. Their interest lies not in their particular calendrical position but in the bare fact that they pretend to offer medical advice. The printer cautioned that such indicators were applicable only to times when "necessity," that is, manifest ill-health, was not in evidence. The implication is clear that such intervention will be preventive, not therapeutic. The reader was warned, moreover, not to use the knife on any member on the day when the moon appeared within that member's governing zodiacal sign. Lastly, preference for different modes of purging (electuary, pill or drink) was suggested by the seasons.

Bleeding and purging were, of course, mainstays of the therapy utilized by contemporary physicians. In the *bibliothèque bleue* they also appeared—but almost never with more elaboration than that evidenced by the *Messager*. No indication was given as to who would execute the prescribed procedures. Recourse to the physician was not suggested; obviously he would not be available either in person or economically to such a clientele. No advice, furthermore, was offered as to how to weigh the prevailing idiosyncrasies of a given patient against the command of the stars.

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[15] This Swiss almanac appeared to have had wide dissemination in northern France. See J C Mayor, *Vie d'un almanach. Le Messager boiteux de Berne et Vevey* (Vevey: Ed du Messager boiteux, 1957). The *Messager* itself surely derived from the earlier *Hinkender Bote*, published at Basel since at least 1676 (Lombard, *Der medizinische Inhalt der schweizerischen Kalender*, 10)

Botanicals and exotic formulations predominate in this literature; the new chemical remedies were noticeably absent. Medicaments prescribed ranged from the simple to the complex. Thus, in the realm of simples, verbena was recommended to be taken orally to counter ulcers of the mouth; the third node of a plant plus its leaves afforded a drink efficacious against quartian fevers; the boiled roots gave alleviation from colic. Moreoever, the virtue of the entire plant was so dry that its application promised to close all wounds.[16] At an opposite extreme was the celebration of theriac found in the *Grand Albert.*[17] This bizarre concoction, a favorite since Galenic times, contained opium and serpent's flesh as its principal components and surely acquired much of its imputed power, at least over public belief, from the elaborate and prolonged ritual attending its preparation. Theriac promised protection against poisons, above all, against the bite or sting of venomous animals. It also proved effective, Albert added, against paralysis, epilepsy, gout, dropsy, leprosy, and madness. Theriac thus preserved its traditional role of sovereign remedy.

Other remedies for gout included the following: "This complaint is caused by Saturn. Therefore, when either Mars or Venus is in the ascendant, take the plant called *marérica* [?] and grind it up, then mix it with an egg yolk prepared as an omelette; eat this while fasting and you will be altogether protected against the gout"—thus *Petit Albert*. A more mundane approach was suggested by the *Gifts . . . to the Four Corners of the World,* a Parisian almanac of 1779: "Boil urine and watercress, make of this a poultice which is applied to the afflicted part; ordinarily the pain will be dissipated in less than twenty-four hours."[18] If stiffness intervenes, the compiler recommended steam, brisk rubbing and application of an unguent.

These varied suggestions regarding gout are typical of the medical advice proffered by the *bibliothèque bleue*. Active intervention was assumed and yet such advice was presented in altogether unsystematic manner. Save for the

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[16] *Histoire générale des plantes et herbes*, 62-63.

[17] *Les admirables secrets d'Albert le Grand* (Lyon, 1741), 275-281 (ATP 1° R. 467). On theriac, see E. H Ackerknecht, *Therapeutics from the primitives to the twentieth century* (New York: Hafner, 1973), 37 note; G. Watson, *Theriac and mithridatum, a study in therapeutics* (London: Wellcome Historical Medical Library, 1966.

[18] *Secrets merveilleux de la magie naturelle et cabalistique du Petit Albert, traduit exactement sur l'original Latin, intitulé · Alberti Parvi lucri libellus de mirabilibus Naturae arcanis. Enrichi de figures mystérieuses, et la manière de les faire* (Lyon, 1791), 241 (ATP 1° R. 515); *Entrennes intéressantes des quatre parties du monde, enrichies de cartes géographiques, des plus correctes; contenant encore, entr'autres matières, aussi curieuses qu'utiles, un ètat très-exact de toutes les troupes du Royaume Pour l'année MDCCLXXIX* (Paris, 1778?), 116 (ATP 1° R. 29.2)

History of Plants, obviously derived from Leonhard Fuch's model of 1542, the compiler-publishers confirm the impression that they culled their medical lore haphazardly from whatever texts might be found about the print shop and from folk tradition decorated with the vocabulary of ancient medicine.

Supernatural and astrological healing, by contrast, offered better defined limits and possibilities. On the one hand there existed the influence of the stars and the power of natural secrets, all of which gave hopes for foreseeing the future or controlling events. On the other hand, this was a profoundly Christian society, and thus religious healing practices appeared frequently in its popular literature. Sin and disease, prayer and the healing powers of the Saints were common themes.

Rigorous determinism, as was remarked earlier, was alien to this literature. God held omnipotence, the stars had influence and man possessed real powers. Neither satanism nor black magic were admitted. "Let no one attribute magic or diabolism [to my work]," declared *Petit Albert,* "if, in offering some of my marvellous secrets, I make use of certain words or figures of speech."[19] Orthodoxy was strictly respected, both out of regard for ecclesiastical and civil authority and for the evident prejudices and desires of the prospective audience.

The "secrets" themselves were diverse and of unrelieved banality. The greatest collection thereof in the *bibliothèque bleue* is the *Marvellous secrets of natural and cabalistic magic of the little Albert;* they also recur frequently in scattered places in almanacs and other collections. Most secrets were of the village wit variety and had absolutely nothing to do with health or disease. Fishing tips, pigeon raising, love potions, guaranteed restoratives of lost virginity, tricks to play on your friend or enemy (particularly the latter), wine preparations—the list is endless and diverting. Medical advice was given, to be sure, but often in a form so varied as to render impossible clear categorization. The bite of a mad dog, for example, while recognized as being "infinitely dangerous," could be countered in a number of ways. Application of any of the following promised full relief: a plaster composed of ground cabbage seed, serpitium and vinegar; fresh root of wild rose; cantarides (Cancers de Rivière), burned during the dog days with the sun in the Lion and taken in bouillon for fifteen days.[20] The hair from the mad beast, burned and drunk down in good wine, also promised a cure. Best of all, however, was a bath in the sea; en route there one could also ingest all the other remedies.

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[19] *Secrets merveilleux . du Petit Albert,* 8-9. Had not, he added, such arcane practices been employed by the ancient Hebrew sages and had not Charlemagne himself received a small collection of secrets directly from the hands of the Pope? Bollème remarks the gradual shift in the *bibliothèque bleue* from a magical view of nature to increasing confidence in strictly human capacities: "Littérature populaire et littérature de colportage," 88-89.

[20] *Secrets merveilleux . . . du Petit Albert,* 38-39.

Presumably these prescriptions and other comparable practices were utilized. They offered a semblance of rationality and prescribed, at the very least, concrete steps to follow which in themselves might offer support in affliction. They claimed to offer specific advice. This claim could not, however, be made of the prediction literature where, again, medical matters made their appearance amidst a welter of others. The purpose of predictions was stated concisely by Thomas Joseph Moult, fictitious author of the obvious favorite among these prophecies, a work deriving from the *Centuries* (1555) or Nostradamus. Study my book, Moult announced, and you will live long and amass a fortune. You will avoid famine and "misfortunes of all kinds," including those affecting bodily well-being.[21] This you will do by "knowing the past and the future." Moult's predictions, tied to solar number, to weather conditions, and to a perpetual calendar of the months, were so general as scarcely to have had a fair chance of systematic failure. Thus, for 1795 his "general predictions" tell us that autumn will be wet and windy, while his "special predictions" announce that a great prince will come to power. Brief observations regarding health and disease were scattered helter-skelter throughout the monthly predictions. For March and October, the seasons of change and traditionally the time of year most to fear serious disease, Moult merely allows that "Drinking and eating wisely, take your absinthe boldly; take care of your blood, that's your business—unless you find extreme action necessary" (March), and "Now is the time to purge yourself and prudently to draw blood; drink wine temperately and make health your pleasant expectation" (October).[22] Doggerel verse contributed similar advice:

Saignée du jour Saint Valentin
Fait le sang net soir et matin.
Et la saignée le jour de devant
Garde des fièvres en tout l'an.

Le jour Sainte Gertrude bon se fait
Faire saigner au bras droit,
Celui ainsi qu'il le sera
Les yeux clairs toute l'année aura.[23]

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[21] *Prophèties de Thomas-Joseph Moult Le tout verifié dans les puits de la verité, aux dépens des astronoms les plus fameux* (n.p., n.d. [1793?], 64 (ATP 1° E. 45).

[22] *Ibid.*, 55-59.

[23] *Prophéties ou prédictions perpetuelles, composées par Pitagoras, Joseph le Juste, Daniel le Prophète, Michel Nostradamus et plusieurs autres philosophes* (Montbéliard, n.d.: early 19th century?), BN V50158).

While Moult and other far-seeing advisors seem only to advance us from the banal to the trivial, they indicate once again the abiding position which medical themes, however minor and without efficacy they may have been, occupied in popular literature.

I complete this summary view with remarks on overtly supernatural healing, that is, on the healing powers inherent in Christian faith and practice. The contrast between flesh and spirit is sharply pointed up in the *bibliothèque bleue*. The body, declaimed the *Book for Children*, a Christian primer of 1728, is of this world. Composed of the four elements, it dies when detached from the soul. The latter is a "spiritual substance, which thinks and reasons." It is, of course, immortal.[24] While a disgust for the body was encouraged, it was also recognized that one must live a term of worldly concern. Suffering was indeed the Christian's lot. The *Small Christian Daily Exercise* of 1731 provided abundant Scriptural authority for this fact, citing, among a host of other texts, *Romans* 8:18: "For I reckon that the sufferings of this present time are not worthy to be compared with the glory which shall be revealed in us."[25] Suffering, in fact, defines our existence here below. A Jesuit, Father Crasset, observed that the sick had but three obligations, "Suffer, obey and die."[26]

This harsh prospect was nonetheless softened by frequent admonition to understand the cause of one's affliction and to appreciate that therein lay also a possible remedy. Disease was the price of sin. "Why is man subject to illness?" children were asked. The catechist answered his own question: "It is because he has offended God. If he had not done so he would always be healthy and would live forever."[27] The resolution of the problem lay in prayer.

A pair of late eighteenth- or early nineteenth-century texts admirably illustrate these simple medical prayers. In a heated preface the compiler of the

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[24] *Le livre des enfans, ou idées générales et définitions des choses, dont les enfans doivent être instruits* (Paris, 1728), 7 (BN R.42075)

[25] *L'enfant sage à trois ans, contenant les demandes qui lui fit l'Empereur Adrien et les réponses de l'enfant* (Troyes, n.d.: Bollème [Bibliothèque bleue, 272] suggests circa 1760) (ATP O^1 R 1 300), cited above in the King James version

[26] [?] Crasset, S. J. *Préparation à la mort* (Troyes, ed. 9, n.d.: *permission* of 1689), 122 (BMT B bl 545, Morin 915), the full text deserves citation (122-123): "Instructions pour les malades. Un bon malade doit faire trois choses. Souffrir, Obéir, et Mourir. Souffrir pour Dieu. Obéir aux Hommes, Mourir à lui-même. Souffrir généreusement. Obéir humblement. Mourir tranquillement. Souffrir tout le mal qu'on lui fait. Obéir à tous ceux qui le gouvernent. Mourir à tout ce qu'il désire. Souffrir sans mumurer. Obéir sans résistance. Mourir sans appréhension. Souffrir en pecheur. Obéir en Chrétien. Mourir en Saint. Souffrir, acceptant tout ce qu'il plaît à Dieu de lui envoyer. Obéir, faisant tout ce qu'il plaît aux hommes de lui ordonner. Mourir, rendant l'esprit aux temps qu'il plaît à Notre-Seigneur de l'appeler."

[27] *Le livre des enfans*, 83.

Physician to the Poor insisted that his collection had nothing in common with those others which offered naught but a "scaffolding of organized sorcery" based on "evil spells and magic charms."[28] Genuine supernatural healing must depend upon Christian fatih, on the omnipotence of a God whose will alone could set matters aright. The compiler offered a selection of efficacious prayers, of which the following, offering comprehensive coverage, is typical:

> Unique and all-powerful God, our source of life and health, we fall on our knees before you to beg your mercy. It is to you, God of clemency, that we come for repentence and in you that we seek . . . a refuge from the afflictions which ravage us . . . . Lord, Let your destroying angel no longer strike us with his sword . . . . O, Lord, purify us, pour upon us the balm of health![29]

Healing prayers could be quite specific. Those addressed to St. Hubert, reflecting the realities of an ill-policed agrarian society, and aiding those who sought relief from rabies, were common in this literature, and, as noted earlier, other healing saints were called upon. *Let It Be Said and Do Good,* a rude little compilation from Troyes, offered a splendid variety of healing prayers. Specialization is evident here; separate prayers were presented for toothache, rheumatism, hemorrhage, fever, colic, skin disorders, carbuncle and afflictions of the eye. Here is an example dealing with burns. It exhibits also the usual quasi-magical practices associated with such invocations.

> Prayer for the cure of all kinds of burns: Three times you should blow on the burn, making a cross, and say: Fire of God, lose your heat, just as Judas lost his color when he betrayed our Lord in the garden of olives. Speak also the name of the person, saying: God by his power has cured you. Do not overlook the nine-days devotion dedicated to the wounds of our Lord Jesus Christ. Amen.[30]

It was possible to combine various therapeutic measures. *Petit Albert* provides a superb example, wherein a Polish soldier cured rapier wounds by means of ritual cleansing, prayer and conjuration of the offending weapon.[31] Touching for scrofula, an article of faith shared by king and masses, surprisingly received no mention in the *bibliothèque bleue.*[32]

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[28] *Le médecin des pauvres Ou receuil des prières pour le soulagement aux maux d'estomac, charbon, pustule, fièvres, plaies, flux de sang, hydropsie, rhumatismes, asthemes, ètouffements, rompures, convulsions d'enfants, choléra, typhus, scarlatine, suette, vérole, piqures ou morsures, maux aux dents, mauvaises humeurs, gales, dartres, etc.* (Troyes, n.d.: surely early 19th century), 2 (BMT B. bl. 137, Morin 753). The title, no more elaborate than many in the collection, promises more than a mere twelve-page pamphlet can deliver.

[29] *Ibid.*, 5-6.

[30] *Laissez dire et faites le bien. Le médecin des pauvres. Christus regnat Christus imperat Christus vincint. J.C regne, J C commande, J.C. est vainquer En Dieu la confiance* (Troyes, n.d.), 6 (BMT B. bl. 136, Morin 751).

[31] *Secrets merveilleux . . du Petit Albert*, 159-160.

Promises notwithstanding, it was evident that a cure was by no means assured. Death often came to the sick and, of course, would take us all at our natural term. Popular literature made vivid the inevitability of death and reinforced the fundamental Christian concern for proper preparation for death. The supreme text in this matter is the *Danse macabre,* a late medieval creation which enjoyed constant favor in the *bibliothèque bleue.*[33] In the *Danse* Death first spoke of his necessary role and then, on the call of the trumpeter, conducted conversations with representatives of the various orders of society. All, from pope to physician, from king to sot, were condemned to death. Having lived well, they now must die well. Their salvation depended upon it. Death, of course, provided the ultimate cure for all of our worldly woes.

These themes recur elsewhere in popular literature, particularly in devotional tracts. In the *Four Destinies of Man* the reader was called up sharply: rather than simply ignoring death, one must face it directly. Indeed, "one must think about death. It comes quickly and often when one expects it least."[34] Death occurred because sin was an everyday reality. God, however, was not without concern for his creatures. Signs or "messengers" of death were provided. Accidents and old age were evident threats. So, too, were various illnesses; fevers, paralyses, pleurisies, the stone, colics and pains in the head and stomach were indicated. Father Crasset's manual, *Preparation for Death,* allows a deeper insight into these vague claims. We have already heard his advice to the sick: suffer, obey and die. In this work and in various others an unstated assumption reigned: the sick were truly the dying. Little prospect was given of cure. In serious matters of illness this view surely represented a realistic assessment of the situation. Nonetheless, it must be noted that these devotional works would also, inevitably, regard death as a turning point in man's spiritual career. Had he led a pious life, it could not be a turn for the worse.

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[32] See M. Bloch, *The Royal Touch. Sacred Monarchy and Scrofula in England and France,* trans. J E. Anderson (Montreal: McGill-Queen's Univ. Press, 1973.

[33] Several copies from the *éditions de colportage* survive, e.g., *La grande danse macabre des hommes et des femmes, historiée de vieux Gaulois, en langage le plus poli de notre temps Avec· Le débat du corps et de l'âme. La complainte de l'âme damnée L'exhortation de bien vivre et de bien mourir. Le vie du mauvais Antechrist. Les quinze signes de Jugement* (Troyes, n d.: *permission* of 6 *mai* 1728). On the shifting perception of death in this period see the admirable anthology edited by M Vovelle *Mourir autrefois. Attitudes collectives devant la mort aux XVII*^{e} *et XVIII*^{e} *siècles* (Paris: Gallimard, 1974).

[34] *Les quartre fins dernières de l'homme Savoir, De la mort, du jugement dernier, des peines d'enfer Et des joies de Paradise* (Troyes, 1727), 29 (BMT B. bl. 112, Morin 956).

Can the people's health be assayed? How best is one to approach such a problem? As conclusion, I address certain questions posed by the medical themes stated in French popular literature and by current studies of other aspects of the medical experience of the poor. Investigation of the *bibliothèque bleue* offers, as remarked earlier, but one option among several. The charitable act was highly valued under the old regime and its focus was frequently and precisely stated. "The poor are subject to an infinity of ailments [and these] . . . for the most part seem to possess a character different from those which afflict ordinary men . . . . It is the poor who have grounds for greatest complaint and who deserve special consolation; it is essential therefore that we pay serious attention to the ills which afflict them."[35] The rationale for and application of such beneficent medical action is now a matter of widespread scholarly scrutiny.[36] Along a different path, Jean Pierre Goubert provides, in a masterly study of sickness and the medical trades in Brittany in the last decades before the Revolution, abundant and precise documentation of the combined effect of a dearth of trained healers, economic depression and disastrous intrusions of infectious disease.[37] Folklorists, finally, examining the principles and practice of French popular and academic medicine in the late eighteenth and nineteenth centuries, find not only substantial differences between them but sharp opposition and fierce resistance to the latter by the former.[38]

Themes regarding health and disease expressed in the *bibliothèque bleue* further develop this emerging portrait of the medical condition and capacity of the poor under the old regime. In this literature was an implicit, not an overt medical doctrine. Disease was recognized as an ever-present and inescapable reality. Health received little attention. Disease was man's common condition, health at best his precarious status after successful battle with disease. Health as an abstract good, ever-shifting but nonetheless approachable by means of assiduous hygienic observance, found, with few exceptions, scant discussion in popular literature.

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[35] M. L. ***, M. de B *** [i.e. C. A. Vandermonde] *Dictionnaire portatif de santé* (Paris, nouv, éd., 1760), II, 58.

[36] See, e.g., C. C. Hannaway, "The *Société royale de médecine* and epidemics in the *ancien régime*," *Bull. Hist. Med*, 1972, *46* 257-273 and the continuing series of publications by Dora B. Weiner, beginning with her *Raspail Scientist and reformer* (New York, Columbia Univ. Press, 1968)

[37] J. P. Goubert, *Malades et médecins en Bretagne, 1770-1790* (Rennes [Paris]. C. Klincksieck, 1974).

[38] The folklorists' program is concisely stated by F. Loux "Practiques populaires d'hygiène et de prévention de la maladie," *Ethnol francaise* 1971, *1:* 121-126 An invaluable survey of non-professional healing and healers is provided by M. Bouteiller, *Médecine populaire d'hier et d'aujourd'hui* (Paris: Maisonneuve et Larose, 1966) Bouteiller, whose attention is directed principally towards the late nineteenth century, introduces a vast domain of popular medicine with which I do not deal, namely, non-academic healers whose numbers and diversity surely made them the primary "medical" presence in rural France There is passing and hostile comment on such healers, lumped together as charlatans, in the *bibliothèque bleue* but it was the regular medical corps which most vigorously opposed these practitioners: see the brief but forceful attack by the abbé Jaucourt in Diderot—d'Alembert *Encyclopédie ou Dictionnaire raisonné des sciences, des arts et des métiers* (Paris, 1753), III, 208-210.

Generally speaking, medical advice in popular literature was activist. As such, it stands in sharp contrast to the expectant medicine promulgated by hygienic enthusiasts within academic medicine and a prominent feature of the regular book trade. Academic medicine and the medical profession prospered, of course, from disease and efforts to find and implement its cure. To this concern increasingly was added an awareness of the advantages of an need for preventive action with regard to disease.[39] This reappeared first in its classic Greek hygienic form; late in the eighteenth century these highly individualistic hygienic precepts were transformed, under combined administrative and political pressures, into assertively social public health principles and action. Cure, however, not prevention—be it individual or social—remained the hallmark of popular medicine. This seems inevitable. To the poor, life offered neither leisure nor learning and both were indispensable conditions for following the precepts of hygiene. While all orders of society were susceptible to disease, particularly the fierce and recurrent outbreaks of epidemic disease characteristic of most societies before 1850, the poor suffered demonstrably higher mortality, and presumably greater morbidity, and exhibited a lower life expectancy than the wealthy. If there were to be medical advice for the poor, almost inevitably it would have to emphasize active intervention.

In this sense there is, then, an affirmation of life in the *bibliothèque bleue*. It is not a joyous affirmation nor one asserted with unquestioning confidence. It simply assumes that in this world we are to cope as best we can; neither society nor science is concerned with our lot. French popular literature has, however, been characterized by its foremost student as a "literature of evasion." Emphasis upon the miraculous, both pagan and Christian, and systematic omission of commentary on political events and social conditions persuade Mandrou that the *bibliothèque bleue* articulates a "form of alienation" of the masses.[40] Unwittingly, perhaps, but with force and consistency the publishers of popular literature contributed to reaffirming in popular consciousness the impotence of the dispossessed and the greater rewards of the hereafter.

[39] See W. Coleman, "Health and hygiene in the *Encyclopédie* a medical doctrine for the bourgeoisie," *J Hist. Med.*, 1974, *29* 399-421

[40] Mandrou, *De la culture populaire* 40-41, 162-163.

If such is held to be the unstated ideological commitment of the *bibliothèque bleue,* it certainly cannot be extracted from all components of that literature. Not merely the medical themes expounded earlier but the intrinsic character of the entire almanac literature can be and have been shown to exhibit an affirmative, not an evasive attitude towards this world and man's place therein.[41] With regard to medical measures it is not their efficacy, objectively assessed, which is in question. Eclecticism reigned: miraculous cures stand side by side with astral influences, venesection and botanical remedies. The separate or combined efficacy of these measures seems wholly uncertain. At issue, instead, is the question of intention. Medical advice in the *bibliothèque bleue,* however crude, most certainly was not intended as a guide to salvation or as an aid to reconciling the poor to his dismal condition. Medical advice, obviously, was stated in order to offer hope and means for overcoming physical affliction, for mastering the diseases of this, not the next world. I find Mandrou's claim too comprehensive.

Taken globally, the medical themes presented in French popular literature but add to the growing complexity of our perception of medicine during the Enlightenment. The *bibliothèque bleue* presents neither a body of reasoned physiological or pathological theory nor a coherent therapeutic program. It derives from academic medicine, preserving many features of the old medicine. It derives also from traditional folk practices and from central tenets of Christian belief. But, most importantly, and unlike so much medical discourse of the Enlightenment, it is not a system and cannot properly be forced into a system. It is a chaos with rudiments of order.

Its paramount interest is the access it provides to values regarding health and disease, indeed, life itself. That interest is heightened by the likelihood that these values were accepted by a vast and still illiterate population. Here we touch upon medical consumers who, it appears, had no direct access to formal or academic medicine. The fundamental concern of the people was, not surprisingly, for the restoration of health. This fact alone is witness to the assertion, so far as human powers allow, of life over death. In matters of disease the seemingly quiescent masses chose action.

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[41] Bollème has emphasized the "practical" component in many areas of the *bibliothèque bleue,* see her *Bibliothèque bleue,* 24-25, *Almanachs populaires, passim.*

*THE FIELDING H. GARRISON LECTURE**

WALTER REED AND THE ORDEAL OF HUMAN EXPERIMENTS

WILLIAM B. BEAN

A comprehensive inquiry into Walter Reed and human experimentation is impossible in the time allotted. I shall consider a few historic phases of experiments on man, the evolution and sometimes revolution of ethical and moral ideas, and the prevailing climate of opinion at the time the Yellow Fever Board under Walter Reed did their classic experiments in Cuba. Walter Reed prepared for his subjects a contract, a covenant that set forth clearly in humane terms his understanding of moral responsibility and the meaning of informed consent. Today sweeping moral agitation by earnest though sometimes sentimental and unwise persons has led them so far as to proclaim that all experiments on people are wrong. So also they have proclaimed that experiments on any living creature are evil.

Traditionally, the good physician determines what should be done for, to, or with the patient. The doctor must protect as well as help the patient. The law, to a degree, protects society. We need to be fully aware of the conflict between individual rights and the rights of society, the commonwealth or the Res Publica. In our concern for the rights of the individual we must not forget that society has rights too. Anyone living in a society has a debt to it. Experimental biological science is necessary to advance curative medicine and public health. Exclusive focus on individual and personal rights may accelerate the decline of society down the path of its own self-destruction. We must strive for optimism even while keeping constantly before us Sir Thomas Browne's words, "There are incurable diseases in medicine, incorrigible vices in the ministry, and insoluble cases in law." When an irresistible force hits an immovable object, chaos may lead to disaster unless we regroup and redefine.

Older History. In ancient times, warriors, kings, and potentates, inured to the destructive bloodshed of warfare and accident, killed most captives,

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* Delivered at the annual meeting of the American Association for the History of Medicine, Galveston, Texas, 13 May 1976. A substantial biographical sketch of Walter Reed included in the address has been omitted here. For further information on Reed, see references 32 and 33.

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often after torture. Some they enslaved as prisoners. At times slaves were used for experiments by physicians and philosopers. These might be motivated by idle curiosity or a true search for understanding.

During the heyday of scientific and medical achievement in Alexandria when the celebrated library and museum flourished, Erasistratus and Herophilus taught about the fabric of the human body from actual observation. Ptolemy furnished an abundance of bodies of dead persons for dissection. Condemned malefactors were made available for vivisection. Celsus in his *De medicina* (1) noted that the Alexandrian anatomists used criminals "for dissection alive, and contemplated, even while they breathed, those parts which nature had before concealed." Nothing permanently useful came of it.

Certain British kings, Fallopius of Pisa, and many others permitted use of condemned criminals for experimentation. They might mitigate a sentence or give freedom to survivors who had no choice but to submit to experiments which put their lives in grave but usually unknown peril. During periods when petty crimes were capital offenses and medical therapy was as likely to do harm as good, experiments on prisoners, potentially helpful to others, were considered perfectly proper. The matter was almost never discussed.

Animal Experiments. Historically, concern for cruelty to animals was articulated many decades before the current fret and worry about experiments on people. This was motivated by an uncertain mixture of compassion and ignorance. It is hard to make any just estimate of the number of us who might have suffered severe illness or died if a very large number of humane experiments on animals had never been done. Not many *of us* would be alive. Few scholars are wise enough to bestow belated praise on the company of workers who have made possible the improvements seen in today's life expectancy, more than double that of the founding fathers of the Republic 200 years ago. We do not know how or where to render thanks for having escaped diseases we have never seen or heard of. The history of experimental medicine reveals recurrent frontal attacks by antivivisectionists. In most places in the western world, their activities take the form of intensive campaigns to promulgate or change laws regulating animal experiments. It is essential that all studies on animals be humane. Though abuses have occurred, they are rare. Where experimentation has been severely curtailed or prevented by legislation backed by some strongly motivated group, broad advances in knowledge of human physiology, in surgical techniques, and their application to human beings have been thwarted, delayed, or denied. A moratorium or a veto on biological experimentation has a profound, predictable, and potentially fatal effect upon society and the common weal. We have no record that opponents of experimentation were tormented at their own death by having prevented experiments that might have spared them.

Human Experiments. Let me introduce some thoughts of Claude Bernard on experimental medicine (2). "For direct applicability to medical practice, it is quite certain that experiments made on man are always the most conclusive. Nobody has ever denied it . . . . First, have we a right to perform an experimental operation on man? Physicians make therapeutic experiments daily on their patients and surgeons perform operations daily on their subjects. Experiments, then, may be performed on man, but what are the limits? It is our duty and our obligation to perform an experiment on a person whenever it can save life, cure or gain personal benefits for the person. The principle of medical and surgical morality, thus, consists in never doing on human beings an experiment which might be harmful to any extent, even though the result might be of great advantage to science, that is, to the health of others. But performing experiments and operations exclusively with the patient's own advantage in view does not prevent their turning out profitably to science."

Autoexperiments. A relatively neglected part of human experimentation is autoexperimentation. Here the experimenter and the subject are one and the same. Lawrence Altman (3) has made a detailed but far from complete survey of this topic. In many classic studies the major or only subject was the experimenter himself. Autoexperimentation has been surprisingly common. It goes back at least to the days of Sanctorius. Nearly four centuries ago he did experiments, weighing himself every day for decades. He invented a clinical thermometer and introduced ideas about human metabolism which launched it as a science.

Witts (4, 5), McCance (6), and many others have said that the safest way to achieve informed consent is never to do to someone else what one has not done to oneself. It is surprising that only rarely has self-experimentation led to mistakes and practically none to disaster. The most famous mistake of interpretation was the result of John Hunter's (7) inoculation of himself with discharge from a penile lesion. He got both gonorrhea and syphilis. Naturally, but wrongly, he concluded that the two were one; thus, he put a cloud over the nature of venereal disease which could not be removed until the gonococcus and the spirochete were identified.

Another famous study that some historians have completely misinterpreted was that of Stubbins Ffirth (8). When an undergraduate medical student at the University of Pennsylvania in the 1800s, he did some experiments on himself. Heroic and disgusting they were. He had been appalled at the havoc caused by yellow fever, about which there was great confusion. He tried to find out whether he could produce yellow fever in himself by ingesting or injecting blood, urine, sweat, black vomit, and other material from severely ill and dying patients. He gave himself a series of the most grotesque inoculations. He swallowed all manner of repulsive material taken from patients in the last stages of yellow fever. There was no way for him to know that at the end stage of yellow fever the virus cannot be recovered. The blood was perfectly safe just as it is safe for a mosquito to bite a yellow fever patient after the third day of the disease. Ffirth's natural but completely erroneous conclusion was that the agent producing yellow fever was not to be found in blood, sweat, urine, feces, or black vomit. This has been misinterpreted by those who forget that a negative finding cannot disprove a general idea except for a special and particular instance (9).

Pettenkofer and Emmerich (10, 11), who were having an argument with Koch and others about the etiology of cholera, swallowed cultures loaded with virulent cholera vibrios. Emmerich developed what any good clinician would recognize as cholera. Pettenkofer, although he did get a sharp diarrhea, was fit again after a day or two. He attributed the diarrhea to other causes. They denied that the vibrio was the causal agent in cholera. This is but another example of how fixed ideas lead to misinterpretation of the obvious.

Cranden (12), in a study in which he produced experimental scurvey in himself, misinterpreted some of the findings, perhaps because the experimental diet affected his judgement.

One of the most remarkable was Forssmann's (13, 14) dramatic self-experimentation in an effort to find out the best technique for, and the degree of safety of, intracardiac injections. After he had made many experiments on cadavers, in 1929 he introduced a very flexible narrow bore catheter into a vein he had opened in his arm. He described what he had done as follows: "Because venipuncture with a thick needle is technically too difficult to perform upon one's own body, I performed, under local anesthesia, a venisection in my left elbow and introduced the catheter, without resistance, its entire length, 65 cm . . . . I checked the position of the catheter itself in a mirror held up by a nurse before the fluoroscopy screen . . . . Even the rather long trip in our Institute from the operating room to the x-ray department, during which I had to climb stairs, traveling on foot with the catheter located in the heart, was not associated with any annoyances."

Later on, Forssmann tried the same technique in order that he might introduce radio-opaque materials into his heart for angiographic studies. Based on his work, cardiovascular research and therapy came of age about a decade later in the laboratory of Andre Cournand and Dickson Richards.

For convenience, reliability, the satisfaction of curiosity, and the use of a known and "normal" control, autoexperimentation has unique advantages. Against these one must consider the role of ambition, self-interest, legal and family responsibilities and even the dark realities of suicide. We know that self-experiments have been done innumerable times and gone unreported.

The danger of experimenting on persons sick or well is so evident that no physician of goodwill considers experiments without weighing the pros and cons, and without soliciting from one or more colleagues advice about consent. An experimenter may fail to be objective. Colleagues and associates must help determine what may or may not be done. A physician, an institutional committee or a group of laymen including lawyers, clergymen, men of affairs, and physicians must watch over the rights of patients and subjects. They should approve or reject proposals and see that all reasonable aspects of informed consent are fulfilled. *They must respect and protect every person's right to volunteer, to refuse, or to opt out at any time.*

Abuses of Experiments. Attitudes change with the changing time. Here are some examples of outrageous abuse of the medical man's position of power and authority to do experiments around the time of Walter Reed's experiments. In their major characteristics they were not exceeded by the Nazi "doctors of infamy." Though some recorded their reactions of horror there were no effective counter measures. It would be easy to compile a list of hundreds of legitimate, compassionate and humane experiments.

I. Armauer Hansen (15), discoverer of the bacillus of leprosy slightly more than a hundred years ago, conducted a uniquely horrible experiment. He tried to infect an unwilling subject with his leprosy bacillus. He could find no experimental animal susceptible to the disease. He was convinced of the transmissibility of leprosy. He wished to inoculate the organism into a person. Without getting permission, indeed against the strongly expressed plea of a 33-year-old woman, he twice pricked into the surface of her eye material from needles he had passed through the nodules of a leprous patient. This created an uproar. When Hansen was interrogated, he admitted that he had not first explained to the subject what he had wished her to do. "Inside the office he had only told her that he would give her a prick in the eye, but "the purpose of this treatment he did not disclose to her." The trial and subsequent acquittal with only a minor reprimand are a part of history. This state of affairs reflects a very different notion of the physician's responsibility to his patient in treatment and in experimentation than prevails today.

II. An example of ruthless cruelty by a surgeon is illustrated by an experience around 1900 related by the Johns Hopkins surgeon J.M.T. Finney (16) in his autobiography. He was interested in seeing the work of a widely-recognized plastic surgeon in Berlin. He said:

> I won't go into the details as to all I saw. I was so disgusted and outraged by what I did see that I never returned. On that one occasion the patient was a young woman about twenty. She was wheeled into the operating room on a stretcher, then stripped of all her clothing, lifted to the operating table and tied there by the orderlies with bandages binding her legs together and her arms to her sides, with her head pulled back over the end of the table and tied fast there in a most uncomfortable position. Thus she could not move her head, arms or legs, but could only cry. The whole procedure was brutal. There was no nurse present, only a maid, and the surgical amphitheater was full of doctors and medical students. When she cried from fright and from rough handling, one of the orderlies would smack her on the side of the face and roughly tell her to shut up. When the surgeon himself came in, she was crying loudly and begging for mercy. He walked over and gave her a resounding smack on the cheek and in turn told her to be quiet. He then proceeded to do the operation, a most painful one, without a drop of anesthetic of any kind, believe it or not. The poor girl screamed and cried until she stopped from sheer exhaustion. The details of the operation are too horrible to relate. I waited until after it was over, just long enough to go up and ask the operator—I won't call him a surgeon—why he hadn't given the poor girl an anesthetic. With a shrug of the shoulder he replied, "It wasn't necessary. We could hold her." Fortunately, that experience was unique, but I must say that it made me thankful to get out of Germany without having to have a surgical operation done on myself or any of my family.

Finney thought German surgeons were skillful operators. But the advances he saw he thought resulted from disregard of human life and suffering. They attempted things that were thought unjustifiable in most other countries. He said: "This was true to a limited extent of all the German clinics that I visited. On the whole German surgery left an unfavorable impression. Though results were fairly satisfactory, the human element was largely lacking. The patient was something to work on, interesting experimental material, but little more." Cullen's biography (17) reflects the same view of some Continental physicians and surgeons.

In an excellent symposium on "the problem of experimentation on human beings" published in 1953, Alexander M. Kidd (18) discussed medicolegal aspects. He remarked, "Americans who studied in Vienna, Leipzig and other medical centers before the first World War admired the skill of the doctors but were shocked at the treatment administered and operations performed which would never have been undertaken had the welfare of the patient been the sole consideration."

The evidence is strong that there were many instances of a callous view of patients' rights. In 1951 I said:

> Moral necrosis is sinister in its pervasive insinuations, and all who are concerned with clinical research, that is, experiments on themselves and their fellow man, must face the implications. The recent degradation of physicians in Nazi Germany exemplifies the decline and fall of a group whose moral obligations went by default in a single generation. The house would not have fallen had not many timbers been rotten. Descent into the gas chambers by doctors of infamy had its beginning in disregard for the patient" (19).

Yellow Fever. Now let us come to yellow fever in Walter Reed's time. For hundreds of years there had been tragic errors in the frustrating pursuit of the cause and search for the manner of spread of yellow fever. Professor G. Sanarelli's announcement in 1897 that he had found the specific etiologic agent of yellow fever, the "bacillus icteroides," gained him the liberal reward promised the discoverer of the cause of yellow fever by South American countries. Sanarelli (20) tried to validate his claim for his bacillus by injecting cultures into patients. He relates five experiments on human subjects which he did without permission or consent.

> My experiments on man amount to five. For reasons readily understood I have not employed living cultures but simply cultures in broth of fifteen to twenty days, filtered through a Chamberland bougie, and then for greater precaution sterilized with a few drops of formic aldehyde. In two individuals I have experimented on the effects of subcutaneous injections, in the other three the effect of endovenous injections. These few but very successful experiments have been sufficient to illuminate with a truly unforeseen light all the pathogenic mechanisms until now so obscure and badly interpreted.
>
> Case III—The subject received an intravenous injection of 10 c.c. of a filtered culture. This was followed within fifteen minutes by nausea and vomiting, general agitation, and pain in the lumbar region. Gradually the abdominal region became painful and the slightest pressure with the hands over the abdomen or in the lumbar region caused severe pain. The temperature, which before the injection was 37.1°C (98.7°F) rose within two hours to 101.3°F, and within four hours to 104.5°F. The patient complained of severe headache during the night, and continued to vomit. The following morning the temperature was normal; at 4 p.m. it rose to 101.3°F, and there was slight delirium; there was complete gastric intolerance and anuria. The liquid vomit had a pale coffee color, and upon microscopic examination was found to contain red blood corpuscles and leucocytes; it had an acid reaction. The anuria continued for two days, when a little turbid urine was obtained from the bladder; this was completely coagulated by heat. Delirium interrupted by brief periods of coma, continued during the second day, after which the patient's condition improved and recovery took place. On the second day the sclerotics had an evident subicteric tint.

> On the second day an explorative puncture of the liver and kidney was made and some juice was obtained through the aspirating needle. Under the microscope this was found to contain, from the liver, hepatic cells in a profound state of fatty degeneration and, from the kidney, epithelial cells in a state of intense tumefaction and granular degeneration.
>
> The injection of the filtered cultures in relatively small doses reproduced in man typical yellow fever, accompanied by all its imposing anatomical and symptomatological retinue. The fever, congestions, haemorrhages, vomiting, steatosis of the liver, cephalagia, rachialgia, nephritis, anuria, uraemia, icterus, delirium, collapse; in short, all that complex of symptomatic and anatomical elements which in their combination constitute the indivisible basis of the diagnosis of yellow fever. This fact is not only striking evidence in favor of the specific nature of the bacillus icteroides, but it places the etiological and pathogenic conception of yellow fever on an altogether new basis.

It was not long before this outrageous conduct was attacked in an editorial in *Medical News* in September 1898.

> Not content with the inoculation of toxin into common animals, Sanarelli resorted to experiments on the human subject. These experiments were not published in the *Annales de l'Institut Pasteur*, and I should have made no reference to these were it not that they have been published *in extenso* in the *Annales d'Igiene Sperimentale*, vol. 7, 1897, and have been freely mentioned in American and English journals.
>
> Two subjects received respectively 2 and 5 c.c. of the filtered toxin subcutaneously. Apart from painful, local swelling, some depression, and lack of appetite, no further results were obtained. The most powerful effects, however, followed the intravenous injection of 10 c.c. of the toxin. Incessant vomiting, complete anuria, delirium and coma were the result; 5 c.c. intravenously in another subject produced likewise very serious results. A fifth subject was not so susceptible and was given 44 c.c. in four injections in eleven days.
>
> Sanarelli sees in these experiments a perfect demonstration of the relation of his bacillus to yellow fever . . . . Nothing is stated concerning the physical condition of the subjects and the presumption is that they were hospital patients . . . .
>
> It is evident, therefore, that the weakened conditions of the patients at the time of the injection had more to do with the profound symptoms observed than the toxin introduced.

At a meeting of the Association of American Physicians in the spring of 1898, Victor Vaughan in discussing the bacillus icteroides and Sternberg's bacillus X said:

> The experiments that Sanarelli made upon men are simply ridiculous, if we take them as evidence that this germ is the cause of yellow fever. He injected the toxin into patients (I suppose they were patients, for they were in the hospital), and he got marked effects, and jumps to the conclusion that it was yellow fever. There are many germs the toxins of which may, on inoculation, produce "marked effects," and we

> cannot conclude from such an experiment that the germ is the specific cause of any disease.
>
> It has always seemed to me since Dr. Sternberg's most excellent work upon the subject that the probabilities are that yellow fever is not due to bacteria. Of course, we may be mistaken, but I think that if it had been due to such a cause Dr. Sternberg would have discovered the organism. I have a great deal of faith in the clinical belief of our brethren in the South that yellow fever never hibernates in cold countries. I do not know Dr. Sternberg's opinion upon this point, but he will remember that our old friend, Dr. Cochrane, held such views and defended them with great tenacity at the Yellow Fever Congress of 1889. Long-continued freezing of Sanarelli's germ does not affect its virulency. You may leave it frozen for a great many days, and yet when thawed out it is just as virulent as at the start (21).

William Osler was more emphatic and explicit.

> One other point that has been referred to I should like to mention—that is, the question of experiments upon men, as they were brought forward prominently in Sanarelli's paper. They have been characterized by Dr. Vaughan as ridiculous, but I beg to say that is not the term to use. The limits of human experimentation should be clearly defined in our minds. From a clinical standpoint almost every dose of medicine we give is an experiment. Who knows what may happen by giving a 2 grain dose of quinine to a child? It may die, as did one of Dr. McKenzie's children. To deliberately inject a poison of known high degree of virulency into a human being, unless you obtain that man's sanction, is not ridiculous, it is criminal (22).

In the Miscellany column of the *Journal of the American Medical Association* (23) of 26 May 1900, on page 1358, there is a long paragraph about experiments on human beings. This dealt initially with the discussion of the German Parliament of Dr. Neisser's inoculations of a few prostitutes with syphilitic serum. "This gave rise to a denunciation of the entire medical profession" although the Ministerial Director, Althoff, argued against the attributing to all the faults of a few.

> Virchow proclaimed the absolute necessity of finally testing remedies on man after conclusive tests on animals: "The conscience of the physician tells him what is right and what is wrong, and a code of ethics of the medical sciences is gradually being evolved, over the entire world, which as time goes on will prove more effective, more beneficent in its results, and be more respected than the deliberations of any peace conference.

In the discussion it was pointed out that millions of people in India had been inoculated with smallpox virus from patients with the acute disease by priests over a number of centuries. A small number died. Jenner had inoculated a healthy person with virus from an animal suffering from a disease the nature of which he only surmised. Pasteur, not a physician himself, inoculated a man with rabies.

Walter Reed's Views. Scattered in the record, there are a number of examples of Walter Reed's concern for humaneness in experimenting upon animals and his deep concern for volunteers.

Sternberg reported experiments in 1895 done by Walter Reed in a paper entitled "Report on Immunity Against Vaccination Conferred Upon the Monkey by the Use of the Serum of the Vaccinated Calf and Monkey" (24). In his discussion Reed recorded that the monkeys were anesthetized before the injection of serum. This was a combination of humane treatment and common sense precaution.

A Dr. L. E. Rauterberg had addressed the U.S. Senate Committee on Vivisection in the spring of 1896 describing horrible things he alleged were practiced on dumb animals under the sanction of the officers in charge of the United States Army Medical Museum. Walter Reed had been Curator of the Museum since 1893.

In the unpublished "History of the United States Army Medical Museum, 1862-1917," D. S. Lamb (25) related that on 5 June 1896, Dr. Walter Reed wrote to Dr. McConnell with regard to the charge of cruelty to animals practiced at the Army Medical Museum. McConnell replied, saying:

> Those who were practically engaged in the Microscopical Division should know better than any one else the character of the work that was performed, and that all animals experimented upon were under the influence of an anaesthetic. One who was not in any manner connected with the Microscopical Division of the Museum, as was the case with Dr. L. E. Rauterberg, could draw upon his imagination very satisfactorily, and write a vivid description of what might have been done with animals, the remains of which he saw under alcohol in specimen jars. I, however, testify that at no time during my connection with the Army Medical Museum, from about 1870 to the end of the year, 1895, have any experiments been performed upon animals in which an anaesthetic was not used, unless one of the ordinary inoculation experiments, which were practically painless, nor were animals kept in a mutilated condition.
>
> Very respectfully,
> (Sgd.) Dr. J. C. McConnell.

Another example occurred at a meeting of the Medical Society of the District of Columbia on 27 October 1897, where Walter Reed presented the details of the study (26) of splenic leukaemia. In the discussion, Dr. A. F. A. King spoke as follows:

> The subject of leukaemia is especially interesting, because we are unable to treat it successfully. Since leucocytes destroy micro-organisms, it might be an interesting experiment, were it justifiable, to inject bacteria into the blood of a leukaemic subject and note whether the large number of leucocytes would so far increase phagocytosis as to protect the system from bacteric infection.

Dr. Walter Reed said:

> I should be afraid to try Dr. King's suggestion upon human beings. We do not know what the injection of bacteria into the blood of human beings suffering with leukaemia might do, especially as the polynuclear elements are much reduced. I agree with Dr. Carr that patients with leukaemia are more liable to abscesses, etc. However, leukaemia is not confined to human beings; the lower animals have this disease, and if we could only get hold a leukaemic dog Dr. King's suggestion might be tried very easily.

The Yellow Fever Experiments. When the Army Board of Reed, Lazear, Carroll and Agramonte was formed it was ostensibly to study infection, but really to work on yellow fever. Immediately upon the arrival of Reed and Carroll in Cuba, 25 June 1900, they joined forces with Lazear and Agramonte who were already on the scene. They met frequently and discussed many aspects of the problem. In one of Carroll's papers (27) he remarks of these early sessions, "the moral responsibility was next considered, and in a later conference it was agreed that the members of the Board would themselves be bitten and subject themselves to the risk that necessity compelled them to impose on others."

It had been agreed by the Reed Board that the members would volunteer, all but Agramonte who was immune. According to Truby, "Reed had proposed this because of his apprehension about experimenting on human subjects unless the members of the Board led the way (28)." Governor General Leonard Wood, a physician, approved the Board's plans of finally testing Finlay's theory on volunteers. Otherwise Reed would never have undertaken this dangerous procedure. Finlay himself had spent years experimenting on human subjects. None of his subjects died, but none got yellow fever. Reed had the support of Surgeon General Sternberg. Experimenting on human subjects at that time was the only way of testing the theory since all efforts at inoculating animals had failed. Truby said, "I don't think that any member of the Board had any expectation of meeting with the sudden success which resulted from Lazear's preliminary experiments. Otherwise Reed would have delayed the mosquito work or had his trip back to the United States postponed."

Reed, because he was in his late 40s, was either advised or ordered by Sternberg not to experiment on himself and this determined his experiments on others (29). He would not use subjects over the age of 40. Carroll, apparently not believing in the mosquito transmission, was bitten by one of the "loaded" mosquitoes. After Reed's death he was criticized for this position. Had Carroll died as a result of his risky and frivolous autoexperimentation it would have put an end to the experiments and prevented verification of the preliminary studies. Indeed the death of any volunteer would most certainly have ended the experiments.

Another example of Reed's concern about "informed consent" is seen in a letter he wrote to General Sternberg from Indianapolis, 22 October 1900, where he had presented the first public announcement of the evidence for the mosquito theory.

> Dear General Sternberg: Just as I was leaving the city, I dropped you a postal suggesting that the words "with his full consent" be erased in the cases of Carroll (page 12) and Lazear (page 20), as their absence from the history of T.C.Y. might attract attention and lead to the inference that his inoculation was done without his consent . . . .

This change was not made because the paper was already in press and came out shortly thereafter.

Reed's supreme concern for proceeding with scrupulous honesty and mutual understanding is seen in the compact he drew up and signed with each volunteer. No experimental animal had been affected by blood or anything else from a yellow fever patient. Lazear and Carroll had been bitten by "loaded" mosquitoes and got yellow fever. Lazear, who died tragically, perhaps thought he would not get yellow fever or did not understand the importance of keeping away from other mosquitoes and from yellow fever patients or the importance of avoiding autopsies. The experiments were not controlled. So Walter Reed prepared a document for the potential subjects to study and sign with him if they so desired. This seems to be the first document to set forth the conditions of the compact. The document runs as follows (30):

> The undersigned, Antonio Benino, (signed) *Antonio Benino* being more than twenty-five years of age, native of Cerceda, in the province of Corima, the son of Manuel Benino and Josefa Castro here states by these presents, being in the enjoyment and exercise of his own free will, that he consents to submit himself to experiments for the purpose of determining the methods of transmission of yellow fever, made upon his person by the Commission appointed for this purpose by the Secretary of War of the United States, and that he gives his consent to undergo the said experiments for the reasons and under the conditions below stated.
>
> The undersigned understands perfectly well that in case of the development of yellow fever in him, that he endangers his life to a certain extent but it being entirely impossible for him to avoid the infection during his stay in this island, he prefers to take the chance contracting it intentionally in the belief that he will receive from the said Commission the greatest care and the most skillful medical service.
>
> It is understood that at the completion of these experiments, within two months from this date, the undersigned will receive the sum of $100 in American gold and that

in case of his contracting yellow fever at any time during his residence in this camp, he will receive in addition to that sum a further sum of $100 in American gold, upon his recovery and that in case of his death because of this disease, the Commission will transmit the said sum (two hundred American dollars) to the person whom the undersigned shall designate at his convenience.

The undersigned binds himself not to leave the bounds of this camp during the period of the experiments and will forfeit all right to the benefits named in this contract if he breaks this agreement.

And to bind himself he signs this paper in duplicate, in the Experimental Camp, near Quemados, Cuba, on the 26th day of November nineteen hundred.

The contracting party,
Antonio Benino (Sgd.)

On the part of the Commission:
Walter Reed (Sgd.)
Maj. & Surg., U.S.A.

Early during the yellow fever studies Spanish newspapers in Havana had flaming headlines about Spanish immigrants inoculated with yellow fever by means of mosquitoes. The newspaper *La Discusion* of Wednesday, 21 November 1900 carried a blast (31) at the yellow fever experimentation, headline: "Horrible . . . . If True." The newspaper had two red hot articles abusing the "criminals" they said took unsuspicious Spanish immigrants and had them bitten by yellow fever mosquitoes and even sleep in infected bedding.

26 November 1900—Major Reed to General Sternberg:

> Some of the Havana papers especially *La Discusion*, have abused us soundly and have charged us with all kinds of inhumanity and barbarities; but since the Spanish Counsel [sic], a most courteous and intelligent gentleman, assures us that we shall have his support, as long as we do not use minors and the individual gives us written consent, I am not at all disturbed by these newspaper attacks.

Jefferson Randolph Kean recorded further details in his diary, November 23rd:

> "Discusion" had two red hot articles abusing the "criminals" who take unsuspicious Spanish immigrants and have them bitten by yellow fever mosquitoes and sleep in infected bedding. Reed went to see the Spanish Counsul [sic] who said that if they (the volunteers) were of age of consent, 24 years and agreed after full understanding, he saw no harm in it, and would make no objection. The first experiment was begun I think yesterday, Nov. 22 by a loaded mosquito biting the hospital corps Private Kissinger. When Reed proposed the experiments to the Spanish immigrants today they were delighted, and those who were under the age almost wept while the others laughted at them. We still evidently have no trouble with the immigrants. They suspected a disgruntled "M . . ." in charge of the immigration camp was spreading gossip.

In all the papers Charles Finlay had published about the more than a hundred subjects in whom he tried to produce yellow fever, there is no statement of his getting permission from the potential victim of the experiments. While this may not seem a simple omission to us today, it would be recalled that his subject, having seen brothers, cousins, family and friends die of yellow fever knew that anybody who came to Havana sooner or later would get yellow fever. If they survived they would never run the risk again. Thus, one should not judge these studies harshly. Finlay was the first person who had tried to produce yellow fever in susceptible persons in its own terrain by what he thought was the way it was spread. It is ironic that his theory was correct, and he actually had blamed the right mosquito but never proved it.

More than a dozen years after the yellow fever experiments were completed it was decided that the surviving volunteers who got yellow fever or their nearest kin should get some recognition. An Honor Roll listed those who were given gold Medals of Honor. This included members of the Yellow Fever Board, Reed, Carroll, Agramonte and Lazear, or their next of kin, the thirteen American volunteers who got yellow fever beginning with Kissinger and the six who slept in filth who did not get yellow fever. Fortunately, by a combination of excellent nursing, prompt institution of bed rest, good diet and enormous luck, none of the 22 experimental subjects, Spanish and American, died of yellow fever. It is ironic and curious that the act of volunteering, of risking life, was not honored. Except for the six who lived intimately with filth the reward was for getting sick and then getting well. Surely today the honor would be awarded for volunteering without regard to whether the results were negative, positive or fatal.

31 January 1901—Major Reed to General Sternberg:

> I cabled to you this afternoon, requesting that you delay publication of our additional note until you receive the copy which I shall mail in the morning. I have inserted a *footnote* of importance which I thought should accompany the note when published. Although our immune (mosquito made) cases, 4 in number, have shown no symptoms whatever, I am very uneasy about the non-immune soldier [Private John H. Andrus] who got the same quantity of blood (1 cc) at the same time. He seems to have acquired a very serious infection, his temperature running along the 104° line now for three days, albumin appeared at the end of 18 hours, but it is not excessive. Should he die, I shall regret that I ever undertook this work. The responsibility for the life of a human being weighs upon me very heavily just at present, and I am dreadfully melancholic. Everything is being done for him that we know how to do.

* * *

Truly we live in a complex world. To understand it, to survive and to flourish, we must do all sorts of experiments. Or we perish. Perceptively given informed consent is the keystone of the arch, linking the pillar of concern for person to the most rigorous scientific critique of procedure and process. There is a right to volunteer balanced by a right to refuse. The physician-investigator needs formal or informal but strict guidance and monitoring by colleagues. Any question about what is right and good must be determined in favor of the patient or subject. But to deny the validity, justice and mercy of truly informed consent is to deny the possibility of covenant between or among human beings. A society governed by principles of universal paranoid distrust is doomed like a tree with a dead tap root.

Walter Reed grew up in surroundings of family love, strict Christian principles and the rigorous discipline of classical education more than a hundred years ago. (A sketch of Reed's life presented in the talk is omitted from the paper. For details see references 32 and 33). He survived many forlorn years as military surgeon in isolated Army camps and posts, enduring the *ordeal by ennui* which so often destroys. As he neared forty his intellectual life had a rebirth in the exciting formative year at Johns Hopkins where Welch, Osler and the others were lighting the lamps of learning at a new temple where medical arts and sciences worked hand in hand. A splendid study of malaria, and an epoch making analysis of a destructive epidemic of typhoid fever during the Spanish-American War demonstrated Reed's high order of talent. The superb and frighteningly dangerous studies of yellow fever followed. Reed, Lazear, Carroll and Agramonte each contributed his part to what was a supreme model of team investigation. They were helped mightily by forerunners and contemporaries. Nott, Beauperthuy, and King had suggested the mosquito as a vector of disease. Charles Finlay suggested the very mosquito which carries the virus. He did a hundred studies trying to transmit the disease. He failed, not knowing the short period when the victim has virus in the blood, and the two weeks the mosquito must wait before she can transmit enough virus. Carter was describing this delay which soon became known as the "period of external incubation." The Board got to work. Sanarelli, criticized for his cruel and misleading experiments, was doubly wrong since his bacillus icteroides was a casual interloper, the hog cholera bacillus which did not cause yellow fever. After Lazear's tragic death Walter Reed was able to decipher his notes which gave the necessary clues. Governor General Leonard Wood supplied $10,000 to build an experimental camp and pay the volunteers. It was soon found that intimate exposure to the filthy gear, black vomit and bedclothes of yellow fever victims did not cause the disease. And the female aedes Egyptii did, if she had bitten a patient in the first two or three days of the disease and harbored the virus for two weeks. Welch suggested blood and an ultra filtrate of blood. Taken at the right time each produced the disease. So the first viral disease had been found in man, though the virus was not isolated till several decades later. Marvelous concatenations of observations, experiments and discovery brought a simple solution to a horrible plague. We hear that contemporary scientists and scholars stand on the shoulders of those who have gone before. I think rather that they kneel on their shoulders. The attitude of reverence for history is appropriate. Walter Reed brought and codified a new element for experimental subjects, that of understanding before consent in volunteering for deadly risk. The work and worry, the constant torment of fear that in addition to Lazear some heroic young man would die, consumed him. But not before he saw that the mosquito's destruction, launched by Kean and a doubting Gorgas, caused yellow fever to vanish from its long home in Havana, and before long the world.

Walter Reed added candor and caution to care and concern in his experiments on man. He introduced a covenant rather than a compact, a sacred scientific document. Thus he gave the substance and symbol of a written agreement to informed consent. This was the essence of the relationship of physician-investigator to subject and patient. In more ways than President Eliot realized, in the citation in Harvard's honorary Master of Arts degree, he deserved the accolade: "Walter Reed, medical graduate of Virginia, the military surgeon who in Cuba designed and carried out those experiments which have given man control of that fearful scourge, yellow fever."

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THE PHYSICIANS OF ROCHESTER, N.Y., 1860-1910: A STUDY IN PROFESSIONAL HISTORY, II*

EDWARD C. ATWATER

On the eve of the Civil War the medical profession in Rochester, New York, as in other parts of the country, was in disarray. A prior paper examined the early years of the medical profession in that frontier community.[1] It reported and discussed the fact that there were too many physicians, that virtually all of them needed additional means of support, that their educational and social status, which had been high at the beginning of the century, deteriorated as their economic security decreased and their geographical mobility increased. In contrast to the early decades of the period studied, only 20% of the 142 physicians who came to Rochester in the 1850s had been to medical school (including homoeopaths and eclectics) and only 17% joined the medical society. By 1866, only 8 of Rochester's practitioners were among the 455 men reporting income of $1,000 or more, and hardly more than one-third of them owned property.

During the half century after the Civil War substantial improvement occurred. Educational requirements were re-established and gradually raised. Economic prosperity, scientific discoveries (especially in microbiology), improved technology, and re-organization of professional societies promoted specialization and research. New hospitals, with professional nursing care, provided the scene for these activities and hospital staff appointments were much sought. The acrimonious contention between sectarian and regular physician, which had divided the profession for half a century, was resolved.

The Physicians. In Rochester, a city of 51,000 people, there were 95 active physicians in 1861, less than half of whom were orthodox practitioners with M.D. degrees.[2] The county medical society was virtually defunct. In the subsequent 50 years 914 more physicians came to the community, 50% of

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[1] E. C. Atwater, "The medical profession in a new society, Rochester, New York (1811-60)," *Bull. Hist Med.*, 1973, *47:* 221-35

[2] The physician data reported here are derived mainly from Rochester City Directories, newspapers, alumni catalogues and hospital records. A keysort card, made for each physician, included biographical and demographic data

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them doctors of the regular school and 17% of them homoeopaths. Except for members of the minor sects such as Eclectics, or Christian Science practitioners, who first came in 1893, or osteopaths, who appeared after 1898, the rest had no educational qualification and included itinerant surgeons, private disease specialists, clairvoyants, female magnetic healers who treated only men, electric physicians who specialized in amenorrhea, abortionists, plus, no doubt, some conventional healers.

By 1910, there were 401 physicians in the community of whom almost 70% belonged to one of several then active professional societies. Though the ratio of physicians to population had remained constant at about 1:500 throughout the period, the ratio of orthodox physicians with medical college degrees rose steadily from 1:1000 in 1875 to 1:660 in 1905, after which it declined again slightly, reflecting the decrease in medical school matriculation.

High mobility both within the community and between cities prevailed among those with little education or professional association. While only 20% of the orthodox M.D. physicians who came to the city during the 1860-1910 period moved away in less than six years, over 90% of unschooled practitioners did so. While 57% of M.D.s stayed more than 20 years, virtually none of those without formal medical training did.

Both at the beginning and the end of the period, about one-third of the orthodox M.D.s had attended the regional schools at Geneva, Buffalo and Syracuse. The most popular national schools were the University of Pennsylvania, the College of Physicians and Surgeons in New York, and Bellevue Hospital Medical College. Service as a house officer at one of the hospitals in the community became increasingly common as the best means of entry into the professional community for both regular and homoeopathic physicians.

For more than 20 years prior to 1874, Sarah Adamson Dolley (M.D. 1851) was the only *bona fide* woman physician in the community. In that year, other schooled women practitioners, both regular and homoeopathic, began to come and, by 1910, there had been at least 60 in Rochester.[3] Of the many who left, at least ten went to positions in state mental institutions. In 1876, women were admitted to the county medical society for the first time and, by 1890, 14 had become members.

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[3] Of these 17 were homoeopaths. In addition to the 60 M.D s, another 94 women practitioners without formal professional schooling came to the community between 1860 and 1910. Among those who had degrees from "regular" schools, Michigan, Woman's Medical College of Pennsylvania, Woman's Medical College of the New York Infirmary, Buffalo and Syracuse were the most popular.

From the time the first two general hospitals opened, St. Mary's in 1857 and Rochester City Hospital in 1864, the dominant faction in the medical community was the small group within the medical society who held staff appointments at one of these two institutions. The development of these hospitals had, of course, great influence on the medical profession.

The Hospitals. In Rochester the evolution of custodial institutions for the sick was similar to that in older cities, though accelerated. Forerunners of the general hospital were the pesthouse, the almshouse, and the marine hospital.[4] Rochester's first pesthouse was provided by the village trustees in 1823 in a "behind part" of the community so that a servant girl with smallpox might be isolated for the protection of the community.[5] Similar temporary rented facilities were established in subsequent years as necessary. Since there was fear of dangerous miasma and acts of arson were not unknown, this was often done in secrecy until 1869 when the more permanent "Hope Hospital" was built on the outskirts of the city for use during epidemics.

The Almshouse, or poor house, opened in 1826 after the Yates Report[6] advised that it was cheaper to care for paupers in institutions than in their own homes. Though the sick poor could seek help here, an air of disrepute was soon associated with the almshouse. It was a place where the "unworthy, licentious, drunken, vicious, lazy, habitual poor" were found and where the "worthy, hard-working poor" refused to go.

The permanent general hospital, a place where nursing care could be provided for the sick poor who would be only temporarily dependent on the taxpayer, was developed in an effort to solve this problem. Though there had been general hospitals in some larger cities after the middle of the 18th century it was not until the 1840s that they became common.[7] By that time, a class of anonymous urban industrial poor had developed and it had become increasingly difficult to care for them adequately. Rochester had the additional problem of transient immigrants and bargemen on the Erie Canal, the waterway which, along with wheat and water power, had made the city prosperous.

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[4] In Boston, a pesthouse was established in 1717, a workhouse for paupers in 1735, a general hospital in 1821, and an insane asylum in 1818. The first general hospital in the colonies was opened in Philadelphia in 1751, the first insane asylum at Williamsburg in 1768 Several seaport cities had marine hospitals after 1798.

[5] In 1821 a vaccine agent was appointed and in 1832 the taxpayers provided free vaccination for 700 inhabitants. *Rochester Advertiser,* May 20, 1828; *Anti-Masonic Enquirer,* Jan. 1, 1833

[6] *New York Senate Journal,* 1824, 95-108 and appendix A; or, New York State Board of Charities, *Annual Report for 1900,* Albany, 1901, I, 937-1145.

[7] The Pennsylvania Hospital, which opened in 1751, is generally acknowledged as the first general hospital in America although several cities had almshouses with infirmaries prior to this The claim of the Charity Hospital of New Orleans, founded in 1737, to being the oldest hospital hinges on the date by which it had evolved from such infirmary status to that of a bona fide hospital Opening dates of other early hospitals include New York Hospital, 1791; Massachusetts General Hospital, 1821; Maryland General Hospital (which later served as an insane asylum), 1797, Connecticut General Hospital, 1826, Commercial Hospital and Lunatic Asylum for the State of Ohio, 1821.

At first the sick poor had been cared for by the Female Charitable Society which had been organized in 1822 for this purpose. Each member was assigned a specific geographic area and was expected to know of and call, at least weekly, upon all the sick within her territory. By 1852 there were 50 districts and 73 visitors. The existence of this "active, untiring . . . band of Christian ladies, who seek out and relieve the indigent sick," made the need for a hospital seem less pressing and, in the view of one editor, retarded the start of one.[8] Nevertheless, by 1857, the municipal relief rolls included 2500 persons and more than a quarter of those requiring public assistance did so as a consequence of sickness in the family, according to the overseer of the poor.[9] There were only three city physicians to care for these sick paupers and they had then no public health nurses or social workers to help them. In June of 1856, for example, they made 265 house calls, twenty of them at night, and had 202 office calls.[10] A hospital seemed necessary.

In spite of earlier attempts to establish a non-sectarian general hospital, the first such institution in Rochester was opened by the Sisters of Charity in 1857. That the sick poor were predominantly of Irish and German birth, and hence of Roman Catholic persuasion, was at least partly responsible for this. Though the hospital, called St. Mary's, was open "to the sick of all denominations or of none" and many felt that further efforts for a non-sectarian hospital should be abandoned, the Protestant portion of the community feared lest Roman Catholics be thought to have a monopoly on "works of mercy and benevolence," and a second institution, the Rochester City Hospital, opened in 1864.[11] Had it not been for generous federal payments for soldiers who made up most of the hospital populations in the middle 1860s probably the second hospital would not have been opened and perhaps neither hospital would have survived.[12] With the local authorities reluctantly paying $1.25 a week for poor patients, the Federal Government paid $5.50 for its hospitalized soldiers.[13] By the end of the Civil War most of the patients were soldiers.

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[8] *Rochester Daily Democrat*, May 26, 1852

[9] *Union and Advertiser*, May 11, 1857; *Rochester Daily Democrat*, May 26, 1832.

[10] *Rochester Daily Union*, July 2, 1856

[11] In support of this argument a Rochester editor cited the experience in nearby Buffalo. A costly City Hospital, opened almost a decade after a Sisters Hospital, had never had more than 25 patients in it while the older institution had six or eight times that number. Editorial, Rochester *Union and Advertiser*, Nov 15, 1859, 2-2.

[12] See David B. Lovejoy's Osler Prizewinning essay, "The hospital and society The growth of hospitals in

After the war, both hospitals tried to attract private paying patients but the advantage was with the non-sectarian hospital which served the Protestant and generally more prosperous portion of the community.[14] The character of the two hospitals soon diverged, St. Mary's remaining the place for the sick poor, while City Hospital attracted not only donations and endowment, but also paying patients.

Positions on the staff of these hospitals conferred not only professional prestige and patronage, but, as soon as staff physicians were allowed to admit private patients, direct economic advantage as well. One evidence of this is the fact that in 1880 seven of the ten regular physicians having property assessments of $7,000 or more were senior staff members of one of the two hospitals. By the end of the century the hospital had become the seat of professional power. The struggle changed from one between homoeopathic and orthodox therapeutics to the matter of who would control hospital facilities.

The Homoeopaths. Rochester's first two homoeopathic physicians arrived in 1842.[15] One of them was accepted as a member of the county medical

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Rochester, New York, in the nineteenth century," *Bull. Hist Med.*, 1975, *49* 536-555 Before the War, St. Mary's had obtained authorization from the federal government for a marine Department which, in Rochester, cared for sick Erie Canal bargemen.

[13] St. Mary's cared for 2500 soldiers by 1866. In 1865, 75% of the total income of Rochester City Hospital came from the Federal Government for services to soldiers This had dropped to 7% in 1866. In the 5 years after the War, appropriation from the state legislature made up for some of this lost income. See Lovejoy, *op cit* (n. 12 above), p. 551

[14] The contest to attract private patients may be followed in the press In September 1865, St Mary's located, "among the suburban residences of the most wealthy inhabitants," advertised "all the seclusion and accomodations of a first-class hotel" including central steam heating, gas lighting, running water, a large physical therapy department, and a farm which supplied fresh milk, butter, eggs, vegetables and a place for fresh-air outings." The Rochester City Hospital responded by building a new wing, with private rooms which had large double windows, an elevator, and improved ventilation. "If," wrote a local editor, "there is any comfort in being sick, it can be had at the City Hospital " The annual bazaars of this institution became and long remained *the* charitable endeavor with which to be associated in the community until the organization of the Community Chest during World War I.

[15] Homoeopathy was the creation of Samuel C. F. Hahnemann, a well-educated German physician who practiced medicine in Leipzig for 42 years In 1790, while translating the *Materia Medica* of the Scottish physician William Cullen, Hahnemann realized that quinine, which was used to treat the chills and fever of malaria, produced those same symptoms when given in large doses to a healthy person From this he extrapolated the law of similars, *Similia similibus curentur,* let similars be treated by similars, or diseases are cured or should be treated by those drugs which produce in the healthy person symptoms resembling those afflicting the patient. This is the fundamental principle of homoeopathy and the word itself, derived from the

society only to be expelled two years later after state laws were changed so that anyone might legally practice medicine without having a license or without being a member of the medical society. With the advent of free trade in medicine, those physicians who considered themselves better trained and more orthodox were prompt in their attempts to exclude competitors whom they viewed as deviant.[16]

Most of the early homoeopathic physicians with formal medical education had received it at orthodox institutions and had later been converts to homoeopathy. Regardless of the theoretical tenets of homoeopathy, at least part of the struggle between regulars and homoeopaths was economic. With too many physicians to serve the needs of the community, homoeopathy made it possible for some physicians to differentiate (or distinguish, or specialize) themselves, as competitors providing similar services or products usually do, while others (the regulars) attempted to segregate or exclude their competitors by joining together in exclusive organization such as the American Medical Association and in academies of medicine and by refusing to consult with "unorthodox" (i.e. homoeopathic) practitioners.[17] The most effective weapon which the orthodox profession had at first was control of hospitals.

During the Civil War, orthodox members of the medical profession had been able to prevent official participation of homoeopaths in military hospitals. Subsequently they were also successful in excluding homoeopaths from the staffs of civilian hospitals, although there was sometimes disagreement in this matter between physicians and hospital managers. In Rochester in 1866 the Board of Lady Managers of the City Hospital, struggling to make ends meet as federal payments for soldiers decreased, were in favor of a liberal policy and agreed "that all or any patients in the private Wards or Rooms who are paying residents, may have the privilege of calling their own

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Greek for "like feeling," refers to this therapeutic concept. Other features included the idea that high dilutions of a drug could initiate the natural body defences through a dynamic process resembling the action of an enzyme. See *Encyclopedia Britannica,* 11th ed., 1910, "Homoeopathy," *Organon,* the bible of homoeopathy, was not published in the United States until 1836, a quarter century after its original publication in Germany. The practice had been introduced to America by Hans Birch Gram in 1825.

[16] This repeal was in response to demands of the Thomsonian botanic physicians, a wholly self-trained sect, for separate but equal recognition and privileges to those accorded the orthodox profession. Members of the latter group preferred to see "free-trade" in medicine in preference to such recognition. Similar demands for professional status would come from the homoepaths after the Civil War, the osteopaths at the turn of the century, and, more successfully, the chiropractors in the 1950s

[17] The American Medical Association, after 1855, required an oath of its members not to consult with irregular practitioners.

[18] Minutes, Rochester City Hospital, April 23, 1866.

Physician, of whatever practice but under the general rules of the Hospital."[18] However, the following year, when the wife of a prominent local citizen wished to be admitted to the hospital under the care of her homoeopathic physician, staff opposition brought a policy reversal on the basis that "mixing two systems of practice in the same institution would be incompatible." By a vote of five to two, homoeopaths were permanently excluded from the Rochester City Hospital.

That same year, the homoeopaths proceeded to organize their own medical society; among its goals was the establishment of a hospital. Throughout the 1870s and 1880s the homoeopaths became increasingly successful practitioners in the community, though remaining small in numbers. In 1880, 29% of the 17 physicians with property assessed at $5,000 or more were homoeopaths, but this group made up less than 10% of all practitioners and only 17% of the regular and homoeopathic group combined. Though there was only one homoeopath for every three members of the regular medical society in 1892, the fact is not a measure of their influence. Many of the more recently prominent and prosperous citizens, including Hiram Sibley, organizer of Western Union and the richest man in town, to say nothing of bank presidents and the two leading dry goods merchants, employed homoeopathic physicians. Hospital facilities were not available to these people and it is not surprising that they should sponsor the opening of two new hospitals in 1889.[19]

By this time doctrinal disputes had already created two factions in the homoeopathic camp. The smaller, conservative group, under the leadership of Dr. Joseph A. Biegler, son of the city's original German-born homoeopath, had formed the Rochester Hahnemannian Society and opened the Hahnemann Hospital. Originally eight in number, and never more than twenty, these men viewed themselves as "legitimate Hahnemannian homoeopathists" and disavowed "all the innovations which have attempted to be foisted upon homoeopathy by the mongrel sect," meaning the larger liberal faction.[20] This claim was reinforced a few years later in a broadside which stated that "the very *life* of Homoeopathy here, was at stake" because most members of the other faction "were openly using not only Alleopathic measures, but were continually chasing after every new fad and patent medicine that unscrupulous manufacturers are constantly invent-

[19] Homoeopathic hospitals had already been opened in other cities: New York, 1869; Albany and Buffalo, 1872, Boston, 1876. Chicago's Cook County Hospital admitted homoeopaths to its staff in 1881, and Boston City Hospital did this in 1886.

[20] Rochester *Union and Advertiser*, Jan. 19, 1886.

ing."[21] Needless to say, strict adherence to the classical rules of homoeopathy were followed at the Hahnemann Hospital and it was claimed that the institution was "*the only one in the world* where Hahnemann's teachings are faithfully observed."[22]

The other institution, the Rochester Homoeopathic Hospital, was dominated by the larger liberal Monroe County Homoeopathic Society, and was under the most exclusive financial sponsorship of various branches of the Hiram Sibley clan for decades.[23] It was, as the conservatives charged, homoeopathic in little more than name and the fact that its staff members had attended homoeopathic schools. Examination of patient records from the Rochester Homoeopathic Hospital for the period 1893-1910 show that conventional drugs were used in conventional doses, including even the occasional use of narcotics.[24] Though its homoeopathy might be questioned, its local leadership in innovation could not be. In 1895 it was the first institution to provide ambulance service. On October 9, 1896, it brought clinical roentgenology to the community only months after Roentgen announced his discovery in Germany. In 1897, its new surgical facilities were said to compare favorably in everything but size with those found in major cities, and appendectomies (none in 1894, 300 in 1910) and other abdominal invasions became reasonably safe if a bit too common. Also in 1897, the Homoeopathic Hospital upgraded its nursing school from the usual 2 years of domestic training and 12 hours of daily duty. By building a nurse's home, it was able to extend its course to three years, accept older girls, and start a curriculum similar to that used at the University of Pennsylvania which included courses in food chemistry, hospital management, special diets and physical therapy. The student, assured the prospectus, would become "almost a practical physician."[25]

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[21] Broadside, "An Appeal to the Friends of Homoeopathy," ca. 1890, Archives of Highland Hospital, Rochester, New York.

[22] *Hahnemann Advocate,* Rochester, N.Y., May 1901, Feb. 1903

[23] Personal and social factors played important roles in attracting support for hospitals The original $27,500 for Hahnemann Hospital had been given by Nina Hargous Appleton of Boston, "in grateful recognition of the services to Dr Biegler to herself and her family." Mrs. Appleton's mother had died in a Rochester hotel since no hospital facilities were available to Dr Biegler The Hiram Sibley family, who gave most of the original money for the Homoeopathic Hospital, had lost the two eldest male heirs to diphtheria in 1886 and 1888, and Hiram himself had died in 1888.

[24] Epilepsy was treated with potassium bromide, acute rheumatism with colchicine 1/100 gr. every four hours, "irritable heart" with digitalis and nux vomica, phlegmonous tonsillitis with calomel or mercury, asthma with ipecac and stramonium powders, alcoholism with paraldehyde, bromides, chloral, light diet, and whiskey every three hours Morphine was used, though rarely. By 1915, "twilight sleep" and radium therapy for cancer were being used.

A combination of young enthusiasm, ambitious and prosperous supporters, and a cause, had propelled this sectarian hospital into a period of prominence. The centralization of charitable giving in a Community Chest in 1917, with apportionment of funds to hospitals on a basis of patients served, the permanent return of the income tax, and the interest of George Eastman, Rochester's newest rich man, in the City Hospital (by now renamed the General) helped to reestablish the leadership of that older institution in the 1920s.[26]

Medical Laws. The gradual reunion of the homoeopaths and the regulars after 1870 is apparent from the changes in laws related to medical education and practice. In 1844, the County Society had purged a homoeopath and, in 1855, nonfraternization with homoeopaths became obligatory for members of the American Medical Association, the rule cloaked in a code of ethics. But in 1872, the state established boards of medical examiners in an effort to upgrade educational requirements. By allowing each major sect to examine its own candiates, the civil authorities gave *de facto* recognition and legitimacy to the homoeopaths. During the subsequent 21 years, though educational requirements became increasingly stringent, a therapeutic orthodoxy was never recognized by the government.

The rigid stance of the regulars toward their homoeopathic colleagues gradually softened. By 1884, a liberal minority of members of the State Medical Society (including many leaders of the profession) had come to feel that any formal code of ethics was inappropriate for gentlemen and opposed further obligatory support of the AMA's non-consultation oath. Unsuccessful in persuading a majority of their fellows, they seceded from the society and formed the New York State Medical Association. It took another 20 years for abandonment of the code and reunion to occur.

The earliest recorded evidence of co-operation between "regular" and "homoeopath" in Rochester occurred on a cold Friday, October 5, 1888.

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[25] Rochester *Democrat & Chronicle*, Sept 23, 1897. Prior to this time nurses were little more than domestics. In 1897, it was estimated that two-thirds of nurses in American hospitals were foreign born Rochester City Hospital was challenged on more than one occasion by immigration authorities for employing alien Canadian student nurses who were willing to sign a two-year contract in return for nominal pay which the hospital largely recovered in charges for books and uniforms Rochester *Democrat & Chronicle*, Apr 1, 1897.

[26] In 1909, Eastman, inventor of flexible dry film and the Kodak camera ("You press the button, we do the rest.") gave $500,000 to the Rochester City Hospital. This gift, ten times the amount of the largest gift which had brought prosperity to the Rochester Homoeopathic Hospital in the two decades preceeding, made it possible to virtually rebuild the hospital

The homoeopathic physicians had publicly challenged "any nine from the allopathic physicians of the city to a game of baseball" to raise money because of the yellow fever epidemic. The challenge was accepted "by the physicians of the old school" who as the "blue pill" team roundly defeated the "little pill" team, 38 to 10.[27] By the 1890s, the regular, Homoeopathic and Eclectic medical societies were able to co-operate in efforts to enforce the State registration law.

By 1908, a homoeopath had joined the orthodox medical society and, two years later, a regular had become a member of the homoeopathic medical society. Within the next five years the older men began to follow suit and by 1925 there were few separatists left. By the early 1920s, orthodox physicians were joining the staff of the two homoeopathic hospitals, which soon adopted new names omitting any doctrinal connotation. Diagnostic laboratories, clinical chemistry and full-time pathologists had become, by this time, the new innovations and previously homoeopathic institutions were prompt in establishing them.

The opening of homoeopathic hospitals, one of which had become for a while the leading hospital in the community, and the popularity of homoeopathic physicians among the prominent had made non-recognition by the "regulars" futile. Restriction of the number of new physicians after 1905 brought increasing prosperity to the profession generally. The importance of pharmacologic doctrine was eclipsed by new technology: aseptic surgery, radiography, trained nursing and isolation.

Professional Organizations. The changes occurring in medicine were also reflected in professional societies. In addition to a regulatory, educational or social function, professional organizations provide a means of establishing, stabilizing, and publicizing a professional hierarchy, when there are so many members that spontaneous social management becomes difficult. The changing identity and character of the dominant faction may be appreciated by examining these organizations.

Prior to the Civil War, the state medical society and its component county medical societies, chartered in 1806 for the purpose of protecting the public from the imposition and the physicians from the competition of quacks, provided educational and social intercourse. After the charter repeal of 1844, the county societies withered since they had lost their coercive authority and membership in them no longer provided legal or economic benefit. The American Medical Association and the New York Academy of Medicine were examples of new professional organizations which arose in the late 1840s in the wake of this collapse and were, at least in part, efforts to reconstruct a professional hierarchy.

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[27] Rochester *Union and Advertiser,* Oct 12, 1888, 3-1

The Rochester Medical Society, started in 1853, was the local version of such groups, it included only those with medical school diplomas. It excluded homoeopaths, the foreign-trained Irish, German or Dutch physicians, and Sara Adamson Dolley, Rochester's first and one of America's first women to graduate from medical school.[28] It included the most prominent physicians of the day and its members dominated the staffs of the two hospitals when they opened.

The biweekly meetings, held usually at the home of a member, and attended by about half of the 35 members, consisted of an elaborate dinner, a formal paper and commentary. Gradually the leading spirits died, and by 1878 the secretary was "compelled to give a report which shows such apathy . . . that there must be a renewal of interest or the usefulness of the society is at an end." In January 1881, at what turned out to be the second-to-last meeting, 8 members were dropped from the rolls for having missed more than 6 consecutive meetings.[29]

The society was the victim of young Turks who, ten years earlier, had formed the Rochester Pathological Society "for social intercourse with mutual improvement in the medical and kindred sciences." The Rochester Pathological Society members lunched together biweekly at the Rochester Whist Club, conducted their business, heard a report on diseases prevalent in the community, listened to a prepared paper, and viewed some pathological specimens. They also occasionally advised and hoped to influence public authorities on medical matters. By 1900, membership had grown to 114.

In 1872 this society opened the first free dispensary in the city, which functioned, except for a few years, until the late 1880s when the city attorney advised that the customary public appropriation to support it was illegal. This and two other dispensaries, the Homoeopathic in 1874 and the Provi-

[28] Dr. Dolley was born a Quaker, trained in an Eclectic school (M.D 1851, Central Medical College, Rochester, New York), married one of her professors, practiced medicine in Rochester until her death in 1910, and was the moving spirit of the Practitioner's Society, later renamed the Blackwell Medical Society, which was one of the four original component groups of the New York State Woman's Medical Association.

[29] Minutes of the Rochester Medical Society, Rochester Academy of Medicine Library. The records of the Rochester Pathological Society, the Rochester Academy of Medicine, and the Rochester Medical Association are also in the Academy Library. Records of the Provident Dispensary and its sponsoring organization the Practitioners Society (later the Blackwell Society and component of the successor New York State Woman's Medical Association) were recently given to the Edward G. Miner Library at the University of Rochester School of Medicine and Dentistry.

dent in 1886, the latter run by the women physicians in the community, provided free care for the sick ambulatory poor and were staffed by physicians who did not have hospital appointments. After the opening of outpatient departments at the City Hospital in 1888 and the Homoeopathic Hospital the following year, only the Provident Dispensary continued. It closed in 1898 after a woman was appointed to the Outpatient Department staff at Rochester City Hospital. The Pathological Society was started by ambitious men denied hospital access and remained dynamic until hospital staff opportunities were assured.

In 1900, still another society, the Rochester Academy of Medicine was organized for "the cultivation and advancement of the science and art of medicine, including the development and maintenance of a medical library." The original members of the Academy were predominantly those members of the Rochester Pathological Society who also held staff appointments at the Rochester City Hospital, St. Mary's or Rochester State Hospital, though some who were eligible by these criteria but who had been critical of the professional leadership of the community were not included. Two women were included for the first time, but homoeopaths were not.

During the year before the Academy's birth, one Rochester pathological Society speaker had drawn "a vivid picture of our failure in dealing with others of the profession" meaning, of course, the homoeopaths. The discussion which followed, recorded by the secretary in unusually brief notes, "leaned to the optimistic side and that we are not so bad as we're painted." Another speaker was critical of the unfair professional and economic competition which the hospitals gave to all practitioners by admitting employees of railroads and other "rich corporations" on the free ward service, thereby excluding the personal physician from participation in their care. "Most of the benefits, of a hospital," he felt, "accrue chiefly to the members of the staff and not the so-called 'outside' doctors." Neither of these dissenters, both of whom were on the staff at City Hospital, became Academy members. The majority felt that the existing system was the most economical way to provide hospital care.

The Academy had begun at least partly as a response to threats such as these and that of the Rochester Homoeopathic Hospital itself to the professional leadership of the senior staff of the long-powerful Rochester City Hospital. Though it abjured the frivolity of meals and promoted a library, such modifications hardly required a new organization. Beside consolidating the power of the City Hospital staff in the community, the Academy organized an elite group which furthered the development of the specialization which promoted many of the scientific advances of the early 20th century.

Though specialists existed before the advent of hospitals and, later, continued independent of them, it was the hospital which made real development and progress possible. Except for general surgery, which did not invade the abdomen until the end of the 19th century, ophthalmology was really the first specialty to develop. As early as 1825, an unsuccessful effort had been made to establish a Rochester Eye Infirmary. In 1837, a reputable local practitioner, George Edwin Munn, restricted his practice to eye problems, prospered, and became known far beyond his locality.[30] Soon after the Civil War the Rochester City Hospital had an EENT specialist on its staff.

It was not until the late 1890s, however, that specialism really became common. Between 1897 and 1910 one or more of the hospitals (except the conservative Hahnemann Hospital) added to their staffs men specially trained in ophthalmology, neurology, dermatology, obstetrics, pathology, laryngology, orthopaedics, radiology and anesthesia. With few exceptions, these specialists and the senior staff physicians and surgeons were members of the Academy.

Academy membership carried great prestige. Fewer than 10% of the physicians in the community were members and even the most prominent homoeopaths were steadfastly excluded. In 1914, the outsiders formed the Rochester Medical Association. The larger, more democratic membership of this organization, which included homoeopaths, made possible a prosperity which the Academy had never enjoyed. Soon the Medical Association had its own building.

The coming of a medical school and its hospital in 1925 further eroded the power of the Academy by introducing into the community a whole group of specialists not beholden to the local hierarchy. Though token faculty appointments as lecturers and consultants were made from among the community—almost exclusively from those on the General Hospital staff who were Academy members—it was inevitable that the balance of power would shift from the hospital staff to the medical school. In 1930, the Academy and the Association composed their differences and amalgamated the prestige of the former with the prosperity of the latter. The Pathological Society gradually became a social organization, the Academy emphasized continuing education, and the Medical Society dealt with economic and political matters. The medical school, with increasing control of hospital staff appointments, became the dominant professional organization.

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[30] Howard A. Kelly and Walter L. Burrage, *American Medical Biographies* (Baltimore Norman Remington, 1920), p. 834

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The event which had the greatest influence on the medical profession between the Civil War and World War I was the centralization of professional power in the hospital staff. Advances in physiologic understanding, therapeutic technology, and private philanthropy made this development possible. Arguments over codes of ethics, therapeutic systems and irregular practitioners, which had seemed so important from the 1840s till the 1880s, faded as it became more important to have hospital privileges than to foreswear consultation with homoeopaths. The professional prestige and the economic reward were here. One reason for the deterioration of medical education in the late 19th century may have been the redirection of the interest of talented men from the professorial role, where prestige had earlier been, to that of senior consultant on a hospital staff. It was for another generation to see the contest to arise between town and gown as a result of the fulltime controversy. It is likely that the transition of the medical profession in Rochester was characteristic of that in many American cities.

OF WHAT USE IS MEDICAL HISTORY?*

LESTER S. KING

When medical students begin their clinical studies, they learn the elements of physical examination and they also learn how to take suitable history. What brings the patient to the physician? How did the present illness start? What were the patient's previous illnesses? his occupation? the family background? possible psychological stresses? and comparable data. To the student all this is very exciting, for it brings him into immediate contact with real patients, and lets him glimpse the vastly complex relationships that attend any given illness.

To be sure, as the student matures and gains experience, he will spend less and less time questioning the patient until, when well established in practice, he may leave the history taking to the nurse (or even to a printed questionnaire that the patient himself fills out). At this stage the once enthusiastic student now has doubts about the value of a detailed history and may shunt it to a minor position. In teaching hospitals the histories are usually left to the most junior house staff, while in community hospitals the histories are often so negligible that the hospital accreditation may be threatened.

How can we account for this change in attitude as a second-year medical student turns into the busy practitioner? There are several possible answers of which I will suggest two.

First, we might say that the history actually has little intrinsic importance. But for the medical student any facet of clinical medicine has novelty and glamour, so that the actual low status of the history is concealed until the novelty wears off.

Second, we can say that a clinical history has great significance, but not in all cases. It has value only where it helps materially in finding out what ails the patient. It then helps to solve a concrete problem, namely, making the diagnosis. The physician, as he gains experience, may find that he can solve the problem—determine the patient's ailment—by using not the history but other sources of information such as physical examination and laboratory data. To be sure, an unusual clinical problem may require a highly detailed

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history, but for the most part this is not necessary. The mature physician, facing a clinical problem, uses history only insofar as it helps in the solution.

If, then, a sceptical medical student asks, "What good is a detailed history of a patient?" we must answer, that it provides a certain amount of *input* into the total problem-solving process. Sometimes this particular input has crucial importance; more often it does not. If you suspect that the history will help, you work hard at getting a good one. If you feel it will not help, you ignore it. But since a good history is not easy to elicit, you must get a good deal of practice learning what questions to ask and how to judge the answers.

II

If the clinical history has a definite practical function in solving individual clinical problems, does history in its most general sense have a comparable function in the world at large? There is one school of thought that offers an affirmative and quite popular answer, namely, that whoever does not learn from the mistakes of the past is condemned to repeat them. Such a formulation implies that a knowledge of history helps us to avoid error, and thus has an intensely practical function.

Such a statement, I believe, introduces value judgments and inferences that are unjustified. By what warrant do we call a particular historical event a mistake? Can we say that there was a "right" course or a "wrong" course? Do events have a sufficient degree of cyclical recurrence, that the "lessons" of one era will "apply" to another? Into any dispute on these topics I will not enter. Let me merely say that I reject the proposition that if we do not learn from the mistakes of the past we are condemned to repeat them.

Yet to reject this popular statement does not automatically deny any practical value to history. This we can see if we return to the model already discussed. Just as a clinical history may furnish important data in treating a patient, so in situations far removed from medicine, history can provide considerable input that may help in reaching an eventual decision.

This aspect was recently discussed by Dr. David Musto, in an informal meeting whose proceedings may eventually be published but which at the present are still in the stage of "personal communication." Dr. Musto described the role of professional historians in government and their part in the eventual decisions that government agencies must make. In difficult policy decisions innumerable facts may be relevant, drawn from the past as well as from the immediate present. The historian himself does not have the decision-making power, but he can supply relevant historical information to those who do make the decisions. The historian, by virtue of his training, can point to events in the past that bear directly on present problems. For example, in a topic such as narcotics control, if some recommended course of action had already been tried in the past and failed, then the circumstances under which this took place would surely be relevant to a new decision. The actual decision itself rests with an executive or "bureaucrat" who may or may not pay attention to the historical input.

I suggest a precise analogy with medical practice. Just as the clinical history of the patient may have considerable bearing on the diagnosis and ultimate treatment, so too if the health of the body-politic seems ailing, and something must be done, history may have relevance. But how much relevance? The answer here must come from an executive, not an historian. The history provides only a part of the total information on which the final decision must rest. Determining the degree of relevance involves judgment and decision quite distinct from the data-gathering of the historian.

If our hypothetical student asks, "What good is history?" we can, then, give one partial answer—it *may* provide data for reaching practical decisions. History *can* be "practical," but this, in my opinion, is only a relatively small part of our total answer regarding the value of history.

III

To find a more satisfactory answer for our impatient student, let us try to clarify the nature of history. I would begin by referring to Santayana, who, in his five-volume *Life of Reason* devoted a chapter to history in the first volume. For him history starts with memory, but while memory "is the basis of historical knowledge, it is not called history until it enters a field where it can be supported or corrected by evidence." Memory needs corroboration, and for this "it must refer to some event in nature, in that common world in space and time to which other memories and perceptions may refer also." We thus combine the subjective and the objective. Memory is subjective, corroboration is objective. Santayana regarded historical investigation as comparable to the natural sciences but with its scope in the past. Testimony, whether in history or in science, must be interpreted according to the laws of evidence.

We do not need to discuss whether history is or is not a science; nor need we get exercised over the topic, whether it is in essence subjective or objective. A great deal has been written on the "subjectivity" of history. I personally adhere to the views expressed by Carl Becker, in his marvelous essay, "Everyman his own historian," but into the details of the controversy between the subjectivists and the objectivists, I will not go. For our purposes it is much more important to discuss the relation of history to "facts."

The term "fact" has many meanings, which we can best appreciate through various antonyms and different usages. Thus, we oppose fact to theory, fact to interpretation, fact to fiction, fact to imagination. We speak of the bare facts, we urge enthusiasts to let the facts speak from themselves. Indeed, we can compile a fine set of mixed metaphors: that facts are sacred, they are the touchstone of truth, the rock on which error founders, but likewise the rock on which an enduring edifice can rest. And fact is something observable by any competent observer.

For many historians the goal of history is to establish the facts, i.e., find out "what really happened"—*Was eigentlich gewesen*. This dictum of Ranke no longer finds literal acceptance, yet the concern with facts seems to dominate historical thinking. At the same time it represents the precise feature that repels so many students from the study of history, from the inner beauty of the discipline.

I suggest a definition of "fact," or at least of what we *call* a fact, namely, *an assertion about which there is no dispute*. Since we communicate through language we must do so in terms of assertions and not of any *Ding an sich* that may lie behind the assertions and which, by definition, is beyond knowledge. When I say that Julius Caesar crossed the Rubicon in 49 B.C., no one will dispute it, nor when I say that William Harvey published his *De motu cordis* in 1628 will there be any debate. We can elaborate these bare statements and give more precise dates and other details which, again, no one will dispute. In all this we are recounting facts that can be corroborated by all the rules of evidence.

But at this very point the sceptical medical student may say, "Suppose that Caesar *did* cross the Rubicon. So what?" and to this question we have no immediately helpful answer. An assertion on which everyone agrees is, indeed, remarkably uninteresting.

Let us approach this problem by pointing to that massive compilation of facts, the telephone directory which, except for possible typographical error, occasions no dispute. Now, a telephone directory can serve an extremely practical function, namely, it can facilitate communication. If we must make a telephone call, the directory is invaluable. The facts in the telephone book may have high practical usefulness, *but only in certain contexts*. If we change the context—if we do not want to make a telephone call (or find an address)—then the usefulness of the telephone directory would be negligible. If the sceptical student should ask, "Of what use is a telephone book that is old and relates to a distant city?" we would have great difficulty in producing a satisfactory answer, at least, if we seek a practical function.

This is a significant proviso. In a suitable context the telephone book—or a table of logarithms or a handbook of physical and chemical constants—has great practical value, and so, too, may some data of medical history in certain restrictive contexts. But while the history of public health, for example, might provide input for legislation, a mass of facts about Galen will not help a surgeon remove an appendix more skillfully. We must freely admit that "facts" of medical history have negligible practical value for the medical student or practitioner, and I concede to our sceptical student that historical data, for the most part, have as much practical value as an out-of-date telephone directory of a distant city.

Clearly, if we want to convert our sceptic to our way of thinking, we must turn to other considerations. We can, for example, point to the way that history enlarges our experience. Youth tends to be simplistic and to ignore the complexities that are so real to the older person. What we call maturity we ordinarily attribute to greater experience which, in turn, permits greater insight into the relations of things, into connections and significance, similarities and differences, relevance and applicability.

I suggest that the study of history enormously enlarges the experience of the individual, allowing him to transcend his personal limitations and to encompass the experience of those who have lived before and who have left appropriate records. For the student of history the limitations of space and time disappear. History does provide a rich background, a broader web of relationships for interpreting the new.

IV

But there are more profound values. We can regard history as both a craft and an art. These terms have a long and complicated overlay of meanings, and I will indicate specifically those to which I refer. By "craft" I mean an activity that requires a substantial technical proficiency; by "art" I mean an activity that involves an aesthetic experience. We can, perhaps, speak of the technical and the aesthetic aspects of history. These, although in essence closely interconnected, can to some extent be separated for discussion. In the realm of music, for example, a pianist must spend years in acquiring technique; but he must also have that quality we call "musicianship," the ability to feel and transmit an aesthetic experience. The technique is only a means; the aesthetic experience is the end, but it requires technique to be manifested. When critics evaluate the performance of any given pianist, they take into consideration both of these factors.

In history the techniques are rigorous. They revolve around the problem of evidence. What are the documents (both published and unpublished)? What is their reliability? their consistency? We speak of the sources and we distinguish the primary from the secondary, and of course, both are important. The historian must know where the sources lie; he must know how to find them; he must know where and how to uncover any untapped sources; and he must acquire vast substantive knowledge of their contents. He must know not merely where they are but what they say.

Further, he must build his critical judgment and establish a basis for evaluation and comparison. He must pass judgment on the sources and their credibility. What is reliable? what is not? The student is not born with a knowledge of these topics. He must learn them, and the learning is an arduous task. Universities offer special courses dealing with historical methodology, an introduction to the technique of historical research. This technical training enables the budding historian to gather data. We may, in a loose sense, say that he is learning to dig out the facts.

But what is he going to do with the data? Here we come to what I call the art of history. Its essence lies in the imagination, in the creative combination of terms and relationships, guided by insight and illumination. The historian starts with a problem. It is easy to say that he then collects data bearing on that problem, but such a statement is rather meaningless. Data are infinite. The craft of a historian involves finding, selecting, and evaluating the evidence. The art of the historian entails the perceiving of relevance and constructing a theory or concept that reflects his imagination and critical judgment. All this comprises what we may call "interpretation." The essence of history lies in interpretation, a word that hides a great semantic complexity.

In a dictionary definition, to interpret is to explain, render clear, expound, provide meaning. But I would call attention to certain other features. Thus, interpretation immediately implies more than one possibility. It indicates variations and alternatives among which to choose. It entails a selection of one alternative and rejection of all others. Furthermore, the alternatives must be real, so that they have supporters or potential supporters. Without live alternatives and real supporters, we would have assertions in which there was no disagreement, i.e., "facts." Hence I suggest that potential disagreement represents one major distinction between facts and interpretation.

A second major distinction relates to complexity. A "fact" ordinarily is simple, for complexity tends to breed dispute, and the greater the complexity the less the chance of general agreement.

V

Let us return to an example already mentioned. I noted previously that the statement "Harvey published his *De motu cordis* in 1628" is a fact. It is simple, completely consonant with all the evidence, discordant with none. And at the same time it is utterly dull. Suppose, however, we say, "Harvey discovered the circulation of the blood in 1628," we no longer make a universally accepted assertion, no longer achieve a unanimous consent. Agreement hinges on particular shades of meaning that we attribute to the terms. "Discovering" is not the same as "publishing," and it is difficult to decide when Harvey actually reached his conclusions. Furthermore, what do we mean by the word "discover"? What do we mean by "circulation"? These terms demand "interpretation" which may take different forms. Some historians may dispute the degree to which Harvey's predecessors "really" had the concept of the circulation. What about Servetus, or Cesalpino? Our apparently simple statement we must *interpret* and thus bring in a series of qualifications and provisos. Instead of a fact we then have a complex relationship whose acceptance depends on whatever specific qualifications and provisos we make. The interest and significance of an assertion depends on the interpretation to which it can give rise.

If we can return now to our collection of old telephone directories, we find historical value in certain potential interpretations. Thus, a series of telephone books may serve as a basis for specific concepts in history, in the same way as have other masses of data, such as deeds and wills, tax records and parish registers. Documents such as these, suitably studied and interpreted, can tell us much about particular social developments. But when this happens we deal no longer with simple facts, but with a host of complex inferences, interpretations, and theories, imaginative constructs and bold hypotheses that far transcends the "bare facts." The elaboration, the interpretation, or, if you will, the theories, take on a significance provided by the mind of the historian. The "bare facts" are interpreted into a conceptual elaboration very different from the bare facts themselves. It is the elaboration, the interpretation, that makes an appeal.

The sceptical student who finds no significance in bare facts is on the right track. He may, however, find some real interest if the data are brought into constructs or interpretation that reflect the insight of the historian.

But even though the historian produces a finely wrought interpretation that explains the data and welds them into a coherent whole, this alone need not convince the sceptical student that history has value. The important question is, does any given study make an appeal? Does the imagination of the historian communicate with the imagination of the student to reach some sort of congruence?

Here we make contact with the world of aesthetics and what I have called the art of history. We can illustrate the point through any of the commonly recognized arts, such as painting, music, poetry, and the like. Any of them will appeal to some groups, leave others unaffected, and actively repel still others. The jazz combo that entrances the teenager may drive the parents to distraction. On the other hand, the Bach afficionado may find that his love of harpsichord music is not generally shared by his colleagues. Why do we find some art forms attractive, others repulsive? Most of us feel that Milton was a great poet and that Edgar Guest was not, that Ansel Adams is a better photographer than Aunt Susie who brought back snapshots of her trip to California.

And similarly with history. We recognize good historians and bad historians. To define these terms would require a whole book but I can at least emphasize the importance of technique and of imaginative insight into the nature of things. Technique alone is sterile. The art lies in having something worthwhile to say, and saying it in a form that will strike a responsive chord in someone else. The bad artist may lack adequate technique, may express only what is trite and superficial, or may fail on both counts. This contrasts with the artist who conveys a sense of universality and depth, and does it skillfully and agreeably. The good artist can evoke a response of pleasurable acceptance and make his audience feel an expanded intellectual or emotional horizon.

I believe that the good historian will fit into the concept of the good artist. He provides an insight into the nature of things as he perceives it—and this is precisely what we mean by an interpretation of data. Critics may agree or disagree, may prove him incorrect in one or another detail or conception, but this need not interfere with the merit of the historian. A great pianist can strike wrong notes and still be a great pianist.

The historian I would like to compare with the photographer who, in a series of pictures, illustrates some activity or locale. He tries at once to capture the inner feeling of his theme and yet achieve a balance with reality—as he sees it. Some of the results hold great beauty and interest, others are quite banal. The photographer faces an infinity of details, all intermingled, all constantly changing, and among them he must constantly choose. He must somehow capture the "decisive moment," the significant particular, the facet that seems to him most expressive and meaningful. Every great picture reveals some fragment of universality. And if the photographer has seen clearly, and if his technique is good, then he can convey to his viewers the significance that impressed him. Some photographers succeed in doing this, others do not. While many people take pictures, there are relatively few great photographers.

The photographer faces the present, the historian faces the past. There are many similarities between the two professions. Both face an infinity of facts among which they must make a selection. Both must have insight and creative imagination. Both must see meaning and significance, both must find some essence that distinguishes art from the casual or merely pretty. Just as no two photographers working on a given assignment will come back with the same series of pictures, so no two historians studying the same subject will see or describe it in the same way. Every historian has a unique point of view, and we know that the personal differences will be reflected in the final results. So close do I consider the analogy between photography and history that I would suggest a definition: I would call the historian "the photographer of the past."

Let us now return to our sceptical student who asks the question—the legitimate question—"Of what use is medical history?" Medical history., of course, is only a special case of history in general.

I would answer his question by considering history as an art. As such, history presupposes its own standard of technical mastery. When these standards have been met, the art of history can enlarge the experience of the audience in a way that is its own reward. It provides an enrichment of life. Through interpretation of data, history gives insight into the nature of things past, into their course and development, their reasons and interrelations. It affords intellectual and emotional expansion, and does so in a pleasurable and satisfying manner. And if history fails to do so, perhaps the fault lies more with the historian than with the student.

Just as the man who does not care for music or literature or fine arts leads a deprived life, so too does the man who does not care for history. Good history, like good music, is its own reward.

In this connection I must refer to Milton's *Comus*, where he described philosophy. But where Milton used the word "philosophy," let us substitute the word "history."

> How charming is divine philosophy [divine history],
> Not harsh and crabbed as dull fools suppose,

AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
REPORT OF THE ANNUAL MEETING*

Galveston, Texas
May 12-15, 1976

The Forty-ninth Annual Meeting of the American Association for the History of Medicine took place in Galveston, Texas, May 12-15, 1976. Sessions were held at the Flagship Hotel, the headquarters hotel, at the University of Texas Medical Branch, and at the Jesse H. Jones Library Building, Texas Medical Center, Houston, Texas. They were preceded on May 12 by two continuing education courses co-sponsored by the A.A.H.M. and the Institute for the Medical Humanities in cooperation with the Continuing Education Division of the University of Texas Medical Branch at Galveston. Dr. Robert Hudson's course discussed "Major Concepts in the History of Disease" while Dr. John Duffy's was devoted to "The History of American Medicine."

This report, which includes the program and abstracts of the minutes of the Council and Business Meetings, has been prepared by the Secretary-Treasurer, Dr. Genevieve Miller. The official minutes and complete financial reports are preserved in the Secretary's office where they may be examined by any member upon request.

PROGRAM

Wednesday, May 12

3:00 p.m. Council Meeting
5:00—7:00 p.m. Registration and Reception

Thursday, May 13

9:00 a.m. Opening Session
Chester R. Burns, *presiding*
Welcoming Remarks
William C. Levin, President,
University of Texas Medical Branch
Truman G. Blocker, Jr., President Emeritus
University of Texas Medical Branch
Presidential Address
"Of What Use is Medical History?"
Lester S. King, Chicago, Ill.

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* The Association held its 49th annual meeting in Galveston in 1976, which was the 51st year of the Association. The Philadelphia meeting in 1975 commemorated the Association's 50th anniversary, but was its 48th annual meeting since no meetings were held in the war years 1944 and 1945.

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10:15 a.m. General Session
Chauncey D. Leake, *presiding*
"Paracelsus—Charlatan or Precursor of Modern Medicine: a Reexamination of His Theory of Disease"
John P. Dolan, Columbia, S.C.
"Medicine, Surgery and the Medieval Church Councils—a Reinterpretation"
Darrel W. Amundsen, Bellingham, Wash.
"The Relationship between Midwives and Physicians during the Renaissance"
Thomas G. Benedek, Pittsburgh, Pa.

12:00 Noon Luncheon Roundtables
"Teaching Medical History"
Chester R. Burns, *Coordinator*
"Women and Medicine"
Judith W. Leavitt, *Coordinator*
"The Practicing Physician and Medical History"
Robert P. Hudson, *Coordinator*
"Oral History"
Gwendolyn Safier, *Coordinator*
"The Barefoot Doctors: Cooperative Health Care in China" (film)
Peter K. New, *Coordinator*

2:00 p.m. General Session
Eugene H. Conner, *presiding*
"The Art Collections of John and William Hunter"
Anthony A. Pearson, Portland, Ore.
"The Contributions of Alexander Graham Bell and Thomas Alva Edison to Medicine"
David A. E. Shephard, Ottawa, Ont.
"Depopulation in Polynesia"
Charles S. Judd, Jr., Honolulu, Hawaii
"The Fünfbilderserie—a Bridge to the Unknown"
Ynez Violé O'Neill, Los Angeles, Calif.

4:15 p.m. The Fielding H. Garrison Lecture
Lester S. King, *presiding*
"Walter Reed and the Ordeal of Human Experiments"
William B. Bean, Galveston, Texas

6:00 p.m. Texas Style Barbecue Dinner and Square Dancing, Ashton Villa

Friday, May 14

9:00 a.m. General Session
Dorothy I. Lansing, *presiding*
"Carl Rokitansky's Theory of Disease: Origins and Criticisms"
Robert J. Miciotto, Baltimore, Md.
"Rudolf Virchow, Julius Cohnheim, and the Program of Pathology"

Russell C. Maulitz, Philadelphia, Pa.
"Germain See (1818-1896), Consultant to Napoleon III"
Richard Satran, Rochester, N.Y.
"Health Care in Eighteenth Century France: the Role of the Veterinarian"
Caroline Hannaway, Baltimore, Md.
"Professional Medicine and Traditional Society: the Cantonal Physicians of the Bas-Rhin, 1810-1870"
George D. Sussman, Nashville, Tenn.

12:00 Noon Luncheon Roundtables (same as Thursday)

2:00 p.m. General Session
Russell C. Maulitz, *presiding*
"Class of 1832: The First Western-Trained Graduates of Egypt's New Medical School"
LaVerne Kuhnke, Boston, Mass.
"Rival Traditions: Western Medicine and Unani Tibb in the Punjab, 1853-1887"
John C. Hume, Jr., Durham, N.C.
"Air Fit for Breathing: Victorian Chemists and Theories of Infection"
John M. Eyler, Minneapolis, Minn.
"East or West? The Geographic Origins of the Black Death"
John Norris, Vancouver, B.C.

6:30 p.m. Cocktails, Caduceus Room, University of Texas Medical Branch

7:30 p.m. Annual Banquet, Moody Medical Library, University of Texas Medical Branch
Truman G. Blocker, Jr., *presiding*
"The Early Days of Space Medicine"
Captain Joseph P. Kerwin, Houston, Texas

Saturday, May 15

9:00 p.m. General Session, Jesse H. Jones Library Building, Texas Medical Center, Houston, Texas
George E. Gifford, Jr., *presiding*
"Welcoming remarks," Sam W. Hitt, Houston Academy of Medicine-Texas Medical Center Library
"The Flexner Report in the Evolution of American Medical Education"
James Monahan, Chicago, Ill.
"The Introduction of the Edinburgh Quizzing System into American Medicine"
W. Robert Penman, Paoli, Pa.
"Disinfection of Mail in the United States"
Emmet F. Pearson, Springfield, Ill.
Wyndham Miles, Bethesda, Md.
"Military Victory and Victorious Medicine: the Continental

Army Experience at Boston (1775-76), Saratoga (1777) and Yorktown (1781)"
Philip Cash, Boston, Mass.
"The Emergence of Biomedical Research in America"
Sandra M. Annes, Los Angeles, Calif.
"Legal and Social Aspects of Medical Malpractice in Nineteenth and Early Twentieth Century Texas"
James B. Speer, Jr., Houston, Texas

Program Committee: Robert P. Hudson, *Chairman,* George E. Gifford, Jr., Frederic Lawrence Holmes, Gordon W. Jones.

Local Arrangements Committee: Chester L. Burns, *Chairman,* Mrs. Malcolm Wall, Truman G. Blocker, Jr., Mrs. William Levin, Emil Frey, William Bean, Mrs. William Bean, Mrs. Jo Frances Leecraft, Sam Hitt, Mrs. Janice Hayes, John P. McGovern.

EXCERPTS FROM THE MINUTES OF THE ANNUAL MEETING OF THE COUNCIL

May 12, 1976

The annual meeting of the Council of the American Association for the History of Medicine, Inc., was called to order at the Flagship Hotel, Galveston, Texas, at 3 p.m. on May 12, 1976. President Lester S. King served as Chairman, and Genevieve Miller as Secretary. The following Council members were present: John B. Blake, John C. Burnham, G.S.T. Cavanagh, Audrey B. Davis, John Duffy, Guenter B. Risse, Ilza Veith, Dora B. Weiner, Richard J. Wolfe. Absent were Johanna Grimes, Gordon W. Jones, Jerry Stannard, George E. Gifford, Jr., and Charles Rosenberg.

The Secretary reported that a *Supplement to the Membership Directory* was distributed to members in July 1975 and that a new *Directory* will be prepared in July 1976. Thirty members of A.A.H.M. have applied for membership in the International Society of the History of Medicine. Dr. Miller commented that the membership remains slightly under 1000. At the end of October 1975 the total membership was 955; today it stands at 981.

Committee Reports

Dr. King announced that Jane M. Oppenheimer, William R. Kenan, Jr. Professor of the History of Science at Bryn Mawr College, has been selected as Garrison Lecturer for 1977. The Chairman of the selection committee was Paul Cranefield.

The William H. Welch Medal Committee (James H. Cassedy, *Chairman,* Jerome J. Bylebyl, and R. Palmer Howard) unanimously recommended and the Council approved by mail vote that the medal be given to Lelland J. Rather of Stanford University with the statement:

> "This nomination is based specifically upon Dr. Rather's most recent book, *Addison and the White Corpuscles* (1972), but also in recognition of his significant earlier work on Virchow, his 1965 volume *Mind and Body in 18th Century Medicine,* and his important continuing studies in the history of pathology."

The William Osler Medal Committee (Peter D. Olch, *Chairman,* Russell C.

Maulitz, and A. Benedict Schneider) recommended and the Council approved the award of the 1976 Osler Medal to Steven A. Brody, the class of 1977 at the Washington University School of Medicine in St. Louis, for his essay on "The Life and Times of Sir Fielding Ould: Man-midwife and Master Physician." Arthur Gelston, the class of 1977 at Cornell Medical College, was awarded Honorable Mention for his paper "Febre Typhus: Its Causes, Diagnosis and Treatment during the Epidemic of 1847, with a Clinical Analysis of 138 Consecutive Cases from the Medical Records of the New York Hospital." Seven essays were submitted for the contest from Washington University School of Medicine, Cornell Medical College, Johns Hopkins, University of Florida College of Medicine, Tulane University School of Medicine, University of Hawaii School of Medicine, and McGill University.

The report of the Ways and Means Committee (Robert Moes, *Chairman*) to consider the development of further income for the Association, recommended a possible book sale of donated items to be held during the Annual Meeting.

No official report was received from the Loeb Library Committee (Saul Jarcho, *Chairman*, Richard J. Wolfe, and Jerry Stannard), but Mr. Wolfe reported that a grant proposal had been submitted by Paul Potter of the University of Western Ontario on behalf of the Association to the National Library of Medicine Extramural Program. The project involves the translation of additional Hippocratic treatises for the Loeb Classical Library. The grant was approved, but has not yet been funded.

Other Business

Dr. Miller reported the receipt of a contribution of $100 from the Texas Allergy Research Foundation through John P. McGovern.

James H. Cassedy has volunteered to prepare *Research in Progress No. 3* to be distributed to the membership in February 1977. The Council approved funding the printing and distribution of the list, and recommended that copies distributed to non-members of the Association should be sold for $2.00 each.

The Council voted unanimously to recommend to the membership that the Association apply to become an affiliated society of the American Historical Association. The Program Committee would be the liaison between the two organizations. This affiliation will provide the opportunity to have joint or collateral programs at the time of the A.H.A. meetings.

The possibility was raised of a joint session with the American Institute of the History of Pharmacy at the next annual meeting in Madison, Wis., in 1977. The Council approved such a joint session, if the Program Committee chairman wishes it.

The Council accepted the report of the Ad Hoc Committee on Education (Chester R. Burns, *Chairman*, Gert Brieger, and Dora B. Weiner). Regarding the proposed Bylaws changes to establish a standing Committee on Education, it recommended that only the general statement of purpose be included, and that the chairman of the Committee on Education should be an *ex-officio* and non-voting member of the Council. In accepting the report the Council requested the Committee to prepare a statement explaining the implication of the proposed Bylaws and distribute it to the membership before the formal vote at the next Annual Meeting.

The report of the Ad Hoc Committee on Meetings (Donald G. Bates, *Chairman*, Robert P. Hudson, Guenter B. Risse) was accepted. The Council voted that the

Committee be instructed that the Council favors a Committee on Meetings acting in an advisory capacity, and that an amendment to the Bylaws should include a statement of purpose.

Dora B. Weiner, the U.S. Delegate to the International Society of the History of Medicine, presented an extensive report. It recommended and the Council approved the appointment of Ann F. LaBerge to represent the U.S. at the meeting of the Permanent International Committee in Paris on May 22, 1976. The Council also approved the suggestion that English be adopted as the second language of the Society, and recommended that the Society be informed at the next International Congress in Quebec in August, 1976, of the possible interest of the American Association for the History of Medicine in hosting the 1984 Congress.

The Council meeting adjourned at 5:30 p.m.

Genevieve Miller

BUDGET 1976/1977

Receipts	
Membership dues (1000)	18,000
Sale of address list, Research in Progress, etc.	100
Annual Meeting	4,000
Interest on savings	1,000
	23,100
Expenditures	
Subscriptions to *Bull. Hist. Med.* (1000)	13,000
Administrative expenses	2,500
Printing and mailing Research in Progress	1,000
Annual Meeting	4,000
Honors and Awards	
Garrison Lecture Honorarium	500
Garrison Lecturer Travel	500
Osler Medal Prize	200
Osler Medal Travel	200
Welch Medal	50
Printing expenses, costs of medals	150
Committee Expenses	1,000
	23,100

EXCERPTS FROM THE MINUTES OF THE ANNUAL MEETING

May 14, 1976

The Annual Meeting of the American Association for the History of Medicine, Inc., was held in the Flagship Hotel, Galveston, Texas, at 4:30 p.m. on May 14, 1976. Lester S. King, President of the Association, served as Chairman, and Genevieve Miller as Secretary.

The minutes of the previous meeting were approved. Dr. Miller read the names of members who had died since last May. They are as follows:

Mrs. Helen Bayne Cherami, New York, N.Y.
Robert R. Crawford, Mansfield, Ohio
Robert E. Fricke, Rochester, Minn.
Martha Teach Gnudi, Los Angeles, Calif.
Thor Jager, Wichita, Kansas
Herbert S. Klickstein, Philadelphia, Pa.
Morris C. Leikind, Tel Aviv, Israel
Grant Levin, San Francisco, Calif.
William S. Middleton, Madison, Wis.
Rudolph E. Siegel, Buffalo, N.Y.
Benjamin Spector, Atlantic Beach, N.Y.
Cecil Striker, Cincinnati, O.

The members rose for a moment of silence. Dr. King called on Dr. Samuel Radbill who spoke in tribute to Dr. Benjamin Spector.*

The Secretary-Treasurer presented a summary of the Financial Report for the Fiscal Year 1974/1975. The auditors reported that the books were in order (recommending that $2000 be transferred from the General Fund into the Endowment Funds), and the reports of the Treasurer and the auditors were approved. Genevieve Miller was thanked for her efforts as Secretary-Treasurer over the past five years.

Old Business

The President announced the National Library of Medicine award of a still-unfunded grant to the Loeb Library project in the name of the Association. He also reported briefly on the reports of the Ad Hoc Committees on Education and Meetings. Dr. Robert Hudson commented that the report of the Committee on Meetings prepared by Dr. Donald G. Bates was well executed. He suggested that members' workshops might be held to discuss new directions the Association might take.

Dr. Miller summarized the reports of the Constituent Societies, noting that the Mayo Foundation History of Medicine Society with 304 active members continues to be the largest local group.

New Business

Dr. King presented the Council's recommendation that Owsei Temkin be elected an Honorary Member of the Association with the following citation:

> "Dr. Owsei Temkin, William H. Welch Professor Emeritus of the History of Medicine and retired Director of the Johns Hopkins Institute of the History of Medicine, served as President of the American Association for the History of Medicine in 1958-60, was the recipient of its William H. Welch Medal in 1965, its Fielding H. Garrison Lecturer in 1962, and the Editor of the *Bulletin of the History of Medicine* from 1948 to 1968. His many publications include *The*

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* Dr. Radbill's remarks are incorporated in an obituary of Dr. Spector by him and Dr. Lawrence Charles Parrish, *Bull. Hist. Med.*, 1976, *50* 619-622

> *Falling Sickness, a History of Epilepsy from the Greeks to the Beginning of Modern Neurology* (1945; 1971) and *Galenism, Rise and Decline of a Medical Philosophy* (1973), as well as numerous articles. His distinguished career as a teacher, scholar, and editor amply support his nomination for Honorary Membership in the American Association for the History of Medicine."

The motion passed unanimously.

Dr. Miller spoke on the Council's recommendation of the desirability of creating an Emeritus Membership in the Association, enabling anyone who has been a member for 20 years and has reached the age of 70 to apply for Emeritus status. Emeritus Members would receive all mailings of the Association with the exception of the *Bulletin of the History of Medicine*. The Following amendments to the Bylaws were approved:

> Article II: MEMBERSHIP
>
> *Section 1:* There shall be *five* classes of members: Active Members, Nonresident Members, Honorary Members, *Emeritus Members,* and Constituent Societies. . . .
>
> *[Add a new Section 5]*
>
> Section 5: Anyone who has been a member for 20 years and has reached the age of 70, may on request become an Emeritus Member and be excused from the payment of dues.
>
> *[Change the following Section numbers to 6, 7, and 8]*
>
> Article VII: PUBLICATIONS
>
> *Section 2:* All members, *except Emeritus Members,* shall receive regular subscriptions to the journal or other publication that prints the offical transactions of the Association.

The Association members present voted unanimously to waive the dues of Emeritus Members.

Dr. King announced the Council's recommendation that the Association become an Affiliated Society of the American Historical Association, an arrangement that carries no financial obligation. The motion to become an Affiliated Society carried, and the Secretary-Treasurer was instructed to make the necessary application.

Dr. King announced the 1977 Garrison Lecturer, Dr. Jane M. Oppenheimer, the International Congress of the History of Medicine in Quebec on August 21-28, 1976, and the forthcoming third edition of *Research in Progress* which Dr. James H. Cassedy has offered to prepare.

The President announced that the next annual meeting of the Association will be held in Madison, Wisconsin, May 11-14, 1977, under the chairmanship of Dr. Guenter Risse. Future meetings will be held in Kansas City, Kansas (1978), Pittsburgh, Pa. (1979), and Boston (1980 or 1981).

Dr. King explained that Dr. Genevieve Miller as resigned as Secretary-Treasurer to be nominated as Vice-President of the Association. The following new officers were elected unanimously:

President (2 year term)	John Duffy, College Park, Md.
Vice-President (2 year term)	Genevieve Miller, Cleveland, O.

Secretary-Treasurer (1 year term)	Charles G. Roland, Rochester, Minn.
Members of Council (3 year term)	Edward C. Atwater, Rochester, N.Y. Thomas G. Benedek, Pittsburgh, Pa. William Coleman, Baltimore, Md. A. Benedict Schneider, Cleveland, O.

The meeting was closed at 5:15 p.m. by the new President, John Duffy, who announced that the Chairman of the Program Committee for the 1977 meeting is George E. Gifford, Jr. Enthusiastic thanks were expressed to the Local Arrangements Committee led by Chester R. Burns for the delightful meeting in Galveston. Applause.

Genevieve Miller
Secretary-Treasurer

Financial Report on the Fiscal Year 1974/75
Ending 31 October 1975

Classified Statement of Receipts and Expenditures

1 November 1974—31 October 1975

General Fund

RECEIPTS		
Dues		
Regular Members	$14128.04	
Contributing Members	450.00	
Non-Resident Members	540.00	
Constituent Societies	550.00	
Contrib. Constituent Soc.	50.00	
Family Members	1025.00	
Student Members	390.00	
Int. Soc. Hist. Med.	609.00	
		$17742.04
Address list sale		40.00
Membership Directory and Research in Prog. Sale		6.50
Refund from Johns Hopkins University Press		1267.50
Registr. Fee Annual Meeting		48.50
	1974/75 Total receipts	19104.54
	Balance 1 November 1974	10501.43
		$29605.97
EXPENDITURES		
Subscriptions to *Bull. Hist. Med.*		13301.75
Administrative expenses		
Printing, postage, stationery, etc.		917.79
Clerical assistance		437 30
Social security		53.84

Print. and mailing Memb. Direct. Supplement		106.12
Print. and mailing Research in Progress		853.54
Dues refunds		63.75
Honors and Awards		
Osler Medal		165.25
Welch Medal		11.85
Garrison Lecture		1000.00
Transfers to other Funds		
Publication Fund		4000.00
Annual Meeting Fund		48.50
International Society Hist. Med.		609.00
	1974/75 total expend.	21568.69
	Balance 31 October 1975	8037.28
		$29605.97

President's Discretionary Fund

RECEIPTS

Lester S. King contribution		250.00
	Balance 1 November 1974	80.77
		$330.77

EXPENDITURES

W. H. McNeill expenses		171.20
	Balance 31 October 1975	159.57
		$330.77

Annual Meeting Fund

RECEIPTS

Philadelphia meeting		718.45
	Balance 1 November 1974	1485.32
		$2203.77

EXPENDITURES

Philadelphia meeting		568.38
	Balance 31 October 1975	1635.39
		$2203.77

Endowment Funds

RECEIPTS

Lester S. King contribution		250.00
Interest on Savings Accounts		1062.31
	Balance 1 November 1974	3697.68
		$5009.99

EXPENDITURES		000.00
	Balance 31 October 1975	5009.99
		$5009.99

International Society for the History of Medicine

RECEIPTS		
Dues collected for U.S. members		609.00
	Balance 1 November 1974	152.71
		$761.71
EXPENDITURES		
Dues transferred to International Soc.		597.60
	Balance 31 October 1975	164.11
		$761.71

Publication Fund

Transferred from General Fund	$4000.00

STATEMENT OF BALANCE

31 October 1974

ASSETS		
Cash in Checking Acct.		
Continental Bank	$1,214.25	
Cash in Operating Savings Acct.		
Continental Bank	7,694.07	
Cash in 5 percent Savings Acct.		
Continental Bank	6,979.59	
Credit—Johns Hopkins Press	30.00	
		$15,917.91
LIABILITIES		00,000.00
NET WORTH		
General Fund	10,501.43	
President's Fund	80.77	
Annual Meeting Fund	1,485.32	
Endowment Funds	3,697.68	
International Society funds	152.71	
		$15,917.91

31 October 1975

ASSETS	
Cash in Checking Acct.	2,270.37
Continental Bank	
Cash in Operating Savings Acct.	
Continental Bank	2,993.18
Cash in 5 percent Savings Acct.	
Continental Bank	2,186.15
Savings Certificate	
Continental Bank	5,247.73
Savings Certificate	
Shaker Savings Assoc.	6,308.91

		$19,006.34
LIABILITIES		00,000.00
NET WORTH		
General Fund	8037.28	
President's Fund	159.57	
Annual Meeting Fund	1635.39	
Endowment Funds	5009.99	
Publication Fund	4000.00	
International Society funds	164.11	
		$19,006.34

	Number of Members		
	1 November 1974	*31 October 1975*	*Net Change*
Active Members	871*	897	+26
Non-resident Members	33	31	− 2
Constituent Societies	22	23	+ 1
Honorary Members	5	4	− 1
TOTALS	931*	955	24

* Corrected 1974 figures

SUMMARY.

127 new active members (including 7 spouses who joined as family members and 19 students)
1 new non-resident member
2 new constituent societies
18 resignations
82 cancellations for non-payment of dues (including 2 non-resident members)
6 deceased members (including 1 honorary member)

BULLETIN OF THE HISTORY OF MEDICINE

Volume 49 of the *Bulletin* was completed with the Winter issue of 1975. The volume contained iv + 615 pages and 19 illustrations. Of these 221 pages, about 35%, were devoted to materials stemming directly from the American Association for the History of Medicine.

A financial statement relating to income and expenditures in its operation of the *Bulletin* for 1975 follows.

The Johns Hopkins University Press
Bulletin of the History of Medicine
Volume 49, #1—4, 1975

Balance 1974 Statement			(525.48)
Income			
Subscriptions	25727.70		
Back Issues	782.35		
Reprints	1015.89		
Permission Fees	140.82		
Microfilm Royalties	474.64		
List Rental	101.24		
Postage	70.65		
Advertising	827.45	29140.74	
Expenses			
Manufacturing	17450.79		
Distribution	512.44		
Postage	331.12		
Reprints	706.86		
List Rental	29.83		
Commission	7261.80		
Promotion	18.00		
Miscellaneous	24.00	26334.84	
Operating Balance 1975			2805.90
Total Balance			2280.42

NOTES AND COMMENTS

DE MOTU CORDIS: WRITTEN IN TWO STAGES?

COMMENT

GWENETH WHITTERIDGE

In his article, "The Growth of Harvey's *De motu cordis*" (*Bull. Hist. Med.,* 1973, *47:* 427-70), Jerome J. Bylebyl has put forward the theory that before the discovery of the circulation, Harvey wrote an "independent treatise on the movement of the heart. Then after the discovery he added the account of the circulation to this earlier treatise . . ." (p. 428). He thinks that Harvey wrote this separate treatise on the movement of the heart and arteries either "before or after the discovery of the circulation," that he subsequently changed his plans and expanded the original treatise into *De motu cordis,* and that when he wrote the more limited treatise Harvey was ignorant of the circulation (p. 428).

Any hypothesis of this kind is difficult to prove and in the case of *De motu cordis* irrefutable evidence is hard to come by. For the existence of an earlier independent treatise of limited scope, there is no direct external evidence for it is not mentioned either by Harvey or anyone else. During the course of the articles, however, Bylebyl cites two pieces of indirect external evidence to support his view. The first of these is the summary of the essential features of the circulation added to the notes for the Anatomical Lectures of 1616 on what was originally a blank page. "Here is an actual case where Harvey had written a self-contained discussion of the movement of the heart and arteries, to which, after a lapse of perhaps ten years, he added an account of the circulation . . ." (p. 450). But we have no means of knowing for certain when Harvey wrote this brief summary. If it was after the writing of *De motu cordis,* as there is reason to believe from Harvey's use of the analogy of the pump, it is difficult to see how it can provide any evidence for the two-stage composition of that book. The second piece of indirect external evidence is equally difficult to bring to bear on the precise problem. It is provided by the set of anatomical notes preserved in the archives of the Royal Society. These notes consist of thirty-four observations, five of which clearly relate to the circulation, three doubtfully, and two to the movement of the heart and

BULLETIN OF THE HISTORY OF MEDICINE 51 130-150 (1977)
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arteries and may mean anything. Of them Bylebyl says that they "clearly contain elements from the earlier study of the heartbeat and pulse as well as from the later work on the circulation, but now these two themes are inextricably intermingled with each other. Harvey does not, as in *De motu cordis,* begin by discussing the movement of the heart and arteries, and only later introduce the quantitative argument and consequent notion of the circulation. Instead, he boldly begins with the quantitative argument, and then goes on to treat the movement of the heart and the circulation more or less concurrently . . ." (p. 461). He takes this as evidence confirming his opinion of the two-stage composition of *De motu cordis* because of the parallel in chs. 8-12 "where we actually find a whole second disquisition on the heartbeat and pulse, viewed now in the light of the quantitative argument concerning the circulation" (p. 462). Apart from any other objections which could be raised to this type of argument, it is sufficient to say that these notes are useless to support any hypothesis concerning the composition of *De motu cordis* for we have no means of knowing their connection with Harvey. They are not in his handwriting. All we know of them is that they were read to the Royal Society by Dr. Gale on 4 May 1687 (that is thirty years after Harvey's death), and they are said to have been obtained from Thomas Hollier. The observations themselves vary considerably in quality and in their arrangement there is no obvious order. No weight can, therefore, be attached to the "intermingling" for there is no proof that the order in which the observations occur in the notes was either Harvey's or deliberate.

To prove his hypothesis, therefore, it would seem that Bylebyl must rely entirely on the internal evidence provided by *De motu cordis* itself, and it is with this that the greater part of the article is concerned.

Because there are no direct references to the circulation or to any of its proofs in the earlier part of the book, Bylebyl, arguing from "silence," always a dangerous practice, maintains that the whole work falls into two parts: (1) the Proem, chs. 2-7 and 17; (2) chs. 8-16, and he says that there is "considerable evidence" to show that these two great divisions "were written at different times, under different circumstances, and with different ends in view" (p. 449).

With regard to the date of the composition of the hypothetical independent treatise, Bylebyl has no firm suggestion to make. He maintains, however, that it would have been "quite reasonable" for Harvey to have prepared such a treatise on the movement of the heart and arteries alone "either before he had discovered the circulation, or even afterwards, at a time when he was not yet sufficiently confident to reveal the full extent of his work'' (p. 441). He bases his assumption on the existence of a controversy over the nature and timing of the systole and diastole of the heart and arteries and believes that it was the ''challenge'' of this controversy that led Harvey to begin his study of the heart and blood vessels in animals. Now this may well be true. It is clear from *De motu cordis* that Harvey was extremely well informed about everything that had been written on the subject. The evidence of the Anatomical Lectures shows that he had begun to work on the problem before 1616 and that he was then certain of the existence of the pulmonary transit of the blood and of the correct timing of the systole and diastole of the heart and arteries. His knowledge of the unsatisfactory nature of the diverse opinions, as he reveals it in the Proem, may well have led him to investigate the matter more thoroughly. But this does not lead to the conclusion that, therefore, Harvey decided to write a separate treatise of limited scope before he had discovered the circulation, or while he was still in the process of verifying his hypothesis, or after he had verified it. Moreover, if we are to suppose that Harvey wrote this treatise *after* the hypothesis of the circulation had occurred to him, we may also suppose that he would then have been fully aware of how incomplete was the work he had done up to that time and how partial and inconclusive any such treatise would have been. It would seem equally ''reasonable'' to suppose that after the discovery of the circulation at least, the idea of publishing such a treatise would not have occurred to him. To support the idea that he disliked publishing unfinished work, we can point to his reluctance to part with the manuscript of *De generatione* because in his opinion the work was incomplete as it lacked his observations on insects. We could also say that, at least as an old man, Harvey disliked entering into controversy. ''Assuredly, it is better at times to endeavour to grow wise at home, than by the hasty divulgation of such things to whose knowledge you have attained with vast labour, to stir up tempests . . . .'' This same dislike for controversy may perhaps explain in some measure his silence during the twenty years of criticism of *De motu cordis,* 1629-1649.

Starting from his supposition that the structure of *De motu cordis* falls into two divisions, the ''silent'' sections and the rest, Bylebyl proceeds to try to find evidence to support his belief that his hypothetical independent treatise forms the basis of the first part of *De motu cordis* and that to it the account of the circulation was added later. Such evidence might be forthcoming if he could prove: (1) that any statement in the ''silent section'' relating to the circulation is undeniably a later interpolation, and (2) that this section contains any statement inconsistent with a knowledge of the circulation of the blood. As Bylebyl allows Harvey the possibility of correcting his earlier treatise in the light of his subsequent knowledge of the circulation (p. 451), any such inconsistent statement will be a "residual or unnoticed contradiction."

With regard to interpolations in the text, Bylebyl finds that there is only one statement which clearly refers to the circulation and that "the others do not necessarily reflect a knowledge" of it but can be "satisfactorily accounted for on other grounds" (p. 451). The passage in question occurs in ch. 4 where, in the middle of describing the movement of the auricles, Harvey inserts a parenthesis in which he alludes to their manner of filling (pp. 453-4). It reads as follows:

> Auriculae enim dum moventur, et pulsant albidiores fiunt, praesertim ubi pauco sanguine replentur (replentur autem tanquam promptuarium, et lacuna sanguinis, declinante sponte sanguine, et venarum motu compresso ad centrum) quin etiam in finibus et extremitatibus ipsarum haec albedo a contractione fieri, vel maxime apparet.

As Bylebyl points out, the sentence in the bracket has a parallel in ch. 15 and can imply a knowledge of the circulation. Proof that it is an interpolation he finds in the repetition of the word *replentur* with a different meaning. He holds that its use in the first instance is "in the static sense of 'are filled' or 'contain,' while that in the parenthesis refers to the actual process of refilling." By introducing the parenthesis, he thinks that Harvey made "errors of semantics and logic." But this is pure opinion and not proof for it is questionable that there is any difference in meaning in the two uses of *replentur*. They can both be equally well translated literally as "are filled."

A passage similar to that quoted by Bylebyl occurs in ch. 5:

> Primum sese contrahit auricula, et in illa contractione sanguinem contentum (quo abundat tanquam venarum caput, et sanguinis promptuarium, et cisterna) in ventriculum cordis coniicit. . . .

Here again Harvey has broken off in the middle of a sentence describing the contraction of the auricle to refer to the blood which is contained in it. So here again we might say that Harvey has made another and similar hiatus in the "logical nexus of the two portions," and therefore this parenthesis is also an interpolation. But here the parenthesis does not begin with the repetition of the same word and its statement reflects a Galenic opinion. Moreover, the habit of making statements in parentheses is characteristic of Harvey's style; *De motu cordis* contains many examples.

If then Bylebyl's quotation from ch. 4 cannot be proved to contain an error either in semantics or logic, it cannot be proved to be an interpolation, for to assert that it is one on the grounds that it contains a reference to the circulation would be to argue in a circle.

As an example of a fundamental yet unnoticed contradiction between the two divisions of *De motu cordis,* Bylebyl takes the references to the post-mortem appearance of the heart, that is to Harvey's diverse remarks concerning the presence or absence of blood in the veins, arteries and right and left ventricle of the heart. "In the 'silent' parts of *De motu cordis* he stresses several times that these organs are always filled with blood, not only in live animals but also in dead ones" (p. 464), and Bylebyl lists all the references which Harvey makes in the Proem and in chs. 5 and 17 to the thick, congealing, black blood which can be found after death in both the right and the left ventricle. With this he contrasts the statement that Harvey makes in ch. 9 to the effect that his hypothesis of the circulation explains "why in dissections, so much blood is found in the veins, but little in the arteries; why much in the right ventricle, little in the left one" (p. 464). On this Bylebyl comments (p. 465): "Thus Harvey now points to the general *emptiness* of the left ventricle and arteries in cadavers as indirectly confirming the circulation, whereas he had previously insisted on their usual *fullness* with blood even after death. In the earlier discussion he was apparently at a loss to explain how blood might escape from the left ventricle and arteries in an uninjured body, and therefore he simply denied that this occurs."

This criticism of Harvey is both careless and wrong. Harvey does not anywhere deny that blood escapes from the heart after death. He is merely silent on the point. Seeing that the relative emptiness of the left ventricle and of the arteries after death was known to the "Ancients" I think it unlikely that Harvey himself was ignorant of this post-mortem state of affairs even before he discovered the circulation, and to say that he "denied" that blood escaped comes very near to asserting that Harvey denied the evidence of his own eyes.

What is more, Bylebyl's remarks imply that he himself thinks that the post-mortem appearance of the heart is always the same, and this is not so. Since there is no such thing as "normal" death, the post-mortem findings are variable because they depend on the cause of death. Now in ch. 9, Harvey says most precisely: *Hinc causam aperte videbis, cur in Anatome, tantum sanguinis,* etc., that is to say: "Hence, you will clearly see the reason why in an Anatomy there is so much blood in the veins," etc. Willis, K. J. Franklin and Bylebyl have all translated the significant words *in Anatome* by the general expression "in dissections," thereby blurring the meaning of Harvey's precise statement. These are findings made during the course of an Anatomy, that is during the dissection of a human cadaver. We know that the bodies dissected at an Anatomy at this time were legally acquired from the judicature and were the bodies of criminals who had been hanged. In all forms of death from asphyxia, most markedly in drowning, but also in hanging, when the heart continues to beat but the chest can no longer expand, the right side of the heart is found at autopsy to be engorged with blood to a greater or less degree, while the left side is relatively empty. Harvey's description fits the appearances which one would expect to find after death from this cause:

> When the lungs have died and ceased to move, the blood is prevented from passing from the little branches of the pulmonary artery into the pulmonary vein and from thence into the left ventricle of the heart . . . . But since the heart does not cease to move at the same time as the lungs, but continues to beat afterwards and to outlive them, it follows that the left ventricle and the arteries send forth blood into the veins and into every part of the body but receive none through the lungs and therefore they are as it were empty.

But lest it should be thought that Harvey is here saying that all the arteries in the body are empty after death, it is as well to bear in mind his further remarks in the same chapter:

> . . . in a dead body when the heart has ceased to beat, you cannot, try as you will, extract more than half of the whole mass of the blood from the jugular or crural veins or arteries when they are opened.

More important, Harvey does not "point to the general *emptiness* of the left ventricle and arteries in cadavers" when describing what is usually found at an Anatomy. He says precisely that "much blood is found in the veins, but little in the arteries; . . . much in the right ventricle little in the left one." It is merely a question of the relative proportions of the amounts remaining in two ventricles.

Of the five passages which Bylebyl quotes from the "silent" section, the first is irrelevant for it is clear from the context that Harvey is here merely upholding Galen's statement that blood and not air is contained in the arteries after death, a self-evident truth. The other four references all relate to findings made *in dissectionibus,* "during dissections," and all occur in passages in which Harvey is dealing with the pulmonary transit of the blood. In them he is therefore concerned to prove that, contrary to the common belief, blood and not air or fuliginous vapours exists at all times in the pulmonary vein and left ventricle of the heart. Now we have no means of knowing how these observations were made or on what kind of material. We might hazard a guess that the observation on the pulmonary vein (the third example Bylebyl gives on p. 464) refers to a vivisection, bearing in mind Columbus's description of his own experiment to prove that it contained only blood.

> If they maintain that fumes and air go out and return by this way, . . . why, when we cut out or open up the pulmonary vein, can we never find either air or fumes in the dissection, and how comes it that we always see the pulmonary vein crammed full of thick blood and never of air. . .[7]

It will also be remembered that when he begins his discussion of the pulmonary transit in ch. 6, Harvey insists on the necessity for working on animals to solve this problem, for merely looking into the dead bodies of humans will not answer the question. But no matter how it was obtained, this observation in no way contradicts what Harvey says in ch. 9. We are, therefore, left with the three passages in which Harvey refers to the presence of blood in both ventricles of the heart.

The first of these passages occurs in the Proem: "Why . . . since the constitution of both ventricles is almost the same . . . and in dissection are both found full of blood that is in both alike blackish and alike lumpy . . . should we think that they were appointed to such diverse uses. . . ?" The second comes from ch. 5: "This uncertainty (that is, as to how the blood could get into the left ventricle when it was supposed to contain only air or spirit) does not a little perplex the anatomists when in dissections they always find the pulmonary vein and the left ventricle of the heart full of thick, knotty, black blood, so that they are forced to affirm that this blood sweated through the septum of the heart from the right to the left ventricle." The third is in ch. 17: "Likewise, the reason why we see in an anatomical dissection (*in anatomica dissectione*) that the pulmonary vein and the left ventricle abound with so great a quantity of blood and indeed of the same blood, alike blackish and lumpy, as that with which the right ventricle and pulmonary artery are filled, is because the blood passes from one side to the other continually through the lungs."

We will assume for the purposes of the argument, that these three descriptions all relate to post-mortem findings, in which case we are faced with the unanswerable question as to how these experimental subjects were put to death. Yet it is on this that the post-mortem state of the heart depends. While it is more usual to find more blood in the right heart than in the left, there are conditions in which the reverse can occur and others again in which the quantity on both sides can be roughly equal. It is unusual to find the left side entirely empty, but the amount that it contains varies considerably. Even when butchers drain out the blood from an animal, it is still usual to find some blood in the left ventricle. (Anyone who has prepared hearts for culinary purposes cannot have failed to notice that this is so.) In these three descriptions, there is nothing to contradict what Harvey says of the post-mortem findings in ch. 9. His descriptions must be allowed to be at least as variable as the accepted post-mortem findings given in any textbook of pathology or forensic medicine.

To go through the whole article in detail would be tedious. It proceeds from assumption to assumption but fails to provide a single piece of irrefutable evidence to support the hypothesis that *De motu cordis* was based on an earlier treatise of limited scope. It is strange that if Bylebyl really believes as he says, and most people would agree with him, that *De motu cordis* is "one of the greatest scientific masterpieces of all time," that he should think that it was made simply by putting "a piece of new cloth unto an old garment." To be a masterpiece it can have no obvious rents or joins. Even if Harvey had written an earlier treatise, had he not re-written it before incorporating it into *De motu cordis* the resultant book could well have failed to be a "masterpiece." But as Bylebyl's conjecture cannot be proved, so likewise it cannot be disproved. It can only be countered by showing that this conjecture is unnecessary to explain the underlying structure and form of *De motu cordis*.

It is difficult to escape the conclusion that the reason for which Bylebyl has put forward this conjecture is that the structure of *De motu cordis* does not conform to his idea of what that structure should be. He says (p. 466) "if he had already made the discovery before writing the earlier parts of *De motu cordis*, we should expect to find in them an interest in the quantity of blood transmitted by the heart," and he stresses the number of occasions in the earlier part of the book in which Harvey refers to his work on the movement of the heart and arteries, the movement and use of the heart and arteries, or the movement, pulse, action, use and functions of the heart and arteries, without mentioning the circulation. (What is the movement and use of the heart and arteries if it is not the circulation? But admittedly, the word is not used.) From this it would seem that Bylebyl's worry derives from the fact that the hypothesis that the blood circulates is not announced until ch. 8, and that there are no explicit references to it as such and no mention of the quantity of blood in the "silent sections." If Harvey had been writing up his discovery today for publication in a modern scientific journal, he might well have announced his hypothesis earlier, for he would have written in accordance with the form now accepted as that in which the validity of the experimental evidence on which a new hypothesis is based can be most easily understood and assessed. But he was writing in the seventeenth century and he adopted the form which then fulfilled this purpose. The clue to the structure of *De motu cordis,* Harvey himself provides. In the anatomical Lectures, when he is discussing the true nature and timing of the systole and diastole of the heart, he says: "this error has been current for a very long time and therefore I have proceeded according to the strictest form because the subject is so ancient a one and has been studied by so many great men." If he thought it necessary to proceed "according to the strictest form" in order to establish the validity of one stage in the investigations that led inevitably to the final conclusion that the blood circulates throughout the whole body, it would seem most likely that he should have been at least as careful to proceed also "according to the strictest form" in the validation of his final hypothesis. And this validation is *De motu cordis.* As to what he means by "the strictest form," Harvey again provides the clue. It lies partly in the full and proper title of the book, *Exercitatio anatomica de motu cordis et sanguinis in animalibus,* that is an "Anatomical exercitation," and partly in what he says in his Prefatory Letter to Dr. Argent and the College of Physicians:

> And since this book alone affirms that the blood goes forth along a new path and returns again, contrary to the way which has been accepted for so many hundreds of years by countless most famous and learned men. . . , I was greatly afraid to suffer this little book . . . to come forth into public view . . . unless I had first laid it before you, proved it by ocular demonstration, answered your doubts and objections and received the verdict of your most accomplished President in my favour.

In these words Harvey has described the procedure of a disputation, and this is the structure of *De motu cordis.* The medical, or in this case the anatomical, disputation was still for the seventeenth century the accepted form for the evaluating and testing of a new hypothesis. So the structure of *De motu cordis* is as follows: chs. 2—7 contain the anatomical evidence on which the hypothesis will rest; each piece is described, tested experimentally and established; ch. 8 states the hypothesis which seems to account for all the evidence set out in the preceding chapters; chs. 9—17 prove the hypothesis by different methods; 9—14 set forth the demonstrative proof of the circulation and end with a formal conclusion in ch. 14; ch. 15 contains the proof from logical argument; chs. 16 and 17 contain the proof *e consequentia.* In ch. 16, Harvey explains that ch. 17, which uses the same kind of argument as ch. 16, is added because "it is my wish that it be established and supplied above all else with arguments based on anatomy.'' The only ''modern'' aspect of the book is provided by the Prefatory Discourse in which Harvey explains why he has undertaken this work and sets out at length all the contradictory opinions current in his day, just as it is now usual to begin any scientific paper by explaining why the work was done. All this I hope I have shown conclusively in the Introduction to my new translation of *De motu cordis*. The titles of the chapter headings in the Latin text, but not in the current English translations, make the structure plain. Throughout Harvey uses the terminology of the disputation as it had been used since the Middle Ages and was still in use in the universities in his day. The material which went into the book had indeed been accumulated over many years. The form in which it is presented in *De motu cordis* is one coherent whole.

DE MOTU CORDIS: WRITTEN IN TWO STAGES?

RESPONSE

JEROME J BYLEBYL

I find it rather difficult to respond to Dr. Whitteridge's critique, since she has largely ignored the major evidence on which I based my case, and on the other hand has treated certain minor aspects of my presentation as if they were of crucial importance. Thus she says that I have produced no evidence in which Harvey directly refers to an independent treatise on the movement of the heart, whereas the main point of my article was to call attention to just such evidence. I refer to the long Proem of *De motu cordis,* in which Harvey repeatedly identifies the heartbeat and the arterial pulse as the main subject of his disucssion, as well as chapter one, which is entirely devoted to an historical account of how Harvey came to write a treatise on this subject.

Dr. Whitteridge also says that I have produced no "external" evidence to verify my thesis, but again this is not the case. For I have pointed to Harvey's anatomical lecture notes, which corroborate most of the details provided in chapter one, and also show that the events described took place before the discovery of the circulation. More specifically, what the lectures show is that by 1616 Harvey had (a) spent many hours investigating the movements of the heart and arteries in live animals and (b) had written out a rough draft of his findings, a draft which reveals a definite plan of organization, as well as careful attention to mode of presentation. Indeed, Dr. Whitteridge herself quotes Harvey's statement in the lectures that "this error has been current for a very long time and therefore I have proceeded according to the strictest form because the subject is so ancient a one and has been studied by so many great men." In Harvey's day there was a very close relationship between the academic lecture and the printed treatise, so that when we find him going to such extraordinary lengths to prepare a lecture on the movement of the heart, we can probably assume that he did so with an eye toward future publication. At all events, it would have been no more unlikely for Harvey to have written a monograph on the movement of the heart than for him to have written a lecture on it, which of course he is known to have done. Moreover, the statement just quoted also points to another important source of "external" evidence in this regard, namely the existence of a vast body of literature on the heartbeat and arterial pulse, dating from antiquity through the early seventeenth century.[1] It was, in fact, a standard topic of investigation and published discussion for the anatomists, physicians, and natural philosophers of Harvey's day, and this makes it seem all the more likely that he would have devoted a special monograph to it.

Thus to summarize my major points: (1) the movement of the heart and arteries was a standard topic of investigation and discussion for the anatomical and physiological authors of Harvey's day; (2) by 1616, well in advance of the discovery of the circulation, Harvey had studied this subject in considerable detail, and had prepared a carefully constructed draft of his findings for his public anatomical lectures; (3) in the Proem of *De motu cordis* he repeatedly identified the subject of his present discussion as the movement of the heart and arteries, and chose the detailed contents of the Proem accordingly; and (4) in chapter one he gave a historical narrative of how he went on from his early investigations of and lectures on the heartbeat and pulse to prepare a formal monograph on the subject. There are, then, solid grounds for suspecting that Harvey originally wrote a monograph that was limited to the heartbeat and pulse, for which the Proem and chapter one would have been the appropriate introductions, and that only subsequent to the completion of this original work did he add his presentation of the circulation. And I have also pointed to a large body of additional internal evidence which seems to confirm this hypothesis.

Dr. Whitteridge's major response to the evidence of the Proem and chapter one is simply to dismiss it as arguing from "silence," as if it could have been mere happenstance that Harvey neglected to mention or even allude to the circulation in these important introductory sections, while going on at great length about the hearbeat and pulse. Further on she even offers the suggestion that when Harvey referred to his work on the heartbeat and pulse, he simply understood this to include the circulation as well—as she puts it, "What is the movement and use of the heart and arteries, if it is not the circulation?" However, even to the modern reader it must seem rather strange to use this phrase with reference to something that is so obviously a function of the heart, arteries, *and veins*. And when we consider the remarks in their original context, it becomes highly improbable that Harvey would have said "movements of the heart and arteries" when he meant "circulation." For as we have seen, the movements of the heart and arteries, both as an observed phenomenon and as a topic of formal investigation and controversy, had a quite precise and circumscribed meaning prior to the discovery of the circulation; Harvey himself originally took up his studies on these earlier, limited terms; and a major point of the later discovery was, precisely, the realization that the movements and functions of the heart and arteries cannot be considered in isolation from the veins, as they had earlier been. Furthermore, after the discovery of the circulation Harvey showed a clear awareness of the distinction between the two aspects of his work, as witness the very title of his treatise, *De motu cordis ET SANGUINIS*, and the opening statement of his letter to Argent, where he refers to "my opinion on the movement and use of the heart, and on the circuit of the blood." Indeed, at the beginning of chapter eight he states that it would be quite possible for someone to understand the movement and function of the heart, as described in the preceding chapters, but to have no inkling of the circulation, and it would be difficult to find a clearer acknowledgement of the distinctness of the two subjects. Perhaps there was even a touch of autobiographical irony in this remark, for if the contents of the preceding chapters could have been *understood* without a knowledge of the circulation, then the chapters themselves could just as well have been *written* without such knowledge. At all events, having personally experienced the vast difference between an understanding of the heartbeat and pulse, and a full awareness of the circulation, Harvey would not very likely have slipped into the careless habit of referring only to the former when he meant the latter as well.

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[1] For a standard airing of these controversies, see Andreas Laurentius, *Historia anatomica*, IX, qq. 6-10 (Paris, 1600), pp. 470-481. I shall discuss the pulse controversy in some detail in a forthcoming publication. For the present I list only the names of some of the participants for the period after 1300. C. Bauhin, Ugo Benzi, Capivacci, Cardano, Cesalpino, Chiocco, Coiter, Colombo, Costeo, Cremonini, Duret, Fallopio, Fernel, Guinter, C. Hofmann, Jacopo da Forli, Jessen, Joubert, Oddi, Pietro d'Abano, Ponce da Santa Cruz, Riolan, Sr. and Jr., Rogano, Rudio, Santorio, M. Savonarola, J. C. Scaliger, Sennert, Telesio, Teodosi, Torrigiano, Valles, Varoli, and Vesalius.

Thus the puzzle remains: why did Harvey write the Proem and chapter one as if he had intended to deal only with the issue of the heartbeat and pulse in the treatise that follows? I suppose that one could devise other hypotheses to account for this (Dr. Whitteridge seems to hint at possible strategic motives but does not say what these might have been), but the main virtue of such interpretations would be to avoid the implication of a straightforward reading, that Harvey did write an early treatise that was limited to these issues. Dr. Whitteridge seems to regard this idea as wildly improbable, indeed derogatory of the stature of *De motu cordis*, but the fact is that the authors of books (even ones that turn out to be "masterpieces") quite frequently change their plans in mid-course, and as I have shown, it would have been perfectly natural for an anatomist of the early seventeenth century to have written a treatise on the heartbeat and pulse. Thus it seems to me that my hypothesis should at least be admitted as the likeliest explanation of the available facts, and that the main burden of the opposing side is not just to denounce it, but to try to refute it on the basis of concrete evidence.

Of the specific objections that Dr. Whitteridge raises against this hypothesis, the only one that is potentially damaging to it is her final point, that *De motu cordis* reveals an overall unity of structure such as could only have been conceived from a single point of view, i.e., with the circulation clearly in mind. Specifically, she suggests that *De motu cordis* conforms to a particular literary format having a set order of procedure, namely the disputation. Now as one who has read his share of *disputationes* and *quaestiones*, including dozens on the heartbeat and pulse alone, I must say that I am more impressed by the differences than by the similarities between *De motu cordis* and this scholastic genre, and so I shall be interested to see how Dr. Whitteridge goes about establishing the identification in her forthcoming publication. However, quite apart from the question of whether *De motu cordis* is to be regarded as a disputation in any technical sense of the word, it is not too difficult to show that the treatise is comprised not of one overall structure, as Dr. Whitteridge suggests, but of two quite different structures, both of which are explicitly outlined by Harvey within the treatise itself.

The first such outline is to be found in the opening of the Proem, where Harvey states that he is about to discuss "the *movement, pulse, action, use,* and *utilities* of the heart and arteries (De cordis arteriarumque motu, pulsu, actione, usu, et utilitatibus)." Each of these terms had a technical meaning in Galenic physiology, and in his anatomical lectures as well as in *De motu cordis* Harvey showed that he was quite attuned to the nuances of these meanings.[2] And if we analyse the chapters of *De motu cordis* with reference to these terms, we find that some of them correspond exactly and in sequence to this listing, while others fall completely outside the scheme. Thus chapters two, three, and four are devoted exclusively to the movement or pulse of the heart and arteries, "pulsation" being the particular kind of movement that is characteristic of those organs; the aim is to give a bare descriptive account of these movements insofar as they can be observed in vivisection. Chapter five marks an explicit transition from "movement" to "action," and to illustrate the sense of these terms Harvey cites the example of swallowing as a single "action" that is composed of many individual

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[2] William Harvey, *The Anatomical Lectures*, ed. G Whitteridge (Edinburgh and London: E. & S Livingstone, 1964), pp. 4-24, *passim*.

"movements." Similarly, the diverse movements of the heart add up to a single, overall action, which is "a kind of swallowing and transfusion of blood from the veins to the arteries." Or again, "The one action of the heart (una actio cordis) is this transfusion of blood, and its propulsion to the extremes by way of the arteries." Chapters six and seven are devoted to vindicating this conception of the heart's action, which would by no means have been accepted by most of Harvey's contemporaries. An essential part of this proof was the defense of the pulmonary transit of the blood, but Harvey made it quite clear that he took up this topic only to the extent that it had a bearing on the proper understanding of the action of the heart. Thus the culmination of chapter seven is the assertion that through the hearts of all kinds of animals there does indeed occur "the dispensation of blood throughout the body."

Finally, chapter seventeen, by far the longest in the entire treatise, is a formal disquisition on the use and utilities of the heart and arteries. In it Harvey addresses himself to two teleological questions, namely why in general do animals have hearts, i.e., what overall purpose or "use" is served by the propulsive action of the heart; and how are the individual structural features of the heart and arteries well-suited to this overall purpose, i.e., what are their "utilities." The central theme of this chapter is that the heart is essentially "an impulsor . . . by which aliment is conveyed to the extremes of the body," the point being that without some such central propulsive organ the individual parts of the body would not receive their required supply of nutriment, whether this be the blood of the higher animals, or its analogue among the more primitive forms. Thus just as the action of the heart is to transmit blood throughout the body, so its use or purpose is to ensure the body a proper supply of nutriment. And the individual structural utilities of the heart and arteries are all related to this overall purpose.

Here I might note, parenthetically, that this is a very strange view of the purpose of the heart to be taken by someone who supposedly had the circulation in mind, and especially the circulation as presented in chapters eight through sixteen. For the author of these chapters sees the maintenance of the circulation as "the one cause" of the movement of the heart, by which he clearly meant the final cause. Furthermore, in establishing his case he repeatedly argues that the heart continually transmits to the arteries far more blood than the parts of the body could absorb as nutriment, thus making it quite clear that the function of the heart cannot be understood primarily with reference to the process of nutrition. Accordingly, in his discussions of the purpose of the circulation Harvey places primary emphasis on the process of vivification, as distinct from nutrition. When we come to chapter seventeen, however, all of this is suddenly forgotten. Instead, we are told again and again that the primary reason why animals have hearts is simply because of the difficulty of forcing an adequate supply of nutriment into the dense tissues of their bodies; and the only quantitative problem that seems to be envisioned is that of a deficiency of nutriment, not an excess. In higher animals the heart also serves to propel the blood through the pulmonary transit, which is related more to vivification than to nutrition, but this is explicitly dismissed as a secondary purpose, not found in all forms. And certainly this reference to the pulmonary circuit ought to have invited at least some mention of the role of the heart in sustaining the general circulation, yet there is none. In sum, the author of these pages is not only "silent" with regard to the general circulation, but seems to be profoundly ignorant of the idea.

Thus in the Proem Harvey outlines a program of inquiry into "the movement, pulse, action, use, and utilities of the heart and arteries," and this represents an exact and complete description of just those topics that are dealt with in my proposed early treatise. The Proem itself is avowedly a refutation of earlier ideas on these matters, while chapter one is an historical introduction to Harvey's own investigations of "the movements, use, and utilities of the heart." In chapters two, three, and four he presents the results of his findings regarding the movement or pulse of the heart and arteries, as observed in live animals. Chapters five, six, and seven represent a well-integrated effort to establish "the one action of the heart" that results from these diverse movements. And chapter seventeen is a survey of the uses and utilities of the heart and arteries, and one that is singularly obtuse with regard to the circulation.

Chapter one also provides another valuable clue for understanding this sequence of topics, for in it Harvey explicitly associates his own monograph on the heart with the plan of his teacher Fabricius to publish such specialized treatises for all the organs of the body. Thus as a secondary motive for publishing Harvey stated: "And I do this the more readily, because Hieronymus Fabricius of Aquapendente has delineated practically all the individual parts of animals, accurately and learnedly, in a special treateise, but has left only the heart untouched." And if we examine this series of Fabrician monographs, we find that they are nearly all based on a tripartite plan or organization, namely "Historia," "Actio," and "Usus et utilitates," "Historia" being a purely descriptive account of the part in question.[3] This was, of course, the method of anatomical presentation that had been recommended by Galen, but Galen and his later followers did not always follow it as rigorously as did Fabricius.

Thus the structure of my proposed early treatise is not only outlined by Harvey himself, it also conforms closely to a well-established model with which Harvey explicitly associates it. There is one important difference, that Fabricius's initial "Historia" is always an account of "structura" based on "dissectio," whereas Harvey begins with a descriptive account of "motus . . . ex vivorum dissectione." Now it certainly represents a profound change to give accounts of "actio" and "usus" that are based on observed "motus" rather than observed "structura," and this substitution epitomizes the change in physiological method with which Harvey is so closely identified. Nevertheless, strictly from the viewpoint of anatomical reportage the change is not so radical, since "vivorum dissectio" was traditionally included with simple "dissectio" as part of "Anatomia." Thus the shift from structure to movement as the focus of "historia" probably seemed to Harvey like an improvement upon, rather than a radical departure from, Fabricius's scheme, and as we have seen, Harvey had no hesitation about linking his own monograph with the latter's series.

On the other hand, Harvey's case for the circulation falls quite outside this plan of organization, and instead has a peculiar structure all its own. It is based, not on a categorical division into movement, action, and use, but on the proof of three specific "suppositions." These are outlined at the beginning of chapter nine, then proven in turn in this and the following four chapters, and then recapitulated in chapter fourteen. Chapter eight serves as an introduction, largely historical, to this presentation, while chapters fifteen and sixteen supplement the factual presentation with two different kinds of "probable" arguments.

Thus chapters eight through sixteen are much more explicitly logical both in organization and method than are the other parts of the treatise, and a number of factors point more particularly to Aristotelian methodology as a guiding influence in Harvey's presentation. First of all, in chapter eight he invokes a simple distinction between fact and purpose, such as is quite characteristic both of Aristotle's methodological writings and of his major biological treatises;[4] in this chapter Harvey offers some preliminary speculations as to the purpose of the circulation, but explicitly postpones the further consideration of this matter, in order to concentrate on the purely factual demonstration. Second, by basing his factual case on the proof of a series of propositions, Harvey seems to be trying to conform to the ideal of a "necessary" or "scientific demonstration," as proposed by Aristotle in the *Posterior analytics*.[5] And third, the further distinction between "necessary" and "probable" arguments is also one that is quite characteristically Aristotelian.[6]

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[3] Hieronymus Fabricius, *Opera omnia anatomica et physiologica* (Leipzig, 1657) *passim*; e.g., "De gula," pp. 99-107.

[4] E.g., *Post. anal.* I, ii, vi; *Hist. anim.* I, vi; *Part. anim.* II, i

Thus we actually find in *De motu cordis* two quite different plans of organization, the one taken over with little change from the Galenic anatomical tradition, the other based heavily upon Aristotelian methodological principles. Moreover, it is not just the case that these two structures are juxtaposed in the same treatise, but one of them disrupts and obscures the other, for by placing his case for the circulation immediately after chapter seven Harvey interrupted the transition from the action of the heart to the use of the heart. There were good reasons for Harvey to insert the case for the circulation where he did, but he was left with little choice but to place his account of the uses and utilities of the heart at the very end of the treatise, where it makes little sense in relation to the discussion of the circulation that immediately precedes it. To obscure this difficulty Harvey included a paragraph at the end of chapter sixteen in which he simply asserts that the following chapter will form part of the further verification of the circulation, and to further strengthen this impression he entitled the chapter "The movement and circuit of the blood is confirmed from the phenomena of the heart, and from those things which are evident from anatomical dissection." However, when examined more closely these cosmetic touches simply underscore the anomalous nature of the chapter, for in the first place anatomical observations, as a category of evidence, ought properly to form part of the main "necessary" demonstration, rather than of the subsidiary probable arguments; and in the second place, the particular anatomical phenomena described in chapter seventeen have no explicit relationship to the circulation as such, and in fact, as we have seen, the author of this chapter seems to have been singularly oblivious to the circulation at the time when he wrote it.

Thus the picture that emerges from the structural evidence is much the same as that suggested by my analysis of the Proem and chapter one, namely that Harvey originally wrote a treatise on the movement of the heart that was complete and self contained, conforming closely to a standard format for an anatomical monograph. Subsequently, he inserted a case for the circulation into the existing framework. The resulting composite had a high degree of *logical* coherence, since the original theory of cardiac action now became the primary basis for asserting the existence of the circulation, but this was purchased at the expense of considerable *structural* distortion, as is reflected most clearly in the peculiar situation of chapter seventeen.

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[5] *Post. anal.* I, ii, iv

[6] E.g., *Rhet* II, xxv.

Once again, I suppose that one could come up with other hypotheses to explain these structural anomalies, i.e., to try to show why Harvey might have deliberately planned it this way from the start, but here also it is a question of how far one is prepared to go in making improbable assumptions simply to avoid a conclusion that is not itself inherently improbable.

Most of the other points that Dr. Whitteridge raises are of minor importance for my overall case, and can be dealt with in summary fashion. I cite the addition to the lecture notes only as a parallel, and for this purpose it is irrelevant whether the addition was made slightly before or slightly after the completion of *De motu cordis*. The Royal Society Notes are also cited only for purposes of comparison, and while I perhaps made too much of this point, it seems to me that Dr. Whitteridge is overly skeptical as to the fidelity of the existing copy of the notes.

It is true that I allow for the possibility that Harvey may have written his separate treatise on the movement of the heart *after* the discovery of the circulation, but one would never guess from Dr. Whitteridge's account that I then go on to devote fully twenty pages to trying to show that it was most probably written *before* the discovery. I felt it necessary to separate the two issues because in her recent book Dr. Whitteridge suggests that even after the discovery of the circulation, Harvey continued to limit his College lectures only to the discussion of the movement of the heart. As she puts it, Harvey's lectures "show that, like Galileo, he continued to teach in accordance with the best authorities of his day at a time when he was beginning to realise, and ultimately to know, that some of these opinions were false."[7] Knowing of the close relationship that existed between Harvey's lectures and his published treatise, I anticipated that the same hypothesis of timidity might be invoked to explain why even after the discovery he might first write a monograph on the heartbeat and only subsequently expand it to include the circulation, and therefore it seemed best to raise this possibility myself and then try to rule it out. I quite agree with Dr. Whitteridge's present position that it is a most unlikely possibility, as applied either to the lectures or to the treatise, but I thought that it would be impolite and self-serving to treat it in so slighting a manner in my article.

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[7] G Whitteridge, *William Harvey and the Circulation of the Blood* (London: MacDonald, New York American Elsevier, 1971), p 110.

As regards the reference to the refilling of the auricles, I raised this as a potential difficulty in my theory, not as proof positive in support of it, and for this purpose I need only show that it might reasonably be taken as a later interpolation, not that it certainly is. Harvey's surviving manuscripts show quite clearly that he was in the habit of adding words, phrases, and sentences to what he had previously written, and the very fact that this particular statement is a parenthetical gloss on a single word makes it an obvious candidate for having been so inserted. And I remain firm in my original conclusion, that an analysis of its content in relation to the surrounding paragraph makes it seem more likely than not that it was a later interpolation.

Finally, there is the issue of the post-mortem contents of the pulmonary veins, left ventricle, and arteries. Essentially, it seems to me that Dr. Whitteridge has largely missed the point of my dicussion, which was *not* to assess *the accuracy of Harvey's observations.* I was fully aware that the factual situation is quite variable, and that it would be practically impossible to try to infer the frequency with which Harvey may have observed one situation or another, and therefore I restricted my commentary in this regard to one rather cautious statement, at the middle of page 464. And since this one statement is quite consistent with what Dr. Whitteridge reports, I am at a loss to say just where I have committed the factual errors that she imputes to me.

Instead, my primary focus was on *the consistency of Harvey's statements,* and even if, as Dr. Whitteridge maintains, the differences were only ones of emphasis, my basic point would still be valid, that Harvey chose to place exclusive emphasis on one side of the picture in the early parts of *De motu cordis,* and on the other side in chapter nine. However, I do not agree that these differences can in fact be wholly reduced to ones of emphasis, rather than outright contradiction. Thus in the Proem Harvey says that "I have very often experienced in dead men, as well as in other animals" that "the arteries contain the same blood, and *nothing but the same blood,* as the veins," while in chapter five we are told that "anatomists . . . in their dissections *always* find the pulmonary veins and left ventricle *filled with blood,* and that thick, congealing, and black"; on the other hand, in chapter nine we are informed that "in anatomy so much blood is found in the veins, but *little in* *the arteries;* and much in the right ventricle, little in the left,'' and further that the left ventricle and arteries, after death, are ''*as it were, emptied.*'' Dr. Whitteridge's reconcilation of these various statements involves (a) dismissing the statement in the Proem as a mere citation from Galen, even though it is avowedly presented as Harvey's personal observation, and even though the Galenic passage cited relates only to observations *in vivo;* (b) supposing that the ''always'' in chapter five relates only to some limited circumstance, not clearly identified, where the observation would ''always'' be valid; (c) supposing, on the other hand, that the first statement in chapter nine relates only to the public dissection of hanged human beings where this statement would generally be valid; and (d) to point out that the second statement in chapter nine does not refer specifically to *all* of the arteries. Similarly, Dr. Whitteridge introduces additional ad hoc assumptions to explain why each of Harvey's other statements might not mean what it seems to mean. And while some of these hypotheses may be more valid than others, it is again a question of just how much subtle exegesis we are prepared to engage in merely to avoid a simple, straightforward explanation, i.e., that before the discovery of the circulation Harvey avoided referring to certain phenomena that were embarrrassing to his position, whereas afterward he singled them out for emphasis precisely because he could now explain them, and thereby strengthen his case.

MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

APPOINTMENTS IN THE HISTORY OF MEDICINE

McGill University

McGill University announces the promotion of Dr. Donald G. Bates to the rank of professor and his appointment to the Thomas F. Cotton Chair of the History of Medicine. A native of Windsor, Ontario, Dr. Bates received his undergraduate and medical education at the University of Western Ontario, followed by two years of internship at Victoria Hospital in London, Ontario. In 1960, he joined the Institute of the History of Medicine, The Johns Hopkins University, serving successively as N.I.H. Fellow, lecturer, and assistant professor in that institution. A decade ago, Dr. Bates came to McGill as Associate Professor and as Chairman of the Department of the History of Medicine, a post which he still holds. He also served as Acting Osler Librarian from 1966 to 1971. Dr. Bates has a research interest in seventeenth-century medicine, particularly in the life and thought of Dr. Thomas Sydenham who was the subject of his doctoral dissertation at Johns Hopkins. He also has a strong teaching interest in the social aspects of medicine, both past and present, holding an appointment in that field in the Department of Epidemiology and Health. He has published in the areas of medical history, medical education, and medicine as a profession.

Donald G. Bates

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University of Toronto

Dr. Pauline Mazumdar has been appointed to the Hannah Chair of the History of Medical and Related Sciences at the University of Toronto. Dr. Mazumdar obtained her medical degree from the University of London in 1958. Her M.Sc. in immunology is from Brunel University, and her Ph.D. in the History of Medicine from the Johns Hopkins University. After residencies in surgery and medicine, she became Senior House Officer in Pathology at the Central Middlesex Hospital and then Assistant Physician in the Blood Grouping Laboratories at the Children's Hospital in Boston, Mass. She returned to the University of London as Williams Research Fellow at St. George's Hospital in 1962. In 1965 she became Research Officer, Department of Immunology at the London Hospital, and in the following year was appointed Assistant Pathologist in the North London Blood Transfusion Centre. In 1972 Dr. Mazumdar was Macy Fellow in the History of Medicine at the Johns Hopkins University and served as visiting professor, Institute for the Medical Humanities, University of Texas Medical Branch at Galveston in 1975. As might be expected from her scientific and historical background, her particular interests are in the history of immunology. Her Johns Hopkins doctoral dissertation was "Karl Landsteiner and the Problem of Species, 1838-1968."

Pauline Mazumdar

A TRIBUTE TO MORRIS FISHBEIN

On 20 October 1976, at a meeting of the Society of Medical History of Chicago, the Immediate Past President, Allen G. Debus, gave the following short sketch of the last of the founding members:

Since Morris Fishbein's death only three weeks ago, we have all read accounts of his life and work in our daily papers. I know also that many of you attended the memorial services held at Rockefeller Chapel last week. A lesser number may have reopened—as I did—the autobiography written when he was eighty. We have been reminded of his avid interests in many different subjects ranging from sports to literature to almost every conceivable aspect of medicine, and it would be hopeless to try to review all of this in the few minutes I have at my disposal this evening. Rather, let me say just a few words of Morris Fishbein—not Morris Fishbein, M.D., but Morris Fishbein, historian.

From the time of his entry to the University of Chicago history was one of the subjects Morris Fishbein cultivated and enjoyed. It was only natural then that he should have become deeply interested in the history of medicine. Now it is true that the bulk of his books and articles were written for the physician or to inform the lay public about medicine. But we should not forget that his historical works are of considerable, significance. He began with historical studies of quackery—understandable since so much of his own effort was directed against medical charlatans. His *Doctors at War* remains the basic study of the medical profession in the second World War. But above all his large history of the American Medical Association is a monument to his industry. This, too, stands as a basic study for all those interested in the development of medicine in this country. Besides his books there were a host of historical articles and one of the last times I visited him he was at work on a paper relating Benjamin Franklin to the foundation of the Philadelphia Hospital—a final Bicentennial offering from his to our field.

But Morris Fishbein was more interested in medical history than these writings might suggest. Many of you will recall his delightful discussion of the early days of our Society—a special meeting we arranged early in 1975 to celebrate the sixty-fifth anniversary of the founding of the Society of Medical History of Chicago. He spoke as a founding father since he was present at the first organizational meeting of this Society in 1909 where he acted as secretary for the organizing committee. Those of you who are here this evening know that he remained active in our Society as long as he was active himself. It was Morris Fishbein who edited the journal of this Society—surely one of the first significant periodicals devoted to medical history in this country. He served as the secretary of the Society for many years—and both he and Anna Fishbein spoke before us with regularity. At the University of Chicago he deposited his collection of rare early medical books—the priceless Fishbein Collection. I need say little more—with this lifelong interest in the history of medicine it is both fitting and proper that the Center for the Study of the History of Science and Medicine at the University of Chicago should be named the Morris Fishbein Center.

Let me add only that Morris Fishbein was not only an historian, he also was history. It will be impossible in future years to write the history of 20th-century medicine without referring to him as a central figure. For this reason, it is so very important that his personal papers covering some sixty years have been preserved. These are now part of the Fishbein Center and the Archives of the Regenstein Library at the University of Chicago.

As for us—and I know that I speak for all the members of the Society—we have lost a very good friend who loved our subject and who enjoyed our meetings. We will never cease to miss his company.

MORDECAI B. ETZIONY (1905-1976)

It was only after a long interval that the *Bulletin* received from Jerusalem the sad tidings of the sudden death, on March 14, 1976, of Dr. Mordecai B. Etziony, who had a number of times contributed to its pages. Born in 1905, Dr. Etziony spent most of his quiet and scholarly life in Montreal, where he was awarded the M.D. by McGill University in 1938 and where he was Assistant Physician at the Jewish General Hospital as well as Medical Registrar, Librarian, Archivist and Historian at the Maimonides Hospital and Home for the Aged, positions which he relinquished in 1970 in order to live in Israel. Dr. Etziony was a devoted Hebrew scholar, and *Bulletin* readers will remember his essays on the Hebrew-Aramaic element in Vesalius (*Bull. Hist. Med.*, 1945, *18:*413-424, and 1946, *20:*36-57), and on *Zori Hazup* (Balsam for the Body). This work by the medieval Jewish physician Nathan ben Joel, of which the *Bulletin* carried only excerpts (1936, *37:* 257-278), continued to occupy him after his emigration to Israel. Three years before his death he published *The Physician's Creed: An Anthology of Medical Prayers, Oaths and Codes of Ethics through the Ages.* Our tribute, although belated, records our grateful recognition of the scholarly attainment and the dedicated service of this kind, learned, and compassionate physician.

WALTHER RIESE (1890-1976)

The *Bulletin* has learned with regret of the death on June 30, 1976, of Dr. Walther Riese. An eminent neurologist, Dr. Riese was born in Germany on June 30, 1890, and received his M.D. degree at the University of Königsberg in 1914. Subsequently he became a member of the Neurological Institute in Frankfurt and was in the private practice of neurology in that city until 1933 when he left Nazi Germany for France, continuing his neurological work, first in Lyons, then in Paris. He came to the Medical College of Virginia as a Rockefeller Fellow in 1941, remaining at the Medical College until his retirement in 1960 as Emeritus Associate Professor of Neurology, Psychiatry, and History of Medicine. Dr. Riese was the author of some 277 articles and books on neurology, medical psychology, and the history and philosophy of medicine. Among his historical writings were *La pensée causale en médecine* (1950), *La pensée morale en médicine* (1954), *A History of Neurology* (1959), and numerous articles in the *Bulletin* and other journals. Dr. Riese was the recipient of many honors and held membership in a variety of societies, including long-time membership in the American Association for the History of Medicine. A Festschrift honoring Dr. Riese, which was in preparation at the time of his death, will be brought out as a memorial volume.

ANNOUNCEMENTS

NATIONAL NEWS

Los Angeles

In connection with the opening of an exhibit, "Leonardo Da Vinci, Anatomical Drawings from the Queen's Collection at Windsor Castle," the Los Angeles County Museum of Art held a special reception honoring the members of the Los Angeles County Medical Association on August 9, 1976. A program on the theme "Leonardo—Medical and Historical Observations" was presented, with addresses by Drs. Elmer Belt, Ilza Veith, Carlo Pedretti, and Donald Strong.

New York

On April 2-3, 1977, in cooperation with the Albert Einstein College of Medicine, the Institute for the Study of Human Knowledge held a Symposium, "Health and Healing: Ancient and Modern." The program included: "Holistic Approaches to Health" (David Sobel), "Healing and Curing in China" (Arthur M. Kleinman), "Mind/Body Relationships in Healing" (Jerome D. Frank), "The Relaxation Response" (Herbert Benson), "Navaho Indian Medicine and Medicine Men" (Donald F. Sandner), "Healers of India" (Peter Brent), and "Self-Healing" (René Dubos).

Philadelphia

Dr. George E. Gifford, Jr., gave the Richard H. Shryock Memorial Lecture at the College of Physicians of Philadelphia on November 10, 1976. His topic was, "Medicine and Natural History—Crosscurrents in Philadelphia."

Madison, Wisconsin

Two short courses in the History and Philosophy of Medicine have been announced by the Department of the History of Medicine and the Department of Continuing Education, University of Wisconsin. The courses are being offered on May 11, 1977, in cooperation with the American Association for the History of Medicine, immediately preceding the Association's annual meeting, May 11-14, 1977, in Madison. "Traditions of Medical Ethics: Rights and Duties," is being presented by Dr. Chester R. Burns, and "What the History of Professionalization of American Medicine Says to the Profession Today," by Dr. Robert P. Hudson. Each course meets the criteria of Category 1 credit for the Physician's Recognition Award of the American Medical Association.

Rockefeller Archive Center

The Rockefeller Archive Center of Rockefeller University in Pocantico Hills, North Tarrytown, N.Y., was opened in August 1975 to serve as the permanent depository for the historical records of the Rockefeller organizations and enterprises. On December 10, 1976, the Center held its second conference, "Trends in

Biomedical Research, 1901-1976," emphasizing the creation and development of the Rockefeller Institute of Medical Research and the Rockefeller University. Participants in the program were Robert S. Morison, Saul Benison, Abner McGehee Harvey, Michael Heidelberger, Pauline Mazumdar, Maclyn McCarty, Edward H. Ahrens, Jr., and Attallah Kappas.

State University of New York at Binghamton

The Center for Medieval and Early Renaissance Studies will hold its eleventh annual conference, October 21-23, 1977. The conference has as its theme, the Black Death and the impact of the recurring epidemics of bubonic plague on fourteenth-century society. Invited speakers include: J. M. W. Bean, Aldo S. Bernardo, Robert E. Lerner, Anthony Melnikas, George Rosen, and Siegfried Wenzel. Scholars wishing to present papers are invited to submit a one-page abstract, with its final title, by May 10, 1977, to Daniel Williman, CEMRS, SUNY-Binghamton, Binghamton, N.Y. 13901. Finished papers must be submitted by August 15, 1977.

PROGRAMS OF MEETINGS

Handerson Medical History Society, Cleveland, Ohio

January 17, 1977: "The History of Ideas in Nutrition," Helen B. Brown.

Johns Hopkins Medical History Club, Baltimore, Md.

January 18, 1977: "Pop Medicine: The Image of the Physician in Popular Culture," George Basalla.

Josiah C. Trent Society for the History of Medicine, Durham, N.C.

February 17, 1977: "The Most Dangerous Drug: Neurology and the Origins of Psychotherapy in Late Nineteenth-Century America," Stephen Wilkerson.

Society of Medical History of Chicago

December 1, 1976: "The History of Blood Transfusion," Israel Davidsohn.

January 26, 1977: "Illinois Neurology on the March," Louis Bosches; "The History of the Chicago Medical School," Mark Hilton.

February 11, 1977: "Towards a Definition of Occult Science in the Renaissance," Brian Vickers.

February 23, 1977: "The History of Melancholy (17th and Early 18th Century)," Thomas H. Jobe; Selections from recent medical music, from the collection of Walter Necker.

University of Virginia Medical History Society, Charlottesville, Va.

January 26, 1977: "Mass Immunization Two Hundred Years Ago," Genevieve Miller.

February 15, 1977: "The Responsible Medical Student: Medical Education in the Early Nineteenth Century," Lloyd G. Stevenson.

Washington Society for the History of Medicine, Washington, D.C

January 27, 1977: "Fear, Confusion, Optimism; Pediatricians and the Mental Hygiene Movement," Alice Smuts; "The Art of Healing in Pre-Hispanic Peru," Kehl Markley.

NEWS FROM ABROAD

Amsterdam

The Free University has announced that it is seeking candidates for the chair in medical history until recently occupied by Dr. G. A. Lindeboom. The selection committee wishes to consider foreign as well as Dutch candidates. Applications will be considered from physicians interested and experienced in the field of medical history or from qualified historians. The post entails responsibility for organizing courses in the history of medicine for medical and other selected students, promoting research in the field, and directing the medical historical institute. Applicants will have to concur with the Reformed charter of the Free University. Further information may be obtained from the chairman of the selection committee, Prof. Dr. T. D. Stahlie, Academisch Ziekenhuis der Vrije Universiteit, De Boelelaan 1117, Amsterdam, The Netherlands.

Germany

As of January 1977 Paul U. Unschuld has been appointed new editor-in-chief of *Subhoffs Archiv*. Another change in editorship is the departure of Professor Karl Rothschuh from the editorial board following his retirement from active university teaching. While *Sudhoffs Archiv* welcomes quality papers on the history of natural and physical sciences in general, it will continue to publish research findings in the history of medicine and pharmacology. Contributions are accepted in German, English and French. Scholars are encouraged to submit articles analysing and interpreting data from primary sources. The purpose is to make archival materials which may be accessible to only a few available to a wider community for further research.

London, England

The Wellcome Trustees have announced that University College London has agreed to give academic recognition to the Wellcome Institute for the History of Medicine. Academic developments and appointments to the academic staff of the Institute will be supervised by a joint University College/Wellcome Institute Committee on which the Wellcome Trustees will be represented. This arrangement has been made to bring University College and the Wellcome Institute, which is directed by Dr. Edwin Clarke, into closer association in order to promote the study of the history of medicine. Students at the Wellcome Institute will now be able to register for University of London degrees.

In 1966 a Unit of the History of Medicine was created in the Department of Anatomy at University College London, funded jointly by the College and the Wellcome Trustees. It is headed by Dr. W. F. Bynum who now holds a joint appointment at the Institute as Assistant Director of Studies. The new arrangement will bring the staff of the two institutions into closer association and afford unique facilities for the undergraduate and postgraduate study of the history of medicine.

London, Ontario

The Faculty of Medicine and the Department of Philosophy of the University of Western Ontario will sponsor a Colloquium on Biomedical Ethics, October 27-30, 1977. Papers are invited; the deadline for their receipt is June 1. Further information can be obtained from Prof. John Davis, Department of Philosophy, University of Western Ontario, London, Ontario N6A 3K7, Canada.

Mexico City

The Medicinal Plants Program (IMEPLAM) of the Third World Center for Economic and Social Studies (CESTEM) held its second colloquiam on the topic, "Evolution of Traditional Medicines in Contemporary Society," April 26-28, 1977. The colloquiam centered around a series of conferences, workshops, and round table discussions, with participation by specialists from Asia, Africa and Latin America. Coordinator of the program was Dr. Xavier Lozoya Legorreta.

BOOK REVIEWS

MAURICE B. VISSCHER (Editor). *Humanistic Perspectives in Medical Ethics*. Buffalo, N.Y.: Prometheus Books, 1972. xiii + 297 pp. $11.95.

Whenever important people attempt significant things the result merits close consideration. Physicians, medical educators, medical historians, and other health-related professionals are well advised to work through this book. Not just because a distinguished physiologist, physician, and medical educator edited it and wrote a stimulating introduction; but also because of what it may teach each of us about a revolution occurring in medicine in 1972 and which now appears to be in full force. All the specialities of medicine are undergoing dramatic historical change in the areas of human values and medical ethics. New standards of professionalism are being hammered out on the tempered-steel anvil of medicine by physicians and others in positions of responsibility. This book argues that medicine must absorb criticism and respond by leading in the process of change. As Visscher succinctly notes (p. xiii), "this humanistic orientation is not new to medical ethics, but human welfare is defined differently now from in earlier times, and the role of medical expertise in society is broadened." Thus, his hope is that this book "may assist in defining ethical goals for physicians" now and in the future.

Fourteen essays by sixteen authors make up this collection, and fall roughly into four groups. In the first group, two essays use a historical, and a third a social focus, to argue for increasing the humanistic dimensions of medicine. In "The Humanistic Tradition in the Health Professions: The Background of Medical Ethics," Chauncey D. Leake develops the theme of love for the art of medicine and love for humanity as a dual framework in which physicians and all health professionals function for patients and for society. Carleton B. Chapman and John M. Talmadge trace the evolution of the right-to-health concept in the United States, contending that the adoption of Medicare signified a fundamental shift in the medical profession's ethos. John G. Bruhn and Douglas C. Smith argue persuasively for broader ethical outlooks by medical educators in keeping with social reform forces.

A second group of two essays represents a more rigorous philosophical search for humanistic values in medicine. Marvin Kohl proposes the sanctity-of-life principle as a beginning point for discussions of euthanasia. Patrick Romanell calls for sorting out various kinds of moral conflicts in medicine through use of the framework of ethics as a philosophical discipline. The emergence of a new field called "philosophy of medicine" in recent years dramatically refines the focus of these two essays

A third group of five essays considers humanistic dimensions of delivering medical and health care. Sociologist H. Roy Kaplan criticizes the fee-for-service system, for creating a business-oriented ethical milieu for physicians, and calls for value changes by individual physicians and citizens so that equal health care will be mandatory. In "A Right to Die," Walter C. Alvarez reflects on sixty-six years of service as a physician. In spritely and interesting language, he argues that, though answers were not always easily found, physicians have dealt with complex ethical subjects like euthanasia in the

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past, and can do so in the future if intrusion by political forces into the professional situation can be minimized. An obstetrician and gynecologist, Howard C. Taylor, discusses recent profound changes in human reproduction and their consequences for physicians in the dual role of patient's companion and society's servant. In an essay on "Human Experimentation," Louis Lasagna illustrates complex and troublesome ethical questions facing medicine, science, and the public by the examples of polio immunization and the process of drug development. Leroy Augenstein took a similar approach in his essay on birth defects (1968), which is reprinted here. He warned that expansion of the tools and knowledge of medicine creates for every physician the necessity of defining and understanding one's ethical framework through reaching out for input and cooperation between specialists in society.

A fourth group of five essays concentrates on what might be called extraordinary or anomalous medical situations which require continuous and courageous vigilance. Mervyn F. and Deborah B Silverman talk about psychotropic drugs and medical ethics, concluding that physicians must rigorously evaluate the evidence regarding this social and physical abuse, and be willing to speak out against unreasonable medical standards which the public and officials attempt to invoke. Tome Murton, a physician and former prison warden, calls for changing the squalid and corrupt prison system which disregards minimal medical standards Gordon Livingston considers the clash between the goal of military medicine as a special system and the broader obligations of individual physicians. He concludes that physicians in the military must speak out against inhumanity wherever it is perceived to exist. Robert Jay Lifton's "Beyond Atrocity: A Personal Psychiatric View" proposes that man's descent into evil in times of war and violence is an extraordinary event which degrades human values and must in each case be identified and contained.

Several themes in the essays can serve as a focus for evaluating the book's value. The personal patient-physician relationship is being challenged repeatedly in the United States today by demands for the doctor to follow broader social definitions of his duty in practice. As Taylor suggests (pp. 167-168).

> "Now a radical change in social values is occurring with profound effects to religious precepts, public policy, and personal behavior. For the physician, adapting to the change has often been particularly difficult, partly because of the profession's long commitment to the older system, partly because, for him, the new ideas are not simply academic (as they may be for the sociologist) or impersonal (as for the legislator) but have immediate practical application to his daily work."

In suggesting this perspective the book is particularly valuable because it focuses the physician's experience into a broader perspective than he may have considered heretofore.

A generational model of medical practice, focusing on one individual's mind-set acquired in medical school, and requiring little change in a lifetime of practice has never been adequate in American medicine. Physicians have adapted to changes in medical knowledge and social expectations. Today medicine is under assault by technologic, educational, and social forces generating greater accountability in medical practice. The recent increasing exploration of physicians' values as part of their clinical skills seems a way for medical professionals to respond to this assault. Dr. Visscher is to be congratulated for collecting this group of essays as an introductory mix of theoretical and practical aspects of humanistic medical care in the evolving social context of the United States.

Reviewed by James Polk Morris, Ph.D., Department of the Humanities in Medicine, Texas A & M University, College Station, Texas.

Erna Lesky (Editor). *Wien und die Weltmedizin.* Wien-Koln-Graz: Hermann Bohlaus, 1974 (Studien zur Geschichte der Universität Wien, Bd. 9). 242 pp. Ill. D.M. 68.

Since its inception at a meeting held at Cornell University in Ithaca, during the 1962 International Congress of the History of Science, the International Academy of the History of Medicine has held a series of triennial symposia focusing on diverse topics: 17th century materia medica (Basel, 1964); theories of the structure of living matter and their influence on medicine (Liège, 1967); development of the basic sciences from the 17th to the 20th centuries and their influence on clinical medicine (London, 1970).

The papers on the first symposium were published together by the Academy's *Analecta Medico-Historica*, I (1966) while some of the works from subsequent meetings appeared in the Academy's *Clio Medica*.

The present volume brings together twenty contributions from those academicians who attended the fourth symposium in Vienna during the fall of 1973. Ably edited by Prof. Erna Lesky, the book focuses on the role of Viennese medicine in the development of Western healing concepts and practices. Although much has already been written on this topic by Max Neuburger, and the editor herself, a broad picture of mutual interrelationships between Vienna and the rest of Europe emerges from these contributions, spanning nearly two centuries from Gerard van Swieten and the First Viennese School to the works of Wagner-Jauregg and Sigmund Freud.

Among the more prominent and extensive papers is an article by Jóse M. López Piñero describing the effects of the First Viennese School on the Spanish medicine of the Enlightenment. J. K. Crellin presents the impact in Britain of Anton Storck's advocacy of eight poisonous plants as drugs, notably hemlock. He cites the growing critical attitude towards therapeutical claims so eloquently expressed by William Cullen. The late Dr. Charles Coury reviewed the beginnings of chest percussion from Auenbrugger to Corvisart, while Pedro Laín Entralgo, in a short personal account, recollected his own postgraduate studies in psychiatry, carried out in Vienna during 1932. In another interesting paper, Luigi Belloni examined the activities and writings of Giovanni A. Brambilla (1728-1800) of Pavia, a Viennese medical graduate who became the personal surgeon to the emperor, Joseph II. Brambilla worked hard to improve the technical and social conditions of surgeons, achieving a clear separation from barbers and near equality with physicians.

Any book attempting to cover so much ground in twenty papers is bound to be incomplete. One glaring omission is the lack of articles by American historians describing and analyzing the influences between Viennese and American medicine. As Noel Poynter stresses in his review of the 19th-century relationships between British medicine and the Vienna School, "Knowledge can best be advanced by an unrestricted flow of information and personal contacts between workers of different countries." It is my hope that such a fruitful exchange will likewise increase in medical history, and that the present leadership of the International Academy will find new avenues and opportunities for multinational dialogue on a more sustained and broader basis.

Reviewed by GUENTER B. RISSE, M.D., Ph.D., Department of History of Medicine, University of Wisconsin, Madison, Wisconsin.

GERT BRIEGER, *Theory and Practice in American Medicine*. Historical Studies from the Journal of the History of Medicine and Allied Sciences. New York: Science History Publications, 1976. xv + 272 pp. Ill. $12.00 (cloth); $4.95 (paper).

In *Theory and Practice in American Medicine*, Gert Brieger has killed two birds with one stone. In a tribute to the bicentennial year, he has published a collection of essays concerning the nation's medical history. By selecting the papers from the *Journal of the History of Medicine and Allied Sciences*, he has also paid a tribute to this important journal, which over the past three decades has greatly enhanced our understanding of the history of science and medicine.

In the volume Brieger has sought to cover as wide a range of topics as possible, and this he has done effectively. The essays are grouped under five categories: "Medical Education," "Medical Theory and Medical Research," "Medical Practice," "Surgery," and "Medical Care." Of these, I found the sections on "Medical Theory and Research" and "Medical Care" to be the most provocative. In the former, Saul Benison analyzes the contributions of the Rockefeller Institute to research in poliomyelitis; Phyllis Richmond portrays American attitudes toward the germ theory of disease between 1860 and 1880; and Jerome Bylebyl discusses aspects of the development of Beaumont's work on digestion. In the latter section, Gerald Grob describes how treatment in nineteenth-century American mental hospitals could be influenced by a patient's race, social class, and ethnic background; and Charles Rosenberg, in what is probably the book's most analytical essay, discusses the nineteenth-century dispensary. Useful features in addition to the essays include a bibliography of the principal secondary books in American medical history and a glossary of nineteenth-century fever terminology.

With the editor's careful selection of papers, this book serves as useful reading and reference for all those interested in American medical history. More important, as the editor himself states, the essays are included not necessarily to provide definitive answers but to raise questions, a task they also succeed in doing. Thus the book serves as a stimulus to future investigations as well.

Reviewed by KENNETH M. LUDMERER, M D., Department of Medicine, Washington University School of Medicine, St. Louis, Missouri.

LYDIA MEZ-MANGOLD. *A History of Drugs*. Basle: F. Hoffmann-La Roche, 1971, 175 pp. Ill.

"This book, which will be of interest to expert and layman alike, is not intended to be a comprehensive guide to the history of drugs, but merely an attempt to present to the reader some representative aspects of pharmaceutical history. This is achieved by incorporating illustrations of typical objects and items, accompanied by a brief explanation" (p. 9).

After a short historical sketch portraying the development of man's ideas about drugs and their role in medicine by Dr. Alfons Lutz, Mrs. Mez-Mangold presents a series of nearly one hundred coloured plates beginning with a Babylonian physician's seal and ending with a hand tableting machine dating from the early twentieth century. The plates vary in format, most measuring approximately 12 cm. by 18 cm. The work concludes with a list of "references" and a three-page index of names.

The textual material is interesting and correct, if neither profound nor original. The plates, both in their choice of subject matter and in their technical execution, are exceptionally fine.

Reviewed by PAUL POTTER, M.D., Ph.D., Department of the History of Medicine and Science, University of Western Ontario, London, Ontario, Canada

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WILLIAM C. GIBSON. *Wesbrook and His University*. Vancouver: Library of the University of British Columbia, 1973. xii + 204 pp. Ill. $7.00.

If the beginnings of most universities are plagued with frustrations and dashed hopes the early history of the University of British Columbia fits well into the common pattern. This might not have been so. With ordinary luck Dr. Frank Fairchild Wesbrook, the subject of this entertaining briography, might well have succeeded in establishing a splendid university within a short period of time.

Already experienced in medical school administration, he had much in his favour: public support and a provincial government that appears, from all the evidence, to have been eager to build and sustain a first-rate institution; a capable Minister of Education who had accepted his appointment on the understanding that he would have a free hand to establish a university and who had learned much from the experience of dozens of universities that had been established in the English-speaking world in the previous 50 years; an enthusiastic and competent, if somewhat confined, base upon which to build, the McGill College of British Columbia.

But luck was not with him. In the year and a half following his appointment as president of the University, Wesbrook achieved a great deal but the doubts preceding and the certainties following the declaration of war in 1914 delayed nearly all definite action and Wesbrook was robbed of the opportunity to reactivate his ambitious plans, for he died before the war ended. But, handicapped as he was throughout this period by illness, Wesbrook pursued his objectives with energy and intelligence, achieving all that was possible under the steadily increasing limitations to constructive action posed by the war. Against odds that would have discouraged a lesser man, he created a university—a far cry, indeed, from the "Cambridge on the Pacific" that he had at first envisaged, but basically sound—and Wesbrook's successors were to be grateful for the principles and the spirit that the first president had so firmly established.

The author, a native British Columbian and a graduate of the University, has made good use of the University's files, Wesbrook's correspondence, published papers and speeches, which reveal much of the man himself, of his early great successes as Dean of the Faculty of Medicine at the University of Minnesota, and of his struggles to repeat this performance in his own country. Of particular interest are Wesbrook's clearly expressed views on education which were, doubtless, regarded as radical in his day but which now can be recognized as prophetic. The book is well worth reading.

Reviewed by H. ROCKE ROBERTSON, M.D., Ottawa, Canada, formerly Professor of Surgery and Vice Chancellor, McGill University.

THE BULLETIN RECENTLY RECEIVED*

Arnaldi de Villanova. Opera medica omnia. II: Aphorismi de gradibus. Ed. Michael R. McVaugh. Granada-Barcelona: Universidad de Barcelona, 1975. xiv + 334 pp.$18.00.

Joseph H. Boyes. *On the Shoulders of Giants. Notable Names in Hand Surgery.* Philadelphia-Toronto: J. B. Lippincott, 1976. xii + 222 pp. Ill. $20.00.

Antonio Carreras Panchón. *La peste y los médicos en la España del renacimiento.* Salamanca, 1976 (Cuadernos de Historia de la Medicina Española, Monografías XXIX). 167 pp.

John S. Chapman. *The University of Texas Southwestern Medical School: Medical Education in Dallas, 1900-1975.* Dallas: Southern Methodist University Press, 1976. xii + 128 pp. Ill. $10.00.

Noel G. Coley. *From Animal Chemistry to Biochemistry.* Amersham, Bucks.: Hulton Educational Publications, 1973. 272 pp. Ill.

Harris L. Coulter. *A Divided Legacy: A History of the Schism in Medical Thought.* Volume II: Progress and Regress: J. B. Van Helmont to Claude Bernard. Washington, D.C.: Weehawken Book Co., 1977. xxiii + 785 pp. $17.50.

———. Homoeopathic Influences in Nineteenth-Century Allopathic Therapeutics. Washington, D.C.: American Institute of Homeopathy, 1973. iii + 83 pp. Ill.

T. Doby. *Development of Angiography and Cardiovascular Catheterization.* Littleton, Mass.: Publishing Sciences Group, 1976. xiv + 218 pp. Ill. $18.00.

Michael W. Dols. *The Black Death in the Middle East.* Princeton, N.J.: Princeton University Press, 1977. xvi + 390 pp. Ill.

Samuel Fastlicht. *Tooth Mutilations and Dentistry in Pre-Columbian Mexico.* Transl. by Maynette Heltzen and Ethel Comstock. Berlin: Buch- und Zeitschriften-Verlag "Die Quintessenz"; Chicago: Quintessence Books, 1976. 164 pp. Ill. $42.00.

Bruno Gebhard. *Im Strom und Gegenstrom, 1919-1937.* Wiesbaden: Franz Steiner Verlag, 1976 (Beitrage zur Geschichte der Wissenschaft und der Technik, Heft 14). 120 pp. Ill. DM 24.

Robert S. Greenblatt *Search the Scriptures. Modern Medicine and Biblical Personages.* 3rd ed. Philadelphia: J. B. Lippincott, 1977. xv + 173 pp. $8.95.

Der Grenzbereich zwischen Leben und Tod. Göttingen: Vandenhoeck & Ruprecht, 1976 (Vorträge gehalten auf der Tagung der Joachim Jungius-Gesellschaft der Wissenschaften Hamburg am 9. und 10. Oktober 1975). 177 pp.

Clive E. Handler (Editor). *Guy's Hospital: 250 Years.* London: Guy's Hospital Gazette, 1976. 232 pp. Ill.

Burton A. Konkle. *Standard History of the Medical Profession of Philadelphia.*

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* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine

Edited by Frederick P. Henry. 2nd ed., enl. & corr. 1973-1974, by Lisabeth M. Holloway, with an index and bibliography. New York: AMS Press, 1977. 544 + 24 pp. Ill. $67.00.

William P. LeFanu. *Notable Medical Books from the Lilly Library, Indiana University.* Edited by S. O. Waife, Elizabeth McCain, Ruth Weber and Charles F. Hammond. Indianapolis: Lilly Research Laboratories, 1976. 275 pp. Ill.

Charles Lichtenthaeler. *Geschichte der Medizin.* Köln-Lövenich: Deutscher Aerzte-Verlag, 1975. 2 vols. Ill.

"Paleopathology," *Medical College of Virginia Quarterly,* vol. 12, no. 2, 1976, pp. 39-86.

E. A. E. Raymond. *A Medical Book from Crocodilopolis: P. Vindob. D. 6257.* From the Contents of the Libraries of the Suchos Temples in the Fayyum, Part I. Wien: Verlag Bruder Hollinek, 1976 (Mitteilungen aus der Papyrussammlung der Oesterreichischen Nationalbibliothek, N S., X). 304 pp. Ill.

Walther Riese. *Il concetto di malattia: storia, interpretazioni e matura.* Traduzione Italiana di Giuseppe Ongaro; presentazione di Loris Premuda. Milano: Episteme Editrice, 1976. 159 pp.

Martin J. S. Rudwick. *The Meaning of Fossils.* Episodes in the History of Palaeontology. 2nd ed. New York: Science History Publications, 1976. 287 pp. Ill. $15.00 (hardbound); $6.95 (paperbound).

R. Ted Steinbock. *Paleopathological Diagnosis and Interpretation.* Bone Diseases in Ancient Human Populations. With a Foreword by T. Dale Stewart. Springfield, Ill.: Charles C Thomas, 1976. xvi + 423 pp. Ill. $22.75.

Studies in History of Biology. William Coleman and Camille Limoges, Editors. Vol. 1. Baltimore and London: The Johns Hopkins University Press, 1977. $14.00.

Joanne Trautman and Carol Pollard. *Literature and Medicine: Topics, Titles and Notes.* Philadelphia: Society for Health and Human Values, 1976. x + 209 pp. $5.00.

U.S. Army Medical Department. *Medical Statistics in World War II.* Edited by John Lada and Frank A. Reister. Washington: Office of the Surgeon General, Department of the Army, 1975. xvii + 1215 pp. Ill. $19.50.

U.S. Military Academy. *Military Medicine and the Wound Man.* West Point, N.Y.: 1976 (USMA Library Bulletin No. 15). iv + 41 pp. Ill.

U.S. National Library of Medicine. *Bibliography of the History of Medicine.* No. 10, 1970-1974. Bethesda, Md.: National Library of Medicine, 1976 (DHEW Publication No. [NIH] 76-1224). viii + 1069 + lxxxvii pp. $16.00.

Universidad Nacional de Córdoba. Instituto y Cátedra de Historia de la Medicina. *Historia general de la medicina Argentina.* Córdoba, Argentina: Dirección General de Publicaciones, 1976. 366 pp.

Katherina Wackerlin-Swiagenia. *Der "Schüpfheimer Codex," ein Medizinalbuch aus dem zweiten Viertel des 15. Jahrhunderts.* Aarau: Verlag Sauerlander, 1976 (Veroffentlichungen der Schweizerischen Gesellschaft zur Geschichte der Medizin und der Naturwissenschaften, 30). 187 pp. Ill. Swiss francs 30.

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THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
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THE HISTORY OF THORACIC AND VASCULAR SURGERY IN THE NEW ORLEANS AREA DURING THE FIRST HALF OF THE TWENTIETH CENTURY

ALTON OCHSNER

Louisiana has a rich heritage of vascular surgery partly because the Charity Hospital, the oldest hospital in the United States, afforded such rich clinical material of all types, especially luetic and traumatic vascular lesions, but principally because of Rudolph Matas, the father of vascular surgery (1-5). Dr. Matas also did a great deal to make thoracic surgery possible. His Chairman's address before the Surgical Section of the Louisiana State Medical Association in 1898 (6) was entitled "The Surgery of the Chest and Some of the Problems Related to the Treatment of its Penetrating Traumatisms and Their Immediate Complications." In this address he stressed the dangers of pneumothorax in thoracic trauma and stated, "Pneumothorax is always associated with a sudden free large opening of the pleura, an immediate collapse of the lung; (2) that while the phenomena vary in intensity according to the individuals and circumstances and in some cases are so slight, that they can be disregarded; (3) it is the duty of every surgeon, whenever he is about to undertake an operation on the chest or neighboring region which might involve the pleura, to assume that pneumothorax is inevitable and he must be prepared to meet the evil effects of acute atelectasis." He further stated, "A procedure that promises the most benefit in preventing pulmonary collapse in operation on the chest is artificial inflation of the lung and the rhythmical maintenance of artificial respiration by a tube in the glottis directly connected to a bellows." His review was excellent and in this and other publications he described the Fell-O'Dwyer apparatus (7, 8).

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Building on the important contributions to vascular and thoracic surgery of Dr. Matas, my associates and I have had the unique opportunity to further develop these fields in Louisiana. Thoracic surgery as a specialty is only approximately 50 years old. Previously, operations on the chest were feared because of the respiratory failure when the normal negative intrathoracic pressure was lost and also because infection, although hazardous in the abdomen, was much more dangerous in the thorax. Before anesthesia was available and the cause of infection was known, very few operations were performed and those usually only because they were possibly life-saving.

After completing my surgical training in the United States under Dr. A. J. Ochsner, I served as an exchange surgical resident in Zurich, Switzerland (1922-23) under Professor Clairmont, who only a few years previously had succeeded Sauerbruch, the Father of Thoracic Surgery, and who had developed a large thoracic surgical service at the Kantonsspital (the teaching hospital) at the University of Zurich. Because of this, I participated in the care of many thoracic surgical cases, including a large number of cases of carcinoma of the esophagus which was particularly common in Switzerland.

Also in Zurich I had an opportunity to make a serendipitous observation which was of subsequent value to me after having returned to the United States. The surgical residents at the Kantonsspital did all the endoscopies. On esophagoscopy of a patient with carcinoma of the esophagus, after anesthetizing the pharynx, the lesion was found to be at a different level than we had anticipated from the previous roentgenogram. The fluoroscopic room was immediately adjacent to the endoscopy room, so the patient was taken into the fluoroscopy room and given barium. Much to our consternation all the barium went into the trachea and bronchi and none into the esophagus. I didn't believe that we had perforated the esophagus. The patient fortunately coughed up the barium and was none the worse for the experience. The following day she was given barium again and this time it passed normally into the stomach. It was obvious that the anesthesia, which was the only different factor in the two days, was responsible for the aspiration. I didn't have an opportunity to investigate this until, after returning to the United States and spending a year in Chicago, I went to the University of Wisconsin as Associate Professor of Surgery where I was able to work it out. Although much had been known about the mechanism of swallowing, the entire mechanism was not understood. The swallowing act is a rather complex one, partly voluntary and partly involuntary. The voluntary act consists of the mastication of food following which the bolus of food is thrown into the pharynx by the tongue. When the bolus and/or tongue comes in contact wit the anterior surfaces of the anterior faucial pillars, the involuntary act is initiated. The involuntary act consists of a dual mechanism which occurs concomitantly. The upper end of the esophagus, normally tonically closed by the cricopharyngeus muscle which acts as a sphincter, relaxes, and the larynx moves upward to a point of safety beneath the epiglottis, prohibiting entrance into the glottis. The site of the initiating stimulus is a small triangular area about the size of an individual's thumb on the anterior surfaces of the anterior faucial pillars. If this area is anesthetized, one cannot swallow, and attempt at ingestion results in aspiration. This phenomenon explains why patients having tonsillectomies under local analgesia invariably aspirate, a condition which had puzzled laryngologists because local analgesia for tonsillectomy was considered safer than general anesthesia and yet the incidence of pulmonary abscess following local analgesia was higher than that following general anesthesia. For this reason many contended that post-tonsillectomy pulmonary abscess was due to embolism rather than to aspiration (9). While at the University of Wisconsin, I used this method of aspiration of contrast material to perform bronchograms. Because it was a simple method and could be done without any effort on the part of the patient, it was designated as the "passive technique" (10, 11).

After completing my exchange surgical residency at Zurich, I served as an exchange surgical resident in Frankfurt-am-Main, Germany, under Professor Schmieden, who at that time had the largest experience of anyone in the world with the surgical treatment of adherent pericarditis.

In March 1924, while in Frankfurt-am-Main, I attended the German Surgical Congress in Munich when the first successful pulmonary embolectomy for pulmonary embolism was presented by Kirschner (12). Although the operation of pulmonary embolectomy had been described by Trendelenburg (13) in 1908 and a sporadic number of cases had been performed, none had been successful because of the very short period of time elapsing between a massive pulmonary embolism and death. At Kirschner's presentation of the patient whom he had operated upon successfully a few months before, there was an electrification of the entire audience in the German Surgical Congress because this is the first time that such a person had been brought back from death. This had the effect of stimulating many surgeons on the Continent and in Scandinavia, including those in the clinic at Frankfurt where Professor Schmieden was chief, to attempt pulmonary embolectomy. Because of the very short time during which embolectomy is possible (the time between massive pulmonary embolism and death), it became customary in the clinic at Frankfurt to place a Trendelenburg set (which contained the instruments needed for embolectomy in a sterile pack) in the room of patients who were candidates for massive pulmonary embolism. Individuals with nonfatal pulmonary embolus were considered candidates for potential massive fatal pulmonary embolism. A skeletal crew remained by the patient's bedside waiting for the terminal event to occur. As one of the lowest men on the totem pole, many nights I sat throughout the night waiting for the patient to die, which fortunately occurred only infrequently. But when pulmonary embolism did occur, many cases of pulmonary embolectomy were successful. During these long vigils, I used to think that there must be a better way to handle such a case and that if we only knew where clots originated, we could interrupt the venous system between the site of the clot and the heart and prevent embolism, relieving the patient of being subjected to a hazardous operation (especially at that time) and permit the staff to go home and get some sleep. Later, when it became known that practically all of these clots originate in the veins of the lower extremity and/or pelvis, it was suggested that ligation of the venous system proximal to these sites could prevent fatal pulmonary embolism (14). Although we originally advocated superficial femoral vein ligation because it could be done relatively simply under local analgesia, we later observed a number of patients, who had had superficial femoral ligation, develop subsequent fatal pulmonary embolism. We therefore decided that inferior caval ligation was necessary.

After returning to the United States and spending a year in practice in Chicago and teaching surgical pathology at Northwestern University, I was invited to become Associate Professor of Surgery at the University of Wisconsin under Erwin Schmidt, who had just succeeded the then leading thoracic surgeon in the United States, Carl Hedblom, and who had developed a large and active thoracic surgical service at Wisconsin. I was placed in charge of this service. On the thoracic surgical service were many cases of pulmonary tuberculosis, requiring surgical therapy, many cases of acute and chronic empyema, and a large number of cases of bronchiectasis.

The University at that time had approximately 15,000 students and a fine student health clinic. I got permission from the Student Health Officials to examine all those students who were frequent patients of the clinic because of recurrent respiratory infections. I did bronchograms on about 600 of them. All were normal individuals except that they had had frequent colds since early childhood (as a rule in the first decade of life), usually starting with very severe pneumonitis associated with pertussis or with one of the exanthemata. Since that time they had been subject to frequent colds beginning with the first cold weather in the fall. We were astounded to find that approximately 70 percent of these young, otherwise healthy, individuals had evidence of bronchiectasis, usually of the cylindrical type. This was an entirely unexpected finding. Following bronchography, the students on return stated that they felt better. I thought that this was a psychic effect because it is well known that frequently after a diagnostic procedure the patient thinks that the "treatment" helped him. When I inquired in what way they were better, they said because their sputum was less. This, if true, could not be psychic. So the next group of students examined were asked to measure their sputum and I did bacterial counts on the sputum before the lipiodol instillation. Following bronchography there was invariably a decrease in the quantity of the sputum and a marked decrease in the bacterial count, from the neighborhood of billions per cubic millimeter to millions or even thousands per cubic millimeter. Their cough became less, they felt better, and they gained weight. It was obvious that the instillation of the lipiodol, which is a product of 40 percent iodine in poppy seed oil, exerted a beneficial effect. I postulated that it was beneficial in two ways. The oil, being heavier than the sputum, displaced the sputum upward which made it possible for it to be expectorated, and second, free iodine was gradually liberated which acted as a bactericide. In patients with bronchiectasis, the oil remained in the bronchiectatic cavities for long periods of time, in contradistinction to the normal person in whom the cilia transport the foreign substance upward to permit it to be expectorated. We were able to find iodine in the urine of the patients with bronchiectasis for as long as two weeks. At that time there was no safe curative therapy of bronchiectasis since surgical treatment was associated with a prohibitively high mortality rate, and the conservative therapy, consisting largely of postural drainage, was very unsatisfactory. Because of this, I employed repeated instillations of lipiodol in individuals with bronchiectasis. Another astounding observation was that in a small percentage (10 percent) with definite bronchiectasis, subsequent bronchograms showed the bronchial tree to be normal.

It was obvious that these individuals could not have organic, pathologic bronchiectasis and I postulated that theirs was a "functional bronchiectasis" (15). Their bronchial dilatation was explained on the basis of relaxation of the bronchial musculature as the result of infection, because it is known that in the presence of infection, smooth muscle relaxes. As a result of the Wisconsin experience, I also postulated that bronchiectasis probably began early in life, usually in the first decade, as the result of a severe pneumonitis usually associated with or following pertussis or one of the exanthemata.

After spending a year at the University of Wisconsin, I was invited in 1927 to succeed Dr. Matas as Professor of Surgery at Tulane University. At that time there was virtually no thoracic surgery being done in the deep South because thoracic surgery was just developing, and because there were no antibiotics, thoracic infections were especially dangerous. What was done consisted of drainage of empyema, drainage of pulmonary abscess, and the artificial pneumothorax therapy of tuberculosis. Because of the prohibitively high mortality rate, surgical treatment of bronchiectasis was not considered and carcinoma of the lung was almost non-existent. As a matter of fact, while I was a medical student at Washington University in 1919, a case of cancer of the lung was admitted to the Barnes Hospital and, as usual, the patient died. Dr. George Dock, our eminent Professor of Medicine, who was also an excellent pathologist, in addition to being a splendid internist, had the junior and senior classes witness the autopsy because, as he said, the condition was so rare that he thought we might never see another case as long as we lived. I was very young (a junior student) and greatly impressed by the rarity of this condition. I did not see another case until 1936, when at the Charity Hospital in New Orleans I saw nine cases in six months. Because I had been impressed with the rarity of it 17 years previously, this relatively large number of cases seen in such a short period of time represented an epidemic and there must be a cause. They were all men; all smoked cigarettes heavily, and all began smoking during the First World War. On looking up the statistics, I found that there were very few cigarettes consumed until the First World War, following which there was a tremendous increase in the consumption of cigarettes, and I had the temerity at that time to say I thought the cause of this new epidemic was the use of cigarettes. The evidence was pretty nebulous at that time and of course no one would believe me.

Although artifical pneumothorax as the treatment of pulmonary tuberculosis had been employed in Louisiana, no other collapse measures were used before I came to New Orleans. The Charity Hospital had a large tuberculosis unit and I had great opportunity to treat a large number of cases of tuberculosis which required collapse therapy (16-19). The procedures which were used surgically were phrenic nerve operations, thoracoscopic division of pleural adhesions, pneumonolysis, and thoracoplasty.

Although venous thrombosis and pulmonary embolism was less frequent then than at the present time, it did occur and we became interested in it. There were no anticoagulants available and Mahorner and I (20-22) investigated the use of leeches in the treatment of venous thrombosis and the prevention of pulmonary embolism. We found that the hirudin secreted by the leech acted as an antithrombic agent and also tended to lyse the clot once it formed. Subsequently, DeBakey and I (23-25) became interested in this very important problem and called attention to the increasing importance of thromboembolic disease and the necessity of prevention of pulmonary embolism by various methods, principally, venous interruption.

In 1939, DeBakey and I (26) reported our first seven personal cases of carcinoma of the lung and were able to collect 79 reported cases. Our first patient, who bled massively from a sarcoma of the bronchus, was operated upon in 1936, a boy 19 years of age. This apparently was the 10th successful pneumonectomy for malignant disease and was the first one done in the deep South. There was a great deal of interest in this case and many attended the operation. After removal of the lung and closure of the bronchus, the pathologist returned to the operating room and said there was no tumor in the specimen. Dr. Francis E. LeJeune, who had done the bronchoscopy, was present and he stated that the tumor was immediately beyond the carina. We then removed the remaining portion of the bronchial stump right up to the carina and that segment contained the malignant tumor. This patient is still well, 39 years later, and is a farmer in Mississippi.

As mentioned previously, we postulated that the cause of cancer of the lung, which was a new disease, was probably cigarette smoking. Because this new disease was increasing relatively rapidly and because there were relatively few thoracic surgeons, we had a tremendous experience in the treatment of bronchogenic carcinoma. In the beginning, I reasoned that all patients with bronchogenic carcinoma who could tolerate it should have a pneumonectomy, because a surgical axiom in treating cancer is that a wide initial resection with en bloc lymph node resection gives the greatest chance of cure. For many years I advocated total pneumonectomy with en bloc excision of the regional lymph nodes in all cases when such was possible. In 1940, we stated (27), "Simple lobectomy obviously does not permit removal of the regional lymph nodes and the relatively high incidence of lymph node involvement is sufficient indication that the regional lymph nodes be extirpated together with the primary lesion. For this reason, the only rational procedure is total pneumonectomy." In the same publication we called attention to the fact that in the autopsy series of the Charity Hospital, carcinoma of the lung had become more frequent than carcinoma of the stomach which, prior to that time, had been the most frequent cancer in men. Early preliminary pulmonary function studies were not done, but we did preliminary pneumothoraces and in this way we could determine whether a person could tolerate a pneumonectomy or not. Also, it was erroneously thought that the gradual collapse of the lung by pneumothorax was desirable and protective.

Early in my experience, before I realized lung cancer was angioinvasive, I contended that if carcinoma could be diagnosed earlier, the results would be better. "Bronchogenic carcinoma is a lesion which is amenable to surgical extirpation and I believe that the five-year salvage rate can be well over 50 percent, if not approaching 75 percent." As the result of my contention that pneumonectomy was desirable in all cases, at the national meetings there was always a controversy between the Boston group, namely Churchill and Overholt (who had begun using less radical procedures for pulmonary carcinoma) and us. I criticized these investigators because I felt that they were not giving the patient the best chance for a cure. After considerable argument on both sides, Mike Shimkin (28), who was then at the National Cancer Institute, asked Overholt and us to submit our cases to him at the National Cancer Institute for review and evaluation. This we did. The groups were similar in size, all private patients, and both were treated by excellent surgeons. The results showed that the mortality rates in the two series were approximately the same, but I was shocked to learn that the five-year survival rate in both cases was also approximately the same. The only difference was that Overholt's cases had less morbidity than ours because they had more functioning lung. It was obvious that my original contention was wrong. My premise that a radical procedure in the beginning gave the best chance of cure was sound, but I did not understand the natural history of bronchogenic carcinoma, which we now know is early angio-invasive. This was first described by Julian Johnson et al (29), of Philadelphia. Dr. Hurst Hatch, who is in charge of our Pulmonary Function Laboratory and who is an eminent medical chest man, conceived the idea that if the vein draining a viscus with cancer contains more cancer cells than the systemic circulation (in carcinoma of the colon the portal vein contains more tumor cells than the rest of the circulation) in carcinoma of the lung there should be a higher percentage of tumor cells in the peripheral arterial blood. (Blood leaving the lung traverses the short pulmonary veins, to the left heart, and then directly into the arterial system.) He and Dr. G. M. Carrera of our Pathology Department, did cytologic examination on the peripheral arterial blood of all patients going though the Pulmonary Function Laboratory. In patients without lung cancer, no tumor cells were found; there were no false-positives. However, of those who had cancer of the lung, 30 percent had tumor cells in their peripheral arterial blood (30). This did not influence us in our therapy because the mere finding of tumor cells in the peripheral arterial blood does not necessarily mean that the patient will get distant metastases, because blood is tumorcidal, but the potentiality for the development of distant metastasis is present. Because of the propensity of lung cancer to invade the vascular system early, and produce distant metastases early, lung cancer has a very poor prognosis. When lung cancer is diagnosable, there is a possibility of its having already extended beyond its originating focus.

Among our many studies on carcinoma of the lung (31-52) we reported two cases (53) in which pneumonectomy was done for primary carcinoma in which metastasis subsequently developed in the axillary nodes. These apparently were the first two cases in which this site of metastasis was reported. An elaborate study of the incidence of metastasis was based upon a review of 3,047 collected cases of carcinoma of the lung.

Although I, early in my experience, believed that by earlier diagnosis more cases could be cured and at one time suggested that a 75 percent salvage rate might be attainable, I was so wrong. The tragic thing about cancer of the lung is that it is increasing more than any other cancer and is by far the more frequent cause of death from cancer in men. In addition to this, most cases are preventable in that they are caused by smoking. Moreover, the salvage rate in cases of cancer of the lung is virtually no better now than it was 20 or 30 years ago. The five-year salvage rate in all the cases seen is between 5 and 6 percent—and this is a disease which is largely preventable.

In 1952, DeBakey, DeCamp, and I (54) reported our results with 948 cases of bronchogenic cancer, with a 54 percent operability rate, a 35 percent resectability rate, and a 28 percent hospital survival rate. The five-year survival of the entire group was 6 percent and of those in whom resection was possible, 19 percent. We have subsequently treated many more cases of lung cancer, but since this presentation contains the experience only up to 1953, they are not included.

Because of another serendipitous observation, DeBakey and I (14, 55-59) differentiated between two types of venous thrombosis which we designated thrombophlebitis and phlebothrombosis. Although Virchow, the great German pathologist, had differentiated between these two types pathologically, their clinical significance was not appreciated. We felt that it was essential to differentiate between these two types although they had one thing in common, namely, a clot in the vein, they were different in every other way—their mode of mechanism of production, the type of lesion, the symptoms, the prognosis, and the therapy. In one, thrombophlebitis, there are many symptoms and frequent disability but there is little risk of death except in suppurative thrombophlebitis. On the other hand, in phlebothrombosis there are minimal or no symptoms but a risk of pulmonary embolism which may be massive and fatal. We were able to demonstrate by clinical and experimental investigation that although the lesion in thrombophlebitis was in the deep venous system, the clinical manifestations (pain, coldness, and blanching of the involved extremity) were due to arteriolar spasm, which could be relieved by sympathetic block. In 1950 we (58-61) demonstrated that *in vitro* clotting is inhibited by alpha tocopherol in the presence of calcium. Since this time I have used alpha tocopherol postoperatively in patients who are candidates for phlebothrombosis. We also became interested and did considerable work on postphlebitic sequelae which were seen frequently in the wards and outpatient clinic at Charity Hospital (62-93).

Because the Charity Hospital had a large number of Negro patients and because the Negroes used lye in the household for cleaning, we saw many cases of lye burn stricture of the esophagus. They were usually young children who drank lye solution. In addition to the immediate treatment of these cases which represented a great problem before the days of antibiotics, there was the danger of subsequent stricture. Owens and I (94) in 1934, did the first anterothoracic esophagoplasty for impermeable stricture of the esophagus in such a case.

Carcinoma of the esophagus, although not as common as it is now, was frequently seen in the Charity Hospital and we had considerable experience with it. The mortality rate was extremely high in the beginning following surgical extirpation but, with the increasing number of patients operated upon, the mortality rate became less. In an extensive review of the literature in 1941, in which 195 cases were collected, we added four of ours (95). In 1942 right thoracotomy combined with laparotomy was advocated for midthoracic lesions (95-100). In 1948 DeBakey and I (101) reported 200 cases seen at Charity Hospital, of which only 58 (29 percent) were considered operable and only 32 (16 percent) were resectable. The operative mortality in the first 16 personal cases was 75 percent, in the last 16, 31 percent, and the last 8, 25 percent.

We also had fairly large experience with the treatment of congenital tracheoesophageal fistulae in the newborn (102). Because of the necessity of making a diagnosis as early as possible before aspiration from the blind pouch had occurred, Gage and I suggested that all newborn babies, immediately after birth, have a catheter passed into their pharynx to determine whether the esophagus was open or whether it ended in a blind pouch. In this way earlier diagnosis was possible before massive aspiration had occurred.

Gage, DeBakey, and I (103) in 1935 did considerable work on the scalenus anticus (thoracic inlet) syndrome. In 1939 DeBakey and I (104) investigated the problem of congenitally depressed sternum (chone-chondrosternum). We were able to collect 31 reported cases to which we added our experience. In 1940 (105) and in 1941 (106) we reviewed the literature on achalasia and added our experience. In 1936 Dixon and I (107) treated and reported a case with superior vena caval thrombosis following trauma and reported an extensive review of this subject. We also had opportunity to observe and treat many mediastinal lesions (congenital, inflammatory, and neoplastic) (108-110).

Because New Orleans has a semitropical climate and also because it is a port city, in the late '20s and '30s there was a large number of cases of amebiasis admitted to the Charity Hospital and many had amebic hepatic abscess. This gave DeBakey and me (111) a chance to study these cases carefully and we observed 15 cases of pleuropulmonary complications of amebiasis and were able to collect 153 reported cases. Most of these followed amebic hepatic abscess although some were hematogenous in their origin.

DeBakey and I became very much interested in peripheral vascular disease and the role of the sympathetic nervous system. We performed many experimental and clinical investigations (112-116). We demonstrated that lower extremity ischemia caused by arteriosclerosis, especially when complicated by diabetes, was frequently relieved by sympathetic block or ablation. Adriani, Parmley and I (117) reported three fatalities and four complications following stellate ganglion block.

Early in my experience bronchiectasis was not amenable to surgical therapy because of the prohibitively high mortality rate—this was before the availability of antibiotics and individual ligation of the hilar structures was not done (11, 15, 118-123). The reported mortality rate following the surgical treatment of bronchiectasis at that time was approximately 50 percent. Usually mass ligation of the hilar structures was used which increased the incidence of infection, which frequently was the cause of death. The results were so bad following resection that Graham (124), in order to obtain better results than by conservative therapy, advocated cautery pneumonectomy which consisted of opening the chest, burning into the lung with a red hot soldering iron, opening up the dilated bronchi and allowing the lung to become attached to the wound resulting in multiple bronchial fistulae. In this way there was drainage of the abscesses and the patients were relieved of many of their symptoms. They were certainly not cured. In 1949, DeBakey, DeCamp, and I (125) reported 96 cases of bronchiectasis having had 105 resections from January to May 1948. The median duration of the symptoms was ten years. The hospital mortality before 1942 was 46.1 percent; after 1942 (83 cases), it was 0. The total hospital mortality rate was 6.25 percent; 5.2 percent subsequently died. End results: 67 percent were cured; 12 percent improved; 6 percent were unsatisfactory; 11 percent were dead.

In 1944, I did the first successful resection of a saccular aneurysm of the aortic arch (126). A 44-year-old patient was found to have a mediastinal tumor immediately adjacent to the aortic arch and the possibility of an aneurysm was considered, but because there was no pulsation (before the days of frequent angiography), it was felt that it was a mediastinal tumor. The patient was operated on; in dissecting the lung away from the mass, it began to bleed and then I could see that it was a saccular aneurysm. Because the bleeding continued and was quite brisk, I was afraid not to do something and in desperation I put two Ochsner forceps across the pedicle. The aneurysm was removed and a row of interlocking cotton sutures were inserted proximal to the clamps and the clamps were removed with a good deal of trepidation. The bleeding was brisk, but with pressure it stopped. I thought that I had been very fortunate until I spoke to the head of our Department of Medicine. He suggested that when the spirochetes began to act on the cotton that the thing might blow out (at that time all aneurysms of the aorta were considered luetic). Fortunately, the patient has remained well. Tuffier had operated upon a similar case a number of years before, but the patient died several days after the operation.

Although subphrenic abscess may be considered by some as not a thoracic condition, it probably should be considered as such. While in Zurich, Nather and I (127) had the opportunity to develop a technique for drainage of the most frequent type of subphrenic abscess at that time, the right posterior-superior space abscess. This technique made it possible to reduce very materially the mortality rate following the drainage of these abscesses, because it was done without contaminating an uninvolved serous cavity which occurred when transpleural or a transperitoneal drainage was used (128, 129).

Between 1927, when I came to New Orleans, and including 1952 (but nothing beyond that date), 100 publications on thoracic and vascular disease, including eight on subphrenic infections, have appeared: 26 of them on venous thrombosis; 24 on cancer of the lung; 11 on bronchiectasis; 13 on diseases of the esophagus; 4 on surgical treatment of tuberculosis; 3 on the heart and pericardium; 2 on mediastinal lesions; 4 on general thoracic subjects, and one each on thoracic complications of amebiasis, lung abscess, chylothorax, resection of aortic aneurysm, the chest, and fatalities and complications of stellate ganglion block.

In addition to these activities, while President of the American Association of Thoracic Surgery in 1948, I appointed a committee to organize the Board of Thoracic Surgery, establish a founders group, appoint an examining committee, and set up requirements for training and certification in thoracic surgery.

During the preparation of this review, I became increasingly embarrassed because it seemed as if I had lost all humility, an attribute I cherish greatly because I have much to be humble about. I sincerely hope that I have been able to convey to you that I have been most fortunate in beginning my surgical career at the time of the birth of thoracic surgery and in having an excellent training in the new specialty. Also I was extremely fortunate to succeed two eminent leaders in thoracic and vascular surgery, Carl Hedblom and Rudolph Matas, and to be associated with two great institutions, the University of Wisconsin and Tulane University.

Finally, I have had the extreme good fortune to be associated with the finest group of young, energetic, gifted and stimulating young surgeons from whom I have learned much and obtained great stimulation and gratification. Because of this I pray you will forgive me this apparent lack of humility by stating it as it was.

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THE PEDIATRICIAN AS HISTORIAN: JOHN RUHRÄH AND ERNEST CAULFIELD.*

GEORGE ROSEN

I

It is not unusual for physicians to become interested in the history of medicine, and more specifically in the development of disciplines or specialities with which they are associated. More often than not this interest remains limited to reading a few well-known classics, collecting books and objects related to the medical past, and perhaps occasionally presenting a paper to a medical society. Not infrequently, however, such interests lead to investigations, pursued more seriously and intensively in terms of scholarly criteria and scientific methods, which contribute to an elucidation and understanding of medicine past and present. Representative of such physician-historians are Rudolf Virchow, Theodor Billroth, Julius Hirschberg, T. Clifford Allbutt, D'Arcy Power, William Osler, David Riesman, Arnold C. Klebs, Harvey Cushing and John F. Fulton. The list could be greatly enlarged but this sample adequately illustrates the point. As members of this group I wish to present two pediatricians, John Ruhräh and Ernest Caulfield. Both contributed significantly to the history of pediatrics, but they differed considerably in character, in the subjects they studied, and in their styles of doing medical history.

II

John Ruhräh (1872-1935), a leading pediatrician during the first third of this century, was born at Chillicothe, Ohio, on September 28, 1872, and received his early education in the local schools.[1] Upon graduation from high school in 1891, he left for Baltimore, the home of his maternal grandmother, to study medicine. There he entered the College of Physicians and Surgeons, and in 1894 received his M.D. During the subsequent three years (1894-1897) Ruhräh held the posts of Assistant Resident Physician and Resi-

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* Presented February 6, 1974, at Grand Rounds, Department of Pediatrics, Yale University Medical School.

[1] The biographical information presented here has been taken from Fielding H Garrison, "In memoriam Dr. John Ruhrah (1872-1935)," *Bull. Hist Med*, 1935, *3*. 400-402, and Wilburt C. Davison, "John Ruhräh (1872-1935)," *Pediatric Profiles*, ed. Borden S. Veeder (St. Louis: C. V. Mosby, 1957), pp. 149-154.

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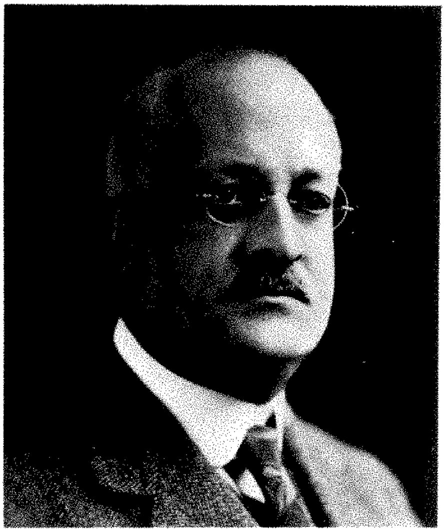

John Ruhräh (1872-1935)

dent Physician at Mercy Hospital. Concurrently he took post-graduate courses at the Johns Hopkins Hospital and served as demonstrator in bacteriology at Physicians and Surgeons. Upon completion of his hospital training, Ruhräh went to Paris in 1897 for advanced study at the Pasteur Institute. Bacteriology and immunology were then the outposts of medical science and Ruhräh was clearly aware of the need for training in these disciplines. When he returned to Baltimore he was prepared for the responsibilities given to him, to head the Pasteur Department of his *alma mater* and to be the quarantine physician of the port of Baltimore, posts which he held for two years (1898-1900). About this time Ruhräh was also appointed associate professor of children's diseases in the Maryland Faculty (1898-1899). He spent the year 1900 doing post-graduate work in pediatrics in Vienna, Berlin, Paris and London. The following year (1901), on his return to Baltimore, Ruhräh set himself up in private practice, and a year later he was appointed clinical professor of the diseases of children, therapeutics and internal medicine in the Maryland Faculty. In 1906, he became full professor of the diseases of children, and in 1915 when the College of Physicians and Surgeons merged with the University of Maryland to form the present medical school, Ruhräh became its Professor of Pediatrics, a chair he held until his death in 1935.

Ruhräh made a number of contributions to medical science and practice, e.g. his investigation of actinomycosis (1899–1900), and his introduction of the soybean into infant dietetics (1909). He also wrote several books on pediatrics and dietetics. In 1917, after the polio epidemic of the preceding year, he published a book on *Poliomyelitis in all its Aspects*, an ironic interest in the light of the fact that in October 1930, at the age of 58, he was stricken with poliomyelitis while travelling in Italy. Ruhräh was active in many medical societies, serving as president of the American Pediatric Society in 1925, and of the American Academy of Pediatrics in 1934.

John Ruhräh's interest in and approach to the history of pediatrics stemmed from several sources. In the first place it was a clear expression of his profound attachment to children and his sense of identification with those in the past who tried to help them. Secondly, he was a man of broad cultural interests, with a marked artistic bent, who took pleasure in literature, music and art. Fluent in German, French, Spanish, Italian and Russian, Ruhräh

Crayon sketch of Dr. Ruhräh examining a baby during a home visit, by Ann Kinsolving.

could appreciate the novels of Turgenev, Schnitzler, and Dreiser, the poetry of Dante and Heine, as well as the humor of La Vie Parisienne. Similarly, in music he could enjoy Mozart, Schubert, Liszt and Wagner as well as Gilbert and Sullivan. Thirdly, early in his career, Ruhräh came to the attention of William Osler, who referred to him as one of the most brilliant of the younger men, and characteristically interested him in the history of medicine and the lives of its practitioners. Osler's influence directed him to books and libraries, and gave him a specific point of view. Not surprisingly, his last major work was a life of Osler.

Through Osler and Welch, he was drawn into the activities of the Johns Hopkins Hospital Club. In this way Ruhräh was brought into favorable environment where his nascent interests could be fostered. But it was also an environment with specific characteristics because the men who put their stamp on it were "men of scholarship, charm, book collecting instincts and the long purse." In a broad sense they were connoisseurs, for whom an appreciation of good living, of food and wine, went hand in hand with an appreciation of books. Ruhräh was a fit member of this company. His "physiology of taste" is reputed to have been as good as his practice of medicine, a characteristic developed upon a wide and appreciative knowledge of French and German wines.[2]

These men, among them William Osler, Henry Barton Jacobs, Howard A. Kelly, W.S. Halsted, Eugene F. Cordell and Harry Friedenwald, were able in a very felicitous manner to combine their interests and predilections as book lovers, collectors of rare volumes, and historians of medicine. As Henry E. Sigerist observed, rarely have there been "so many collectors of old medical books together at one time in our town." Animated by common interests they formed a close-knit group which set the tone for the medical circles of Baltimore around the turn of the century, and provided an atmosphere congenial to a latter-day medical humanism. In this group, John Ruhräh found himself among kindred spirits, and it was in this milieu that his historical studies received direction and form.

Ruhräh's studies in the history of pediatrics began in 1904 with a paper on Robert Whytt, the 18th-century Scottish physician, whose *Observations on the Dropsy in the Brain* (1768), gave the first full clinical description of tuberculous meningitis. Over the following years he collected and read old books on children's diseases, occasionally contributing a historical paper. An example is the study, published in the *Annals of Medical History* for

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[2] Siegfried Weisberger, "Doctor Ruhräh at home," *Bull. Hist. Med.*, 1936, *4:* 221-229 (see pp. 222, 224).

1919, of Walter Harris (1647-1732), an English physician whose small book, *De morbis acutis infantum* (1689), passed through eighteen editions in four languages, the last in 1742, and continued to be quoted for almost a century. Six years later, however, in 1925, Ruhräh published his major contribution to the history of pediatrics, a volume of over 600 pages entitled *Pediatrics of the Past*.[3]

A major motivating influence toward the preparation of this book came from Fielding H. Garrison, the medical historian, while writing a history of pediatrics for the first volume of Abt's *System of Pediatrics* which appeared in 1923. According to Garrison, it was necessary "to give a detailed account of the contents of the individual pediatric texts and treatises up to the time of Billard," that is up to the early 19th century. This was not difficult, but in dealing with the later literature he required consultation. "In endeavoring to ascertain the status of the more recent textbooks and systems, the analysis of which is not the historian's province, but a *question de métier*, I applied to a number of leading pediatrists of the country for an expression of opinion. Among these, Ruhräh was particularly helpful, through his wide reading in the modern authors, from Rilliet and Barthez to Pfaundler and Schlossmann, from Billard to Filatoff and Figueira. Through this pleasant relationship he was led to a deeper study of the older authors and so made the present collection."[4]

Ruhräh acknowledged Garrison's encouragement and emphasized the function of his volume as a supplement to the latter's history of pediatrics. Yet, "it may not be out of place," he wrote, "to try to estimate just what this volume is, and to make clear why the neglected masters of medicine of other times should be sought, not to honor them as some would have it, but for our own profit and good."

> To the average medical student and physician, the literature of the present seems to hold all that is worth while; they give scant heed to the seers who laugh to scorn much that masquerades as new. Medical truth, medical knowledge, medical literature is like the world, a continuous performance! tragedy, drama or comedy, as you will, which each of us visit a little while until we, too, pass on.

From this point of view and out of many conversations and considerable correspondence with Garrison "grew the idea that it would be interesting to collect from the earlier writings an anthology, not of curious or absurd things, but of the real contributions which have helped to make pediatrics what it is today.[5]

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[3] John Ruhräh, *Pediatrics of the Past. An Anthology* (New York: Paul B. Hoeber, 1925).

[4] Fielding H. Garrison, Foreword in Ruhräh, *Pediatrics of the Past*, p. xiv.

The anthology which appeared in 1925 was for Ruhräh a labor of love, an aspect readily apparent to the reader, and represented "the result of a number of years, the spare hours of which were used in reading the works of the early writers, in looking up their lives and in trying to estimate their influence on pediatrics—a delightful task that has been thoroughly enjoyed."[6] Except for a small section of some twenty pages dealing with a few ancient and medieval writers, Ruhräh included works published during the three centuries following the invention of printing, that is, from the pediatric incunabula to the end of the 18th century.

There are several ways of dealing with the history of medicine. The Renaissance established medical historiography and provided it with a basic theme that has remained prominent to the present. The interest of Renaissance authors was focused primarily on the biographies and writings of famous physicians. In considerable measure this bio-bibliographical trend represents the influence of humanism, particularly medical humanism, as well as the interest in individual personality characteristic of the Renaissance. Medical history was thus started on the bio-bibliographical path which it was to follow for centuries. Another approach is to trace the development of knowledge as it relates to particular problems, such as diseases and organ functions, and to examine this evolution within a socio-cultural context. As Ernest Caulfield, to whom I shall turn shortly, put this position, "a medical history of a country should include not only the biographies of its doctors but also of the histories of the diseases which they tried to conquer."[7]

The approach and method employed by Ruhräh are in the older tradition of medical history. Each section consists of an introduction to a particular medical author followed by one or more selections from his work. In a few cases the entire text is presented because of its rarity—for example, the pediatric incunabula of Paolo Bagellardo, Bartholomaeus Metlinger, and Cornelius Roelans—or because of other qualities such as the clinical acumen and common sense of Thomas Phaer in the first English book on the diseases of children in the vernacular, or the profound humaneness and love of children evidenced by Felix Würtz in the first book on infant orthopedics, or the superb, concise clinical delineations of William Heberden. Wherever appropriate, cultural information is provided to enable the reader to see the author and his work in the contemporary context. The result wrote Ruhräh is

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[5] Ruhräh, *Pediatrics of the Past*, pp. xix, xxiv.

[6] *Ibid.*, p. vii.

[7] Ernest Caulfield, "Some common diseases of colonial children," *Trans. Colonial Society of Massachusetts*, April 1942, pp. 4-65 (see p. 4).

"a sort of personally conducted tour through the literature of the sixteenth, seventeenth and eighteenth centuries . . . . Just enough of each author considered is given to whet the appetite and prepare the palate for the pièce de resistance which follows."[8] Not only does the reader visit a strange, almost unknown land, but like many tourists picks up interesting items of information and comment. One learns, for example, that Francis Glisson, the 17th-century English physician who wrote on rickets, is alleged to have "escaped plague by thrusting bits of sponges soaked in vinegar up his nostrils."[9] Daniel Whistler, his contemporary, whose little book of 1645 on rickets was the first on the subject, is described as "a picturesque character of great charm of manner whose dealings were questionable. He attained many high offices and, as Munk says, 'in an evil hour he was elected President of the Royal College in 1683.' He defrauded the College in some way 'but in what precise manner or to what extent is not recorded.' "[10] Equally characteristic is the succinct remark in his note on Samuel Thomas von Soemmering, the German physician and anatomist, whose study on the pernicious effects of corsets appeared in 1788. "Up to this time," says Ruhräh, "his work had scarcely attracted the attention of the scientists, but when he spoke on corsets all Europe listened."[11]

Upon its appearance, *Pediatrics of the Past* was well received and rapidly established itself as a basic work in its field. In 1931, in the preface to his history of pediatrics, G. F. Still described it as "masterly and delightful," and Ernest Caulfield in the same year noted that it had become "the standard source book for students of pediatric history."[12] And it has continued to be used up to the present. Indeed, a recent bibliography on childhood and youth for historians still lists *Pediatrics of the Past* among the sources to be consulted on the professional care of children.[13]

From this base, Ruhräh planned to continue his "journey into the history of pediatrics," using as his Baedeker the bibliography, *Grundlage der Literatur der Pädiatrik*, published in 1850 by Friedrich Ludwig Meissner, a Saxon physician. The fruits of Ruhräh's efforts began to appear in 1928 in the *American Journal of Diseases of Children* as a series of short articles which continued to be a feature of the periodical into 1935, the year of his death. Overwhelmingly, they followed the pattern set in *Pediatrics of the Past*, and in 1932 a collection of these vignettes were published as *Pediatric Biographies*. However, there were also contributions on the history of diseases and various pathological conditions such as appendicitis in children, infantile edema, craniotabes and hypertrophy of the colon. These articles can still be useful to anyone interested in the history of clinical pediatrics, and should not be overlooked as sources of information.

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[8] Ruhräh, *Pediatrics of the Past*, p. xxv.

[9] *Ibid.*, p. 255.

[10] *Ibid.*, p. 258.

[11] *Ibid.*, p. 437.

[12] Ernest Caulfield, *The Infant Welfare Movement in the Eighteenth Century* (New York: Paul B. Hoeber, 1931), p. xii; George Frederic Still, *The History of Paediatrics* (London: Oxford University Press, 1931), p. vii.

[13] C. John Sommerville, "Toward a History of Childhood and Youth," Bibliographic Note b, in T. K. Rabb and R. I. Rotberg (eds.), *The Family in History. Interdisciplinary Essays* (New York: Harper and Row, 1973), p. 232, n. 19.

The *Pediatric Biographies* and the later vignettes were intended as preliminary sketches for a full-scale history of pediatrics, but Ruhräh never achieved this goal. However, he did publish several other works. In 1925, he produced an edition of *A Dissertation on Gout and all Chronic Diseases . . .* (1771) by William Cadogan (1711-1797), physician to the Foundling Hospital of London. Ruhräh's translation from the German of Fujikawa's *History of Japanese Medicine* (in the Clio Medica series) appeared in 1932, and at the time of his death he had completed a biography of Sir William Osler, based to some extent on Harvey Cushing's work, which appeared posthumously. John Ruhräh died on March 10, 1935 at the age of 62, following a cerebral accident. He left an extensive contribution to the history of pediatrics despite the demands of a large practice and the physical consequences of his attack of poliomyelitis.

III

As a historian of pediatrics, Ernest Caulfield differed from John Ruhräh both in temperament and interests. What was common to both was that they were practicing pediatricians, and that both had a deep interest in the history of their branch of medicine. Caulfield was neither a book collector nor a bon vivant. On February 15, 1950, replying to Madeline Stanton who had called his attention to a book for sale in which he might be interested, Caulfield wrote, "It was thoughtful of you to send me the note . . . . Years ago I decided that book-collecting was the quickest route to bankruptcy, and haven't bought anything in years, except a few choice items under $5.00."[14] But he did make an original and lasting contribution to the history of children's diseases which has implications beyond the immediate subject.

Ernest Caulfield was born on September 8, 1893, in Hartford, Conn., and was educated locally.[15] In 1916 he received a B.S. from Trinity College, and in 1921 an M.S. from the same institution. Following graduation, Caulfield entered the Johns Hopkins Medical School, receiving his M.D. in 1920, and

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[14] Letter in biographic file on Ernest Caulfield, Historical Library, Yale Medical School.

[15] Information about Caulfield's career has been obtained from the biographical file mentioned above, from listings in such publications as *American Men of Science*, and from conversations with persons who knew him.

Fig. 3. Ernest Caulfield (1893-1972), about the time he produced the study of the "Throat Distemper."

like so many other medical students he was also a member of the Medical Corps, U.S.A., for the duration of World War I. During 1920-21 the young physician served as an assistant in physiology at his alma mater, but at the end of the year Caulfield left Baltimore and returned to his native grounds.

This action was probably due, at least in part, to the influence of Grover F. Powers (1887-1968) who was chief pediatrician in the outpatient department of the Johns Hopkins Hospital while Caulfield was a student, and who in 1921 came to Yale as assistant professor of pediatrics. This influence seems also to have had a part in Caulfield's choice of pediatrics as a field of practice, a conjecture supported by the fact that in 1921 he obtained an appointment in pediatrics at the Yale Medical School. Moreover, Caulfield remained deeply attached to Powers, dedicating to him the important study on the throat distemper, and expressing this feeling in an eloquent appreciation when Powers died in 1968.

Shortly after his return from Baltimore, Caulfield opened an office in Hartford, where he practised until his retirement. Throughout his professional career he was associated with the Hartford Hospital, where he was appointed assistant pediatrician in 1933, visiting pediatrician in 1939, and eventually chief of pediatrics for the period 1945-48. Concurrently, he maintained his association with the Yale Medical School, where he became lecturer in pediatrics in 1935, and assistant professor of clinical pediatrics in 1938.

Caulfield was a well-trained, knowledgeable, highly competent clinician, who exemplified in a pediatric setting the best qualities of a family physician, but he was no innovator in his specialty. Caulfield's clinical contributions are largely casuistic, and the two papers which Faber and McIntosh record as having been presented at meetings of the American Pediatric Society were historical.[16]

Caulfield reminds one of the poet William Carlos Williams, also a pediatrician. Both remained practitioners and achieved recognition in the local medical community by becoming chief pediatrician in the community hospital, Caulfield in Hartford Hospital and Williams in the Passaic General. And both, despite large professional responsibilities, were able to pursue their non-clinical interests with highly successful consequences.

What stimulated Caulfield's interest in the history of children's diseases is not clear. Perhaps he brought it with him from Baltimore where such interests could easily be acquired? The likelihood of this source of infection is indicated by his first historical paper published in 1928, an analysis of an 18th-century pediatric anthology of 1742.[17] Opening with a quotation from Ruhräh's *Pediatrics of the Past* on the rarity of this book, Caulfield considered the identity of the anonymous compiler and on that basis of circumstantial evidence suggested for this role John Armstrong, physician-poet and brother of George Armstrong, founder of the first dispensary for children (1769). During this period another influence came into play, an influence which directed Caulfield's talents into channels where they could come to full fruition. The stimulus was his observation of the familial incidence of pyloric stenosis on which he published a paper in 1926, and which led him to look into the early history of this condition.[18] As reported by Caulfield several years later, he became "interested in George Armstrong because of his early description of a disease called pyloric stenosis. The fascination of reading Armstrong's book, especially concerning the dispensary which he founded, and an attempt to uncover some records of his obscure but eventful life, led to a study of contemporary medical literature." At this point Caulfield began to develop a new view of the materials which he was studying, recognizing that it was possible through them to understand problems of infant and child health as aspects of social life. He wrote:

> A number of old books, especially when concerned with a definite period, give us a composite mental picture of the people of those times. We are able to learn of their mode of living, their likes and dislikes, their pleasures and their troubles, their sicknesses and the manner of combating them. So from these contemporary sources, supplemented by some modern contributions to eighteenth century medicine and

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[16] Harold K. Faber and Rustin McIntosh, *History of the American Pediatric Society, 1887-1965* (New York: McGraw-Hill, 1966), pp. 190, 224

[17] Ernest Caulfield, "A full view of all the diseases incident to children," *Annals Med. Hist.*, 1928, *10*: 409-416

[18] *Idem*, "Familial incidence of pyloric stenosis," *Amer. J. Dis. Child.*, 1926, *32*: 706-708

[19] *Idem*, *Infant Welfare Movement* (n. 12 above), pp. xi-xii

social life, gradually there became discernible a well-marked and successful infant welfare movement.[19]

The product of this insight and the studies described by Caulfield was *The Infant Welfare Movement in the Eighteenth Century,* initially published as a series of papers in the *Annals of Medical History,* and then issued as a book in 1931. Essentially it represents a transitional stage in Caulfield's development as a medical historian. A major portion of the book is devoted to four individuals and their works: Thomas Coram and the London Foundling Hospital, William Cadogan and his essay on nursing, Jonas Hanway and the infant parish poor, and George Armstrong and the dispensary for poor infants. Each man is presented in a separate chapter, but all are viewed within a large social framework, the movement to improve infant health and welfare, and their activities are analyzed from the perspective of the contribution made toward awakening the public conscience to the value of a child's life.

At the time of publication, this book was a pioneering endeavor. The concept of a social history of medicine was new to both medical and general historians. In the intervening forty-odd years the concept has been accepted, and subsequent studies on infant health and welfare have carried Caulfield's work further and in some degree have supplanted it. Nonetheless, the book is still useful for any student of child health in the 18th century. Based upon patient, accurate research and historical acumen, qualities characteristic of Caulfield as a historian, *The Infant Welfare Movement* is still reliable within its limits. Moreover, it is highly readable, since it is written in a straightforward, muscular prose which has as its aim the presentation of events from which conclusions can be drawn so that the reader will have a clear picture of what actually or probably happened in the past. This style characterized Caulfield's writings and adds to their attractiveness for the reader.

Soon after the publication of *The Infant Welfare Movement,* chance presented Ernest Caulfield with a subject to which he was prepared to apply his talent for historical investigation. Sometime in 1932 (or 1933) Caulfield happened to meet Albert C. Bates, librarian of the Connecticut Historical Society, in the Watkinson Library in Hartford, where he was working on some medical sources.[20] Bates showed him a copy of Jonathan Todd's sermon on the throat distemper preached at East Guilford in 1740. His interest aroused, Caulfield set about investigating the epidemic of this disease which from 1735 to 1740 involved "most of the inhabited regions of New England and caused great loss of life wherever it appeared."[21] In this study which took some six years, Caulfield made use of both medical and non-medical sources. In addition to medical publications, he studied newspapers, church records, religious broadsides, sermons, public proclamations, diaries and family histories as well as the gravestones in churchyards. (To a limited extent he had used non-medical sources for his study of the infant welfare movement.) As a result of painstaking analysis of these sources, Caulfieled traced the epidemic to several independent foci and showed how, over five years, it spread and ravaged New England, killing some 5000 persons, most of them children, in a population of 200,000. Furthermore, applying his knowledge of pediatrics and epidemiology, he concluded that two diseases were actually involved in the epidemic. One with a frightfully high case fatality rate he believed to have been diphtheria, while the other with a lower death rate was scarlet fever. Finally, Caulfield used this study to suggest subjects for further investigation. One was to examine the occurrence of epidemics in connection with the westward movement of the frontier. "If the development of the New England frontier was attended by epidemics of diphtheria," he observed, "it would be interesting to know if the development of other American frontiers was also attended by similar epidemics. I cannot offer very much scientific evidence, but it is probably more than a coincidence that such epidemics did actually occur when the American frontier was extended beyond the Alleghenies." In this connection, Caulfied also called attention to the significance of the demographic factor in the occurrence of epidemics. He noted that the earlier 18th century was "a period of land speculation and the population increases naturally occurred in frontier towns where land was more easily obtained. Perhaps by 1735 this population increase was just enough to upset the balance between immunized and unimmunized subjects and an epidemic was the result."[22] Caulfield returned to this point in a later study, "Some Common Diseases of Colonial Children," where he noted that the appearance of epidemic diseases was not completely fortuitous. "Given a constant birth rate," he wrote, "a steady growth in population density obviously produced a more and more rapid renewal of concentrated groups of nonimmune children, a fact which explains the orderly increase in frequency of epidemics."[23] Lastly one may note his suggestion of a causal connection between the emotional aspects of the epidemic and the profound psychological and religious upheaval known as the Great Awakening that followed soon thereafter.[24] The problems suggested by Caulfield have hardly been touched by medical historians. Epidemic out-

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[20] Lloyd G Stevenson, "The historical writings of Ernest Caulfield," *J. Hist. Med.*, 1965, *20*: 38-42 (see p. 39).

[21] Ernest Caulfield, *A True History of the Terrible Epidemic vulgarly called the Throat Distemper which*

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occurred in His Majesty's New England Colonies between the Years 1735 and 1740 (New Haven: Yale Journal of Biology and Medicine, 1939), p. 1.

[22] *Ibid.*, pp. 112-113

[23] Caulfield, *Some Common Diseases* (n. 7 above), p. 13

[24] *Idem, Throat Distemper* (n. 21 above), p. 2.

breaks in relation to the moving frontier represent an aspect of a larger problem, the relation between the movement of population and the occurrence of disease, and should long since have aroused the interest of American historians. Perhaps Caulfield's challenge will be taken up soon since historians seem to be cultivating similar interests at present.

Unfortunately, the importance and implications of his original study were not realized when it appeared in 1939. First published in installments by the *Yale Journal of Biology and Medicine* in 1938, it was then issued in a limited edition of 375 copies for the Beaumont Medical Club. The slim volume received favorable notices in a few journals, but was not reviewed in the *Bulletin of the History of Medicine,* nor in the *William and Mary Quarterly,* the periodical devoted to early American history and culture. A decade later, however, in April 1949, Carl Bridenbaugh, historian of colonial America, felt impelled to write a belated review for the latter publication "because historical journals have uniformly overlooked it," and because "this remarkable little book . . . is undeniably the ablest study of colonial medical history I have ever come across."[25] Since then other historians working on population change or urban history have begun to recognize the value of his work.

Following the publication of the *Throat Distemper,* Caulfield continued his investigation of the epidemic diseases of colonial America, particularly those of children. Between 1942 and 1958 he published eight papers on such topics. They have been listed together with his earlier publications by Lloyd G. Stevenson in his elegant evaluation of Caulfield as a medical historian.[26] He continued to collect data on colonial children and their diseases, but unfortunately the infirmities of advancing years, particularly visual difficulties, made it impossible for Caulfield to prepare them for publication. Nonetheless, when he died in 1972 at the age of 78 he had made a permanent contribution to the history of medicine and to the methodology by which certain of its aspects could be pursued. He had shown how this could be done by applying curiosity, ingenuity and industry to the study of available sources. As he commented in 1942:

> "Young physicians . . . on first becoming interested in medical history, are usually unaware of the vast amount of American material yet to be explored. They should not be discouraged because American medical books contributed so little to the advancement of medical science; but if truly interested in the history of their profession, they will find that colonial newspapers, sermons, letters, proclamations, and the like, although written by and for laymen, often contain important medical facts. Even casual observations on diseases in the diaries of observant laymen are frequently more instructive than the best of nebulous theories. Every historical library

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[25] Carl Bridenbaugh, *William and Mary Quarterly* (3rd ser.), 1949, *6.* 338

[26] See note 20 above

has at least a few church records, bills of mortality, physicians' letters, or original descriptions of diseases awaiting modern medical interpretation."[27]

Ernest Caulfield was a modest man, not given to blowing his own horn, but in his work he provided an example of how the pediatrician as historian could make an original contribution to the understanding of medical problems and open up new possibilities of investigation in historical epidemiology and the social history of disease for others to explore.

IV

John Ruhrăh and Ernest Caulfield, both pediatricians who contributed to the history of children's diseases, were very different as individuals and in their historical work. Ruhrăh was a tall, massive man, physically imposing, with a gracious manner, whose capacity for genial satire and talent for light versification made him an entertaining, even brilliant conversationalist. A man of methodical habits who enjoyed cultural pursuits, he was a collector who sought out choice examples of pediatric literature which he then presented for the edification of those who had similar interests. Ernest Caulfield, on the other hand, was short and slight of build, imbued with considerable nervous energy and intellectual curiosity. In contrast to Ruhrăh whose cultural interests tended to be international, Caulfield had a deep, abiding interest in colonial America and particularly in New England. Thus, his interest in churchyards was not limited to their contribution to epidemiology, but extended to the stonecutters who made the gravestones. Indeed, Caulfield became an authority on these craftsmen and presented his findings in a series of articles in the Connecticut Historical Society Bulletin (1951-1963). Although he began with conventional contributions to medical history, Caulfield opened up a new path for exploration.

In each case the contexts within which these men moved helped to mold the nature of their work. In Baltimore where the Osler ambience persisted, Ruhrăh, a generation older than Caulfield, produced historical studies of some value but of a conventional nature. As an outsider in Hartford, where his interest in local colonial history could be fostered, Caulfield was able to break out of the conventional mold and to produce creative historical work of a high order. Both pediatricians made significant contributions, but there can be no doubt that Caulfield was more original and ultimately more important for the history of medicine.

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[27] Caulfield, *Some Common Diseases* (n 7 above), p 4

THE PHYSICIAN AS AN EXPERT WITNESS IN ATHENIAN LAW

DARREL W. AMUNDSEN
and
GARY B. FERNGREN

Historians of forensic medicine have consistently maintained that in classical Greece the judicial process did not avail itself of the services of physicians as expert witnesses. Alcantara Machado writes that the jurists of Greece never did perceive the advantages that they could derive from the application of medical knowledge to the judicial process.[1] Douglas Kerr simply states that "amongst the Greeks . . . it does not appear that [physicians] were called to give evidence in courts of law."[2] J. A. Gorsky expresses the same view: ". . . there is no actual evidence that medical knowledge was used forensically in the courts of Cos or Athens."[3] Erwin Ackerknecht is in agreement: "No document exists that provides evidence for the use of medical experts in ancient Greece. . . ."[4] He explains this apparent state of affairs by saying: "Many factors may have contributed to such an attitude, such as the low social status of the medical profession. . . ."[5] Sydney Smith, although he believes that "there is no clear evidence that medical knowledge was officially made use of in establishing proof in courts of law," nevertheless, on the basis of what he thinks was "the high standing enjoyed by the physicians of ancient Greece,"[6] rather than from specific evidence, concludes that "it is hard to believe that [physicians'] opinions were disre-

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[1] "Apesar disso, os juristas da Grecia não perceberam jamais o proveito que poderiam tirar da applição dos conhecimentos medicos á investigação dos factos juridicos. Nas muitas orações judiciarias de Demosthenes, Eschines, Lysias, Antiphon, Isocrates, que chegaram até nós, nenhuma allusão se nos depara á intervenção de peritos no processo ou á influencia das doutrinas biologicas na elaboração das leis " Alcantora Machado, "O exame pericial no direito romano," *Revista de Criminologia e Medicine Legal*, 1928, *1*. 5.

[2] Douglas Kerr, *Forensic Medicine*, 6th ed. (London: Adam and Charles Black, 1956), p 340

[3] J. A. Gorsky, "The history of forensic medicine," *Charing Cross Hospital Gazette*, 1960, *58* 32.

[4] Erwin H. Ackerknecht, "Early history of legal medicine," *Ciba Symposia*, 1950-51, *11:* 1286

[5] *Ibid* , 1286 f.

[6] The contradictory opinions of Ackerknecht and Smith on the social status of physicians are illustrative of a problem that presents itself to anyone who studies the social history of ancient medicine. There are a variety of factors that are not objectively compatible: On the one hand there is the often expressed opinion that the Athenians of the classical period held all "craftsmen" in low esteem (and physicians were, by their own admission, craftsmen) and, on the other hand, there is the question whether such prejudices, often derived from philosophers, are truly reflective of the attitudes of the ordinary Athenians, many of whom supported

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garded by a legal system which was equally anxious to maintain its place of high esteem."[7]

Specialists in Attic law have differed on the question whether physicians were employed as expert witnesses from a mild admission of their limited employment[8] to this enigmatically laconic aside: "Expert evidence by such witnesses as doctors hardly formed a special category."[9]

The purpose of this paper is to present and discuss the evidence from Athenian orators and other sources that bear upon the actual or theoretical use of physicians as expert witnesses.[10] We shall seek to demonstrate: (1) that forensic medicine was used in Athenian courts and other public bodies, however limited according to modern standards; (2) that use of expert medical witnesses was tempered by the essentially private nature of the Athenian judicial system; and (3) that the testimony of physicians in matters of a medical nature was held to deserve special credence.

There was in classical Athens (and in many other Greek cities) what can be termed, without many of its modern connotations, a public medical service. What the functions of the public physicians were and who comprised the public medical service are subject to dispute, but we can say, with reasonable certainty, that many physicians who practised in Athens were

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themselves by their own crafts Edelstein asserts that in Hippocratic antiquity the average physician was a craftsman and thereby occupied a low position in society. Cf "The Hippocratic physician" in *Ancient Medicine Selected Papers of Ludwig Edelstein*, ed. Owsei Temkin and C. Lilian Temkin (Baltimore The Johns Hopkins Press, 1967), p 87. At the same time, in maintaining that the writing *On the art* is the work of a physician, he points to the contention, in that writing, that attacks made against all *technai* must be answered by the experts concerned and, indeed, in the fields that concern them *Ibid.*, pp. 101-102 See also "Greek medicine as science and craft" in Owsei Temkin, *The Double Face of Janus and Other Essays in the History of Medicine* (Baltimore. The Johns Hopkins University Press, 1977), pp. 151-153 Additionally, in the case of the medical profession, there are many venomous attacks on the profession in general or on individual physicians in particular balanced by attestations of the highest respect and admiration. The evidence, which is ambiguous, is open to a variety of interpretations

[7] Sidney Smith, "The history and development of forensic medicine," *British Medical Journal*, 1951, *I* 600. Although quite incidental, Smith's reference to "a legal system . . . anxious to maintain its place of high esteem" presupposes the existence of a legal profession. There was no identifiable legal profession in classical Athens unless the citizenry in the aggregate could be so classified. The orators were not lawyers in any respect approaching the modern sense of the word and there were no professional judges

[8] R. J. Bonner, *Evidence in Athenian Courts* (Chicago University of Chicago Press, 1905), pp. 79 ff., and E. Caillemer, "Anakrisis," in Daremberg and Saglio, *Dictionnaire des Antiquités Grecques et Romaines* (Paris, 1873-1919), vol 1, p 263

[9] A R W. Harrison, *The Law of Athens*, vol. 2. Procedure (Oxford Clarendon Press, 1971), p 134 Cf J H. Lipsius, *Das attiche Recht und Rechtsverfahren* (Leipzig Reisland, 1905), p. 867, n 5.

[10] It is well to point out that forensic medicine, in its larger scope, falls outside the purview of this paper We shall not address ourselves to such questions as the extent of pathologic and toxicologic knowledge available to the Athenian physician or the degree of material in the Hippocratic Corpus that could have lent itself to forensic contributions

not members of the public medical service.[11] Further, there is no evidence to indicate that those physicians retained by the polis had any specified forensic role. Nor does the office of coroner seem to have existed. This rather remarkable deficiency can be credited in great part to the distinctly private nature of Athenian law, by which no one but the party affected could act as plaintiff or prosecutor and the injured party had to bring a private suit. Even homicide, "the crime par excellence in modern thought,"[12] was considered a private offence with the kinsmen of the deceased bearing the responsibility of investigating, indicting, and prosecuting the alleged killer.[13] Only in those cases in which the welfare of the citizens in the aggregate was considered to be jeopardized could a public suit be brought by any citizen, even if not directly affected. Ackerknecht was aware of "the predominantly private character of the prosecution of crime;"[14] however, as we have seen above, he construed this as evidence of the lack of expert witness by physicians in Athenian law courts.

An examination of the writings of the Attic orators (mostly of the fourth century B.C.) reveals, however, that individuals summoned physicians as witnesses (1) to certify illness that would prevent an individual from performing the functions of an office for which he had been chosen; and (2) to give testimony in the law courts regarding injury or death. We shall deal with the latter first.

In the pseudo-Demosthenean speech *Against Evergos*[15] the plaintiff tells of a wet-nurse who died as a result of a severe beating inflicted by Evergos and his brother Theophemos whom he is prosecuting on other charges. It is related that, after the beating, the plaintiff demanded that Theophemos summon a physician to treat the woman. When he failed to do so, the plaintiff summoned his own physician along with witnesses. After the physician had determined that the woman's condition was hopeless, the plaintiff summoned other witnesses and served notice on Evergos and his accomplices to provide for her care. On the sixth day after the assault, the woman died.[16] In the course of the trial the plaintiff introduces depositions to verify these events.[17] Unfortunately, in extant orations from this period, the texts of depositions are not given, there merely being a statement indicating at what point the speeches in the law courts were interrupted for the reading of the depositions.

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[11] For which see L. Cohn-Haft, *The Public Physicians of Ancient Greece* (Northampton, Mass · Department of History of Smith College, 1956)

[12] Harrison, *op cit.* (n 9 above), p 77 Cf. G. M Calhoun, "The early history of crime and criminal law in Greece," *Proceedings of the Classical Association*, 1921, *18* 91.

[13] K. Freeman, "The mystery of the Choreutes," in *Studies in Honour of Gilbert Norwood*, ed M. E White (Toronto University of Toronto Press, 1952), p. 90.

[14] Ackerknecht, *op cit.* (n 4 above), p. 1287.

[15] Critics are virtually unanimous in the view that this speech was not written by Demosthenes Apollodoros has been ,suggested as a possible author by, *e g* , G. Kennedy, *The Art of Persuasion in Greece* (Princeton. Princeton University Press, 1963), pp. 207 and 247.

We have no way of knowing what was contained in the depositions in the speech against Evergos. It is interesting to note, however, the inferences made by classical scholars on the basis of these missing depositions: Lipsius remarks on the absence of an expert medical statement in this case[18] and Harrison writes: " . . . and we look in vain for expert medical evidence [here]."[19] The absence of expert medical testimony in non-extant depositions is not particularly remarkable. Bonner guesses at the content of these depositions and concludes that "laymen as well as a physician are called to testify to the condition of a person who had been wounded."[20] Although the plaintiff may have called the physician to make a statement, we have no way of knowing whether the physician's testimony was included in the depositions (as Bonner maintains) or was not (as Lipsius and Harrison hold). It should be noted that this was not a trial for homicide. Since there were no surviving relatives of the deceased wet-nurse available to prosecute, the killers were immune from an action for homicide. The portion of this speech devoted to a description of the wet-nurse's death was presented merely as an aside in a perjury trial. It was intended to illustrate further the general character of the defendant and his lack of responsibility. The witnesses who are specifically mentioned served to verify that those who inflicted the beating had failed to provide medical assistance and that they had been notified of the woman's hopeless condition. A physician's testimony here would have contributed little to the issues involved in the suit. Accordingly, in actual fact, this case furnishes us no evidence *pro* or *contra* regarding the use of a physician as expert witness in Athenian law courts.

If the testimony of a physician was included in depositions, would it have been given greater credence than that of the layman, *i.e.*, would it have had greater force in a matter of a medical nature? The answer to that question perhaps lies in the speech against Conon written by Demosthenes for Ariston. Ariston brings an action for assault against Conon who, he alleges, had viciously beaten him. He asserts that the extent of his injuries was such that he was carried first home and then to a public bath where he was thoroughly bathed and shown to physicians. Being extremely weak he was afterwards carried to a friend's home located nearby. The depositions of his friends who aided him are read and a separate deposition of a physician is introduced. Ariston then describes his condition: Although the swellings on his face and his bruises did not greatly concern the physician, there ensued continuous attacks of fever with violent and acute pains throughout his body, especially in his sides and stomach, and he was unable to eat. A copious hemorrhage occurred without which, according to the physician, he would have died of internal suppuration. A new deposition of a physician is read followed by depositions of Ariston's friends who had visited him during his illness.[21]

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[16] Pseudo-Demosthenes, *Against Evergos*, 67

[17] Evidence in testimony was introduced in Athenian law courts in the fourth century B.C in the form of written depositions, witnesses did not testify personally. See R. J Bonner and G Smith, *The Administration of Justice from Homer to Aristotle* (Chicago: University of Chicago Press, 1930), vol 1, pp 357 ff

[18] " . . . niemals ärztliche Gutachten verkommen," Lipsius, *op cit* (n 9 above), p. 867, n 5.

[19] Harrison, *op cit* (n 9 above), p 134, n 1

[20] Bonner, *op cit* (n 8 above), p 80, n 2

During the course of the trial depositions made by friends of Conon are introduced asserting that the charges made by Ariston are false. Ariston counters with the argument that those testifying on Conon's behalf are known to be men of low repute. It could be held that the testimony of Ariston's friends carried no more weight than the opposing witness of Conon's friends. In anticipation of this, Ariston says: "But I also bring forward physicians as witnesses (*ἀλλὰ καὶ μάρτυρας ἰατροὺς παρέχομαι*). This gentlemen of the jury, is not the case with them [*sc*. Conon and his witnesses]."[22] The use of the emphatic particle *καί* shows the stress that the speaker placed upon the significance of his having physicians to testify to the veracity of his allegations. Further elaboration is not needed; there is simply the bald statement that Conon has nothing of equal weight to counter this testimony.

The reaction of Lipsius and Harrison to this passage is remarkable. Lipsius maintains that the physician's testimony in this case did not carry the force of expert witness[23] and Harrison follows him, commenting that "no particular emphasis is laid upon the evidence of the surgeon, as distinct from the bystanders, as to the character of the hurt suffered by the plaintiff . . . ."[24] But *pace* Lipsius and Harrison it seems quite apparent that Ariston counted the deposition of the physician as nothing less than the testimony of physician *qua* physician. The special emphasis that he placed upon this testimony is obvious both owing to his choice of words and their context. Ariston appears to have believed that the members of the jury would be receptive to the force of a physician's deposition as professional testimony. Bonner properly understands the significance of this passage: "This is an undoubted instance of expert evidence which no one but a professional man could have given. Its value depended entirely upon the professional skill and experience of the witness. We have here the real beginnings of expert evidence."[25] Bonner, unfortunately, draws a further conclusion on the basis of the content of the non-extant deposition: " . . . the meager deposition of the Athenian witness bears little resemblance to the evidence extracted from an expert witness in a modern court."[26] One must avoid the assumption that, since the physicians of that day fell short of the standards of modern pathologic knowledge, their testimony lacked the force of expert testimony in the eyes of Athenians. The attitude of Lipsius and Harrison seems predicated upon such an opinion and betrays a prejudice that has only to be pointed out to be refuted.[27]

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[21] Demosthenes, *Against Conon*, 9-12

[22] *Ibid* , 36

[23] " . . auf gleiche Linie mit der Aussage anderer, die ihn besucht, gestellt; es fehlt also das Bewusstsein von der Bedeutung des sachverstandigen Urteils " Lipsius, *op. cit* (n 9 above), p 867, n. 5

[24] Harrison, *op cit*. (n. 9 above), p. 134, n 1.

The extent of credence given to the physician *qua* physician by Athenians of the fourth century B.C. is further substantiated by sources other than the orators. Both Plato[28] and Aristotle[29] have a good deal to say about the qualifications expected of physicians. Aristotle maintains that there are three categories of "physicians": the ordinary practitioner, the master of the craft, and the cultivated layman who has studied medical theory as part of his general education.[30] He concedes that, while some individuals appear capable of treating themselves, ". . . they could not do any good to someone else,"[31] and he is quite specific in delineating those areas of knowledge that separate the professional physician from the layman.[32] Plato also assumes that a physician's knowledge is greater than a layman's in medical matters: ". . . the physician's drugs, when prepared with various colors and perfumes, seem different to us, although they are the same. But to the physician, who is considering only their medicinal value, they seem the same and he is not confused by the additions."[33] In the dialogue bearing his name, Phaedrus remarks that a man who might, on the basis of a limited knowledge of medicine, claim to be a physician, "is crazy and, since he had heard something from a book or had stumbled upon some drugs, thought that he was a physician even though he knows nothing about the art."[34] In the *Ion* Socrates asks who is best qualified to judge the accuracy of statements made in conversation about the wholesomeness of various foods. The answer, "the physician," is given without hesitation.[35] Later in the same dialogue Socrates paraphases a passage in the *Iliad*[36] concerning the giving of a posset to a wounded man. He then asks whether it is a matter of the physician's or the rhapsode's art to determine the accuracy of Homer's description and once again the answer, "the physician's," is immediately given.[37]

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[25] Bonner, *op cit* (n. 8 above), p. 80

[26] *Ibid*

[27] There are two additional passages sometimes cited in discussions of physicians as expert witnesses. In Antiphon's third *Tetralogy,* a hypothetical case written for forensic instruction, the defendant in a homicide case contends that the victim died not owing to the blows that had been administered allegedly in self-defence but because of the incompetence of the phsyician under whose care he had been placed. Witnesses are produced to attest the physician's incompetence Bonner (*op cit*. [n. 8 above], p. 80) writes: "Had this been a real case, we should no doubt have seen the evidence of two groups of physicians pitted against each other." Inasmuch as this speech was a didactic exercise, it could hardly be used as evidence either way in the question of the use of phsyicians as expert witnesses

In the pseudo-Demosthenean oration *Against Boeotos (II)* a physician has voluntarily informed the court that Boeotos had approached him asking that he make an incision on his head and that the request was refused. Subsequently Boeotos had had a cut made on his own head and had then charged a personal enemy with assault. Although the witness here is that of a physician *qua* physician, yet it is not a statement of expert opinion, as Caillemer (*op cit* [n. 8 above], p. 263) suggests, but merely of fact

[28] See, *e g* , *Phaedrus*, 268 A ff, *Republic*, 360 E f and 459 C; *Cratylus*, 394 A f; *Charmides*, 156 B ff, *Timaeus*, 88 A.

[29] See, *e g* , *Nicomachean Ethics*, 1102 a, 1137 a, and 1180 b, *Topica*, 101 b; *On Respiration*, 480 b; *Metaphysics*, 981 a; *Magna Moralia*, 1199 a

In the *Statesman* an interesting question is discussed: What would be the consequences of calling an assembly of all the people, or of only the rich in which anyone, whether he was engaged in skilled work or had no special qualifications, should be free to offer an opinion about diseases and how drugs and surgical and medical instruments should be used, with the decision of the majority accepted as fixed procedure for the art? The conclusion is reached that the art of medicine would be virtually destroyed.[38]

In the *Gorgias* there are two discussions that reflect on medical competence being expected of public physicians. Socrates says that it would be ridiculous for men to be so foolish as to attempt to start public practice as physicians without first getting adequate training in the art.[39] Earlier the sophist Gorgias argued that if an orator and a physician were to vie before the ecclesia for appointment as public physician, the orator, the "master of speech," who is able to sway the people, would be chosen over the man of medical competence.[40] These passages from the *Gorgias* indicate that there were standards of medical competence applied in the actual selection of public physicians (although they need not be taken as evidence for the actual mechanisms by which public physicians were chosen). Moreover they quite clearly indicate, as do the other passages cited from Plato and Aristotle, that physicians were held to possess professional knowledge distinct from that of the layman. The second passage cited from the *Gorgias* additionally illustrates awareness that the master of speech or, for that matter, anyone facile of wit, clever with words and blessed with the gift of persuasion, was thought capable of convincing an audience of his proficiency in an area in which he had no real competence if there was no special credence given by the audience to the qualified professional in the area under consideration. Only in the absence of standards of professional competence could the type of sophistry illustrated by Gorgias' example be effective.

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[30] Aristotle, *Politics*, 1282 a.

[31] Aristotle, *Nicomachean Ethics*, 1180 b

[32] References in n. 29, above; especially *Nicomachean Ethics*, 1137 a.

[33] Plato, *Cratylus*, 394 A f. Even the word for drug (pharmakon) is ambiguous in Greek and could be conceived of in three different ways: (1) a drug that is never harmful, (2) a drug that is always harmful or fatal; and (3) a drug that is helpful if used properly or harmful or fatal if used improperly. The confusion was not merely semantic.

[34] Plato, *Phaedrus*, 268 B.

[35] Plato, *Ion*, 531 E

[36] *Iliad*, XI, 639-40.

[37] Plato, *Ion*, 538 C

[38] Plato, *Statesman*, 298 E ff

All the examples given from Plato and Aristotle might well strike the reader as statement of nothing more than common sense. Of course the qualified physician was expected to possess more knowledge of medical matters than would the layman. But why should application of this obvious truth to the Athenian judicial process be denied by modern scholars? The evidence of Plato, Aristotle, and the orators suggests that the layman indeed attributed to physicians a competence in medicine that gave special authority to their professional judgements.

In Plato's last dialogue, the *Laws*, in which Socrates was not a participant, a Cretan, a Spartan and an Athenian discuss the laws that should be made for a new city. In a section dealing with restitution in cases of fraudulent sale, procedures for redress are established for the sale of a slave suffering from phthisis, stone, strangury, epilepsy, or any other ailment, mental or physical, that most men would fail to notice. In the event that the purchaser is a physician or a trainer, he may not gain restitution. Similarly, if the defect was mentioned at the time of sale, there shall be no right of return. If, however, such a slave is sold by a physician or a trainer without advisement of the slave's condition, the buyer shall have six months (twelve in the case of epilepsy) to demand restitution. It is further specified that the case shall be tried before a body of physicians nominated and chosen by both the parties and that this procedure shall be followed even if both seller and buyer are laymen.[41] To what extent Plato's *Laws* reflect the contemporary laws of Athens (or other states) is a much debated question. G. M. Calhoun warns us that "in the *Laws* it is often difficult to determine when Plato is copying existing legal rules and practices and when he is suggesting innovations."[42] G. R. Morrow adds that "outright invention plays almost no part at all [in Plato's *Laws*]. . . . We have seen that his creations are developments of beliefs and practices already in existence."[43] And M. Rostovtzeff even maintains that Plato's *Laws* is a collection and codification of the whole of Greek law of his time.[44] It appears that in this case Plato is citing an Athenian law. In Hyperides' *Against Athenogenes* a law is cited covering agreements between individuals which states that, whenever anyone sells a slave, he must declare in advance any physical disability from which the man suffers. Otherwise the slave in question can be returned to the seller.[45] Plato's law is more detailed than that cited by Hyperides and the latter makes no mention of a body of physicians chosen to arbitrate the matter. But Plato's law is so close to that cited by Hyperides that derivation is the most economical hypothesis. Moreover, it is difficult to imagine that a successful action could be brought agains the seller in this case without the expert testimony of a physician. The application of Hyperides' law may well have depended on a physician's testimony in a case contested on medical grounds which would be difficult to settle on the basis of conflicting witness. It is quite possible that in the *Laws* Plato is making mandatory a procedure that was common but yet depended upon individual initiative in Athenian process. Regardless of whether or not the passage from Plato's *Laws* reflects actual procedures in the matter of expert judgement, it indicates that physicians

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[39] Plato, *Gorgias*, 514 D F This theme is taken up by Xenophon (*Memorabilia*, 4, 2, 5) where the participants in a discussion are reduced to laughter when one of the speakers fancies a candidate for the office of public physician saying that he has never studied medicine, indeed has always avoided even the appearance of studying the art; but he asks to be appointed so that he can learn by experimenting on them.

[40] Plato, *Gorgias*, 456 B f

[41] Plato, *Laws*, 916 A ff. Note that although both physicians and trainers are held accountable for recognizing the presence of an ailment, only physicians are to provide final arbitration

[42] G. M Calhoun, "The materials for a study of Greek law," *California Law Review*, 1924, *12:* 474.

[43] G R. Morrow, *Plato's Cretan City A Historical Interpretation of the Laws* (Princeton: Princeton University Press, 1960), p. 591

[44] M Rostovtzeff, *A History of the Ancient World* (Oxford: Clarendon Press, 1929), vol 1, pp. 337 f.

[45] Hyperides, *Contra Athenogenes*, 15.

were assumed to possess specialized knowledge and that they were held to be better qualified than laymen to render expert decisions in cases of a medical nature.

If a person had been elected to an office by the people in assembly, he could decline to serve for reasons of health. If he was incapable of appearing before the boule, there is evidence that his physician could appear and make affidavit of the illness.[46] Since the affidavit of a physician probably was not specifically required, however, it is reasonable to infer that a person who sent his physician to testify to his illness believed that this professional testimony would carry greater weight than that of a layman.

In Athens any citizen who was disabled, whose property was worth less than three minas and who did not possess adequate means of support, was legally entitled to a pension from the polis.[47] The claims for pensions apparently were reviewed each year before the boule. Among the speeches attributed to Lysias is a speech addressed to the boule by a cripple[48] whose right to receive the pension has been challenged on the grounds that he is not sufficiently disabled, is not poor and is disorderly and arrogant. The speaker enters on crutches and maintains that the challenge was motivated by personal enmity. He briefly describes the difficulties that he endures in maintaining a small shop and to refute the charge that his occasional riding of a borrowed horse belies his disability, he asserts that it is actually due to his difficulty in walking with crutches that he sometimes uses a horse out of necessity. The remaining and lengthier part of his speech is devoted to the matter of his poverty and character and previous service to the polis. Both Lipsius[49] and Harrison[50] find it significant that we have no expert medical testimony introduced here. If our thesis were that expert medical testimony or certification was mandatory in those aspects of Athenian procedures where it is required in most modern processes, our case would be weakened by Lysias 24. But that is not our contention. In most areas of the Western world today, professional medical certification of the extent of disability is required when seeking state finanical aid for the disabled. If that were not the case now, there would be many instances in which medical certification would not be sought by the applicant when, in his opinion, *res ipsa loquitur*. In the case of a man who has lost both his legs, if a medical affidavit were not required, would he feel compelled to seek one? Would the physician likewise, if it were not required, appear before a hearing and say "it is my considered, professional opinion that this man has no legs?" In a hearing on one's right for disability pension in classical Athens a man did not find it necessary (and he might have considered it even ludicrous) to hire the services of a physician to testify to his disability if the physical impairment was manifestly evident.[51] This, however, does not preclude the probability that, in cases where to the layman *res ipsa non loquitur*, where the physical ailment was not readily apparent, the individual found it to his advantage to take the initiative in obtaining expert medical testimony (as we have tried to demonstrate was the case in the law courts and in the certification of illness before the boule) in the belief that greater credence would be given to the expert witness.

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[46] Aeschines had sent his physician to certify his incapacity to serve in an embassy (*On the Embassy*, 94 f) and Demosthenes later had questioned Aeschines' motivations, implying that the illness was feigned (*On the False Legation*, 124 f.)

[47] Aristotle, *Constitution of Athens*, 49, 4

[48] Lysias, 24 On the difficult problem of Lysias' authorship of the speeches attributed to him, see K J Dover, *Lysias and the Corpus Lysiacum* (Berkeley· University of California Press, 1968), and on Lysias 24, see especially p 189

[49] Lipsius, *op. cit* (n 9 above), p. 867, n. 5.

[50] Harrison, *op. cit.* (n. 9 above), p. 334, n 1

In the Athenian law of inheritance it is specified that if one's mind is impaired by disease or the effect of drugs, if he is under the influence of a woman, or senile, or insane, or under constraint, he can be declared incompetent.[52] It can be assumed that here also the individual's initiative applied whether *res ipsa loquitur* or whether it would be advantageous to either contestant to procure the testimony of a physician. In the *Laws* Plato's procedure for determining the sanity of the defendant charged with a violent offence,[53] although it does not require medical testimony, certainly does not preclude its use on an individual initiative. Given Plato's mandatory body of physicians in the determination of physical and mental impairment in suits involving the fraudulent sale of slaves and his frequent comments on the physician's superiority in medical matters, it is likely that he would give greater credence to expert medical testimony in the determination of sanity. It should be noted, however, that the physician *qua* psychiatrist is not frequently encountered in the classical world and the very nature of the layman's (as well as the physician's) attitudes toward the etiology of mental illness, except when the impairment was held to be due to drugs or the result of a physical disease,[54] would have made the physician a less likely candidate for expert testimony in questions of sanity than in cases of a strictly physical nature.

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[51] Although Bonner feels that expert testimony might have proved useful in Lysias 24, he reaches a more reasonable conclusion than do Lipsius and Harrison: "The cripple who appealed to the jury to trust their own eyes as to his physical condition probably found it unnecessary to produce the evidence of a physician" (*op. cit* [n. 8 above], p. 80).

[52] Demosthenes, *Against Stephanos (II)*, 16

[53] Plato, *Laws*, 864, D f.

The two major difficulties that present themselves in investigating the problem to which this paper is addressed are the paucity of detailed source material and the predominant tendency of modern scholars to base their conclusions on comparisons of Athenian private procedure with the much more rigidly defined modern usages. Harrison's assertion that "expert evidence by such witnesses as doctors hardly formed a special category"[55] is correct only to a very limited extent. If the definition of "special category" is contingent upon a legally imposed specification that makes expert medical testimony mandatory in resolving questions of a medical nature, then indeed expert medical testimony did not exist as a special category in classical Athens. But owing to the highly private nature of Athenian law and judicial procedure, in most instances the individual had to take upon himself the initiative of performing on his own behalf, either as plaintiff or defendant, the majority of roles now taken out of his hands by state officials and attorneys. When the use of professional medical testimony was deemed desirable by the individual, it was employed, and its employment was predicated upon the belief, evident both in the orators and in other sources, that in medical matters the testimony of the physician both deserved and was in fact given greater credence than that of the layman.

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[54] See, *e g.*, the comments of a physician in Helidorus' *Ethiopian Romance* "Our art professes to treat diseases of the body, not those directly of the mind, except when the mind is affected along with the suffering body and accordingly is helped when the body is cured" (IV, 7). For a discussion of this passage, see D W Amundsen, "Romanticizing the ancient medical profession: The characterization of the physician in the Graeco-Roman novel," *Bulletin of the History of Medicine*, 1974, *48* 328 ff

[55] Harrison, *op. cit.* (n. 9 above), p 134

RIVAL TRADITIONS: WESTERN MEDICINE AND *YŪNĀN-I ṬIBB* IN THE PUNJAB, 1849-1889*

JOHN C. HUME, JR.

The Punjab is a very large territory located in the northwest of the South Asian sub-continent. The area was the first portion of India reached by invaders and raiders from the West, whether Greeks under Alexander, or Turks under Babar. The Punjab was under the control of a succession of greater or lesser Muslim Turkish kingdoms from the twelfth through the eighteenth centuries. The last of these kingdoms, founded by Babar, was consolidated and expanded by his descendents until it covered most of India as the Mughal Empire. Mughal control of the Punjab continued until 1739. From that date until the end of the eighteenth century the Punjab was gradually brought under the sway of several Sikh groups and was united under the leadership of Maharajah Ranjit Singh between 1799 and 1809. A series of succession struggles occurred after the death of Ranjit Singh in 1839. These struggles were viewed with alarm by the British who wanted no suggestion of political instability in the Punjab. Within six years the Sikhs and British were at war and in March of 1849 the Punjab was annexed by the British-controlled Government of India.[1]

Provincial-level administrators of the Punjab faced, among other things, the task of developing an overall health policy for the newly annexed area. All of these administrators were British after 1849 and most of them were without medical training. One of the most pressing issues these men faced was whether to use such medical manpower resources as were at hand or to insist that any government-sponsored medical system be based entirely on men trained solely in the allopathic system (the name by which European medicine was known in the Punjab throughout the period under discussion). A system based on resources at hand necessarily meant involving men prac-

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[1] *The Oxford History of India*, ed. Percival Spear (Oxford: Oxford Univ. Press, 1958), pp. 232-444; 609-619 Sanjit Singh Bal, *British Policy Towards the Punjab, 1844-49* (Calcutta: New Age Publishers Private & Ltd., 1971).

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ticing in *yūnān-i ṭibb*, or Greek medicine, the medicine indigenous to the area. These men often were not well trained even in their own system. On the other hand, a system based solely on allopathic physicians meant that the extension of the system would be slow and might possibly mean that it would never reach beyond the cities and towns of the Punjab. The choice, clearly recognized at the time, essentially was between *poor* quality medical relief or *no* medical relief for the agricultural classes of the rural Punjab.

The final annexation of the Punjab by the British-controlled Government of India in 1849 had been preceded by a four-year buildup of British and Western educated Indian personnel in the area. Most of these people expected, and in the case of British personnel, could legally demand, the provision of allopathic medical support. In addition, the Punjab Government was determined to provide medical relief not only for its own personnel but also for as many inhabitants of the rural areas as possible. This decision was in line with a general policy of rural uplift intended to secure to government the loyalties of the agricultural classes of the Punjab.[2] To meet these expectations and various needs, the British Resident at Lahore had sought permission from the Government of India to open an allopathic medical school in Lahore. This sanction had been given in 1847 but for some reason the school was later abolished.[3] A second attempt was made in 1853 when a school of anatomy was opened at the Lahore Cantonment. This school was to train Punjabis to the limited qualifications of local native doctor but it was closed because it was deemed "unsuccessful."[4] One result of these abortive efforts to train physicians in the Punjab was to leave the bulk of the subordinate government medical service positions in the hands of the graduates of the allopathic Medical College and School of Calcutta founded in 1835. The vast majority of the graduates of these institutions were Bengalis, not Punjabis.

Unfortunately (from the British perspective) there seemed to be some resistance to allopathic medicine on the part of most Punjabis, be they inhabitants of rural or urban areas. In the eyes of many British officers the lack of success enjoyed by allopathic medicine was due largely to the Bengalis and it was necessary

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[2] Syad Muhammad Latif, *History of the Punjab* (New Delhi: Eurasian Publishing House, 1964), pp. 572-577 P.H M. van den Dungen, *The Punjab Tradition· Influence and Authority in Nineteenth Century India* (London: George Allen and Unwin Ltd., 1972), pp 100-108

[3] Punjab, Punjab Civil Secretariat, *Press Lists of Old Records in the Punjab Civil Secretariat,* Vol IX, *Lahore Agency and Residency* 1847; Book 171, no 80, pp. 364-368; Vol. X, *Resident, Lahore and Chief Commissioner, Cis and Trans-Sutlej Territories,* B-13 May 1848, no. 386; B-5 August 1848, no. 1164, B-19 August 1848, no 1306; B-23 September 1848, 1570; B-2 December 1848, no 2133. These are the major existing sources for the establishment of the school. John Lawrence notes in another proceeding that he was unable to determine what progress the school made and that when a proposal was made to revise the staffing of the school " orders were given to abolish the school." Punjab, *Proceedings in the General Department,* B-24 October 1856, nos. 36-38. (Hereafter cited as *P G.P. General.*).

[4] *P.G.P. General,* B-24 October 1856, nos. 36-38, B-26 December 1857, nos. 3-6.

> . . . to send amongst the people persons of their own kith and kin and to whom they would naturally turn sooner than they can be expected to do, to a set of over-educated foreigners with whom they have no one idea in common, and towards whom they do not even bear the respect which they do the European Character.

The Punjab Government decided, in 1857, that the long-run solution to the problem was to establish once again a medical college in Lahore to provide the right kind of medical subordinates, i.e., to train Punjabis in allopathic medicine.[5]

The Chief Commissioner of the Punjab, John Lawrence, requested the Civil Surgeon of Lahore to draw up a plan for a medical college which could be submitted to the Government of India. The reasons given to justify sanction were: (1) the difficulties in obtaining qualified medical subordinates of all classes in the Punjab; (2) the proven unfitness of "Hindoostanees and Bengalees" whose

> . . . constitution and habits render them unqualified to enter into the feelings and idiosyncrasies of the inhabitants of the Punjab and who regard the service in these Provinces as that of a Foreign country;

and (3) because of Government's desire to improve the mental culture and conditions of the Punjabis.[6] The decision to train Punjabis was still viewed a long-term solution by the Punjab Government.

The decision to start this medical school was based on the premise that the system of medical relief should be centered on fixed dispensaries staffed by allopathic physicians. The dispensaries were located at the administrative headquarters of each district and later at various sub-district headquarters. They stressed out-patient services but most had inpatient beds for seriously ill medical patients and for surgical cases. The headquarters' dispensary was under the direct control of a senior Indian physician. This man had four to five years formal medical training and possessed college or university qualifications equivalent to today's M.B.B.S. in a hierarchy of status. The subordinate professional staff included one or more Indian physicians trained in a vernacular-language medical school with two to three years of formal medical training. They possessed licenses from their school to practice medicine. The whole establishment was under the nominal supervision of the Civil Surgeon, usually a European, who performed a consulting role.

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[5] *P.G.P. General*, B-12 April 1856, nos 100-112. "Punjab Dispensary Report: Mooltan Division. Commissioner's Review " The Medical College of Bengal was preceded by the Calcutta Native Medical Institution founded in 1822 The latter school was founded to train medical assistants who possessed previous medical experience either as dressers or as practitioners of indigenous medicine. The school and the Medical Classes of the Sanscrit College (*vaidak*) and the Madrassa (*yūnān-i ṭibb*) were superseded by the Medical College of Bengal which was "not intended merely to supply the wants of the state, but of the people, and to become a moral engine of great utility and power." The closure of the Native Medical Institution was opposed by its director, John Tytler, who argued that it was important to use local people who knew local customs and language in the medical service and that such people would not go to a European-type college. "Report of the Committee . . on Native Medical Education." *Indian Journal of Medical Science*, 1835, *2*: 99-120; John Tytler, "Native medical education," *India Journal of Medical Science* 1835, *2*: 7-13.

[6] *P.G P General*, B-26 December 1857, nos 3-6, B-2 April 1859, nos 14-17.

The dispensaries were expensive to maintain and, because they stressed a curative approach, reached a relatively small number of people. These defects combined with the decision to replace sub-assistant surgeons and native doctors from Bengal with men born in the Punjab seemed to imply a further slow-down in a process many felt was much too slow already.[7]

Some administrators, both medical and civil, questioned the very utility of dispensaries in the effort to expand medical relief. The dispensaries were of necessity concentrated in the towns, and most of the towns were controlled by practitioners of indigenous medicine. In the opinion of John MacIntire, the Multan Civil Surgeon, government should concentrate on rural areas where the rapid spread of medicine was certain since the people were desperate for any kind of medical aid regardless of the system that aid represented.[8] This could be arranged at first by sending an allopathic Native Doctor on tour with each District Commissioner to treat acute (but curable) out-patient diseases at no charge.[9] As soon as this program proved successful, government could then open branch (out-patient) dispensaries, thus bringing Western medicine to the very door of the patient. Other administrators stressed reasons of economy, loyalty, and the responsibilities of an enlightened government to justify the prompt expansion of medical relief into rural Punjab.[10]

The direction and emphasis of government medical efforts remained a problem throughout the period under discussion. Orthodox medical and civil administrative opinion stressed the extension of dispensary services while a few administrators attempted to implement model programs designed to bring medical services to the rural population. However, there was one major obstacle to the establishment of either type of program: the lack of Punjabis trained in allopathic medicine.

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[7] *P.G P General*, B-3 September 1859, nos. 71-102 "Punjab Dispensary Report: Review of the Chief Commissioner.", B-12 February 1855, nos. 12-17

[8] John MacIntire, M D Glasgow, 1837, M.C R S , 1838 Entered the IMS in July 1838. #B 1297 in Dirom G. Crawford, *Roll of the Indian Medical Service* (London: Thacker & Co , 1930).

[9] *P G P. General, op cit.* (n 5 above).

[10] *P.G.P. General*, B-3 September 1859, nos. 71-102. "Punjab Dispensary Report. Trans Sutlej States, Commissioner's Review." The most forceful exposition of government's view of successful medical relief was made by R H. Davis, Secretary to Government, Punjab, "His Honor will always regard the increase of the attendance at Dispensaries as much a positive proof of the popularity of a Dispensary as the increased number of vaccine operations is of appreciation of Vaccination " *P G P. General*, B-18 May 1861, nos 30-31. "Punjab Dispensary Report: Lt Governor's Review."

There were two possible manpower sources to meet the short-term requirements of the government medical establishment. First, local people might be trained in allopathic medicine by civil surgeons. The primary drawback of this technique was that such people were often of low caste or class and further could only be used at the dispensary under very close supervision.[11] The second alternative was to use *ḥakīms*.

These practitioners of *yūnān-i ṭibb* (Greek Medicine) dominated rational medical practice in the Punjab prior to 1845. The principal medical authority for the *ḥakīms* of *yūnān-i ṭibb* was (and is) Avicenna. Surgery was not practiced by *ḥakīms* but was left to *jurrāhs*, who were often barbers as well, and *suthais*, who performed eye surgery.

The British were aware that a strong indigenous system of medicine existed in the Punjab. *Yūnān-i ṭibb* had dominated the system from the twelfth century. It had received the bulk of medical patronage given by the Islamic rulers of North India. This patronage was court and, therefore, urban centered. It took the form of supporting medical libraries, medical schools, hospitals, and prominent physicians. The tradition of court-sponsored medicine was continued into the nineteenth century, and reports of early British observers make it clear that the cities possessed "sophisticated" medical operations and possessed large numbers of men trained in medicine. Rural areas were touched occasionally by government medical efforts such as those of Sher Shah, Sultan of Delhi during the sixteenth century, who attempted to maintain a physician in all overnight way stations on the caravan routes under his control.[12]

Many British officials, medical and civil, favored the use of *ḥakīms* as a means of extending the medical system into rural areas. The *ḥakīms* solved various social problems in that they were Punjabis. They came from respectable families in and of the community, they were careful of their behavior and "above all their systems of practice are carefully adopted to the prejudices and practices of their patients."[13]

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[11] Diron G Crawford, *A History of the Indian Medical Service 1600-1913* (London W Thacker & Co., 1914), vol. 2, pp. 100-102. *P.G.P. General*, B-10 May 1856, nos 58-67. "Punjab Dispensary Report: Peshawar Division, Murdan Dispensary Report."

[12] The principal sources for this short overview of *yūnān-i ṭibb* are: E G. Browne, *Arabian Medicine* (London, New York, Ibaden. Cambridge Univ. Press, 1962); Cyril Elgood, *A Medical History of Persia and the Eastern Caliphate from the Earliest Time Until the Year A.D. 1932* (London, New York: Cambridge Univ. Press, 1951); R.L. Verma, "The growth of greco-arabian medicine in medieval India." *Indian Journal of the History of Science*, November 1970, *5*: 347-363, Muhammad Zubayr Siddiqi, *Studies in Arabic and Persian Medical Literature* (Calcutta: Calcutta Univ., 1959); I H. Qureshi, *The Administration of the Sultanate of*

There was precedence for the employment of *ḥakīms* by the local governments. Lawrence had suggested and authorized the training and the use of *ḥakīms* as vaccinators in 1857 and earlier approved their use in times of exceptional need.[14] Thus, the Kangra District Dispensary Committee, when confronted with a cholera epidemic in 1856, employed thirteen *ḥakīms* to distribute medicine and treat patients. These men were instructed in the proper use of the medicine by the Civil Surgeon. The Superintending Surgeon of Kangra was impressed by the efforts made by these men; however, he was "not prepared to say what amount of good the hakeems did towards curing the disease but they certainly did much toward giving confidence to the people."[15]

Early emergency programs like that of Kangra District were attended with enough success to prompt Punjab Government sanction to attempt small extender-type programs on an experimental basis. The most ambitious of these early programs was in Gujrat District and was started in 1862. Twenty *ḥakīms* were hired to assist in the treatment of disease. The Civil Surgeon, J.W. Johnstone, wrote a short medical guide to the diagnosis and treatment

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Delhi, 4th ed. (Karachi: Pakistan Historical Society, 1958); William C B. Eatwell, "Memorandum on Mohammedan Medicine in India, being answers by Hukeem Mohammed Mirza, of Delhee, to certain questions on this subject," *Indian Annals of Medical Science* 1860-61, *7*. 106-111. Eatwell provides an interesting contemporary account of the history of *ṭibb*; John Martin Honigberger, *Thirty-Five Years in the East Adventures, Discoveries, Experiments, and Historical Sketches, Relating to the Punjab and Cashmere in Connection with Medicine, Botany, Pharmacy, & c* (London: H. Bailliere, 1852, reprinted in Calcutta, 1905), *passim* Honigberger is a principal informant on medicine as practiced in the Punjab immediately prior to annexation; Percival Lord, M B , "Notes on the state of medical and surgical science in the countries bordering on the Indus," *Transactions of the Medical and Physical Society of Bombay*, Bombay 1838, *1* 276-295. Lord gives an account of *ṭibb* as practiced in the South and West Punjab, *P G P General, op. cit* (n. 5 above), gives the views of several British observers on *ṭibb*; Ayurvedic medicine apparently was less extensively practiced in the Punjab than *yūnān-i tibb* until at least the last quarter of the nineteenth century Contemporary reports stress the dominance of *yūnān-i tibb* during the 1840s and 50s The lack of *vaidak* practitioners was the subject of an editorial in an Indian owned and managed newspaper as late as 1883. The editor regretted that ayurvedic medicine was "so little known and practiced in the Punjab where the Yunani system seems to have supplanted it altogether " *Tribune*, 10 November 1883, p. 7.

[13] *P.G.P. General, op. cit.* (n 5 above).

[14] *P G.P General*, B-16 May 1857 nos 7-49 "Punjab Dispensary Report: Chief Commissioner's Review: Trans Sutlej States, Kangra Dispensary Report."

[15] *P G.P General, op. cit.* (n 14 above). Superintending Surgeon of Kangra Dispensary was probably John Williams, M.R.C.S., F.R C S 1859 Crawford, *Roll*, #B1519

of the most common ailments which was translated and given to the *ḥakīms*. The *ḥakīms* then were given medicines and sent into the district.[16] A year later the Punjab Inspector-General of Prisons and Dispensaries, A.M. Dallas, added that the employment of *ḥakīms*, if supervised with care by the medical and civil administrators, could be a successful and valuable measure as the *ḥakīms* could reach more people than could government medical officials. Further, Dallas believed that over time the *ḥakīms* would come to recognize the superiority of allopathic medicine to *yūnān-i ṭibb*.[17]

It seemed clear that a system of medical relief based on *ḥakīms* placed throughout the district provided a reasonable alternative for those charged with finding some way to improve the health of the rural public. *Ḥakīms* were socially acceptable to the people and they knew the minds of their patients. Further, it was assumed that the *ḥakīms* could not help but be impressed by the superiority of allopathic drugs and techniques and that this would aid in the spread of allopathic medicine generally. Finally, there were large numbers of trained or partially trained *hakīms* available for immediate use. These reasons helped convince the Punjab Government to sanction in 1867 a program proposed by the District Commissioner of Sialkot, Lieutenant Colonel T.W. Mercer, to establish a district-wide scheme for medical relief dependent for its manpower needs on district *ḥakīms*.

Mercer's program became the prototype for future Punjab rural medical efforts using *ḥakīms*. The program was called variously the Sialkot program, the Mercer program, or, most often, the *ḥakīm* system.

Mercer wanted to provide some form of immediate medical relief to the agricultural classes of his district who were outside the areas serviced by the government dispensaries. He wished to extend the government medical system and to do this he had to use such medical men as were present in the district capable of working without direct supervision. Mercer admitted that he did not know how much medicine the *ḥakīms* knew. Indeed, he could not understand how they received any formal training since

> pharmaceutical works were not to be found amongst them, except here and there amongst Huqeems of wide spread reputation dwelling in the large towns, and such works were in manuscript and rendered unintelligible to less educated Huqeems by the introduction of many Arabic words.

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[16] *P G.P. General,* B-4 June 1864, nos. 32-38. "Punjab Dispensary Report 1863, Employment of native Hakeems." John Wilson Johnstone, L.R.C S., Edinburgh, 1858. M.D. Edinburgh, 1859 Later assisted in the Sialkot program while Civil Surgeon of Ludhiana. Crawford. *Roll* #B 1857.

[17] *P.G.P. General,* B-22 July 1865, nos. 27-30 "Punjab Dispensary Report. Employment of Native Hakeems " Alexander M Dallas, M.R C.S., 1855. Trained at Guy's. Dallas was a senior civil medical administrator in the Punjab from 1863 to 1889. Crawford. *Roll* # 1722.

Further, Mercer conceded that for the most part *ḥakīms* practicing in rural areas were "profoundly ignorant, superstitious and unscrupulous, and limited in their services to those who were most easily imposed and better able to pay extortinate fees." Even with these problems, Mercer believed that some of the men could be used to extend medical relief if they were handled properly. He noted that when stationed at Amritsar, he had attempted to bring the various medical groups of the town into professional communication with the government medical officials. This approach had worked so well that it occurred to him that it might be possible to devise a scheme "for drawing out of obscurity the nucleus of a medical profession, and gradually graft upon it the system which science and the experience of ages have now made universal in civilized countries."[18]

The *ḥakīms* employed in Mercer's program were to treat diseases, distribute simple medicines, primarily quinine, to act as sanitary inspectors, as registrars of vital statistics, and to aid in the provincial vaccination program. Each *ḥakīm* was placed in charge of a circle which consisted of thirty-seven villages. A central village was selected as his place of residence. All circles in a sub-division; or *tehsil*, were placed under a graduate of the government vernacular language program at the allopathic medical school in Lahore. This man was titled Huqeem Ali (chief *ḥakīm*) in order to identify him with the system and all *ḥakīms* in the *tehsil* were placed under his orders. Mercer put the whole establishment under the supervision of a sub-assistant surgeon who was given an allowance of Rs 50 per month from the *ḥakīm* fund in addition to his pay.[19]

Mercer was pleased, even astounded, by the success and popularity of the program. He believed its popularity was due to the involvement of local people in all areas of the program. The initial selection of *ḥakīms* was up to the people. Their nominees were brought to the district headquarters where Mercer examined them. Mercer used five tests: popularity; acquired skill; physical ability to perform in a position requiring extensive traveling; character; and finally, the ability to pass a simple examination in the names of diseases and the use of drugs.

Mercer was convinced that he found the best available men for the job although he admitted that "there is a vast amount of ignorance amongst them." The men were supervised closely for some time and those found unfit for any reason were removed.[20] Mercer started a training program designed to fill future allopathic medical requirements using boys selected from the district. Money from the *ḥakīm* fund was set aside to train those sons of *ḥakīms* who showed desire and ability. It was Mercer's view that these men would form the nucleus of a well-educated medical profession in the district which, building on the confidence and good will of the people of the district, would extend medical relief throughout the area.[21]

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[18] Punjab *Gazette* XIV (25 February 1869) 3rd Supplement, paragraphs 5-7.

[19] *Ibid.*, paragraphs 9-14.

[20] *Ibid.*, paragraph 19.

Mercer had good reason to believe that the *ḥakīms*, at least, would take advantage of the educational opportunities offered by his system. Formal instruction of *ḥakīms* and their families in allopathic medicine was first tried in 1863 when a class composed of the sons of local *ḥakīms* was started at Amballa Dispensary. The boys who showed promise were encouraged to go on to the Lahore Medical School and further allopathic training. The program succeeded in attracting such a large number of students that it was necessary to establish a waiting list.[22]

The success of the Sialkot experiment apparently prompted the Lahore Medical School in 1870 to add a class for the relatives of *ḥakīms*. The class was trained in allopathic techniques particularly in anatomy and surgery in which *yūnān-i ṭibb* was considered to be very weak. The Principal of the school, T.E.B. Brown, reported that ten scholarships had been provided for three years, thirty scholarships in all. Six of these came from the Punjab Government while eighteen were provided by district governments and two by an Indian Princely State. The program was very successful and resulted in a Government of India award to the superintendent of the program, Sub-Assistant Surgeon Rahim Khan.[23]

A proposal to lend more government support to the education of *ḥakīms* was approved by the Lieutenant Governor and sent to the Punjab University College Senate for approval. The report was the work of a Committee set up by the Director of Public Instruction to plan for the types of degrees to be given by the various faculties. The Committee proposed to give training in the "English system" and to upgrade the "Titles in Oriental Medicine" by requiring candidates for such titles to possess knowledge of Western medicine in addition to *yūnān-i ṭibb* or ayurvedic medicine. The English system would be taught at the Lahore Medical School by means of lectures and practical work. For *yūnān-i ṭibb* no formal teaching institution was to be established, rather the candidate was to present a certificate from a recognized authority indicating his competence in *yūnān-i ṭibb* or *vaidak* medicine. The English degree of Licenciate of Medicine and Surgery would require no knowledge of either *yūnān-i ṭibb* or *vaidak* medicine. However, passing an examination in the principles and practice of medicine including that of *yūnān-i ṭibb* would be required by all M.B. candidates. The M.D. candidates would, in addition to these other requirements, write an original essay in English or *urdū*.[24]

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[21] *P.G P. General,* A-36 January 1869, paragraphs 24-26.

[22] *P G.P. General,* B-14 June 1864, nos 32-38. "Punjab Dispensary Report: Umballa Dispensary."

[23] Punjab, *Proceedings in the Home Department* (General Branch) A-30 May 1872 (Hereafter cited as *P G P Home General*)

Two years later the Senate approved a plan to allow Oriental College to grant certificates and titles to *ḥakīms* and *vaids* as well as classes to train these men in the dual program.[25] The first men to enroll in the class were practicing *ḥakīms*. The primary training was in the "English System" under the tutelage of Rahim Khan, superintendent of the vernacular language division of the Lahore Medical School. By 1876, five men had passed the *ḥakīm-i-ḥāẓiq* (Licentiate equivalent) examination and were studying for the title of *umdat-ul-ḥakamā* (M.B. equivalent); another nineteen were enrolled in the *ḥakīm-i-ḥāẓiq* program.[26] A formal Department of Oriental Medicine was established at Oriental College.[27] By 1884, the department had an enrollment of forty-three active students and an additional sixteen students in private tutorials. The Principal of Oriental College noted that one serious problem existed for students of *vaidak* medicine who wanted to study allopathic medicine at the allopathic medical school since classes there were conducted in *urdū*, a language of which most of the *vaidak* students were ignorant.[28]

The Oriental College program in *yūnān-i ṭibb* and *vaidak* medicine had a more serious problem that that of language: the active opposition of most members of the allopathic medical establishment. Some opposition had always been present to the various Oriental medical programs and in 1882 the opposition came together on the issue of medical registration. The Government of India forwarded to the Punjab Government for comment a bill proposed by the Bombay Government which was designed to register medical practitioners of the City of Bombay. It was the opinion of the Government of India that any medical registration bill passed should apply on an all India basis. The bill would not forbid irregular practice but would have created registered practitioners on the model provided by the United Kingdom. Certain exclusive rights would be conferred on the registered practitioner, i.e., the right to sue for fees, the right to sign government certificates, and the right to call oneself a registered practitioner. Normally such a bill would be expected to have the support of the allopathic medical profession. But in the Punjab such support was not forthcoming because of the belief on the part of many that the bill as proposed would give *ḥakīms* and ayurvedic practitioners who possessed university qualifications the same legal rights as the allopathic physicians.[29]

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[24] *P G P Home General*, A-7 March 1872

[25] *P G P Home General*, A-5, 1874

[26] *P.G.P Home General*, A-11 July 1876, paragraph 40

[27] *P G.P. Home General*, A-2 June 1877.

[28] Punjab, *Proceedings in the Home Department* (Education Branch), A-1 January 1884

Punjab Government consulted various "native literary and scientific societies" regarding the proposed bill. Five of these (the *Anjuman-i Punjab,* the *Anjuman-i Kasur,* the *Anjuman-i Peshawar,* the *Anjuman-i Islamia,* and the Delhi Literary Society) supported the general provisions of the bill while the sixth (the *Guru Singh Sabha*) supported the idea of medical registration.[30]

The *Anjuman-i Punjab,* the most prestigious of the societies in the Punjab, initially split over the course of action to take. Founded originally to develop an Oriental university in the Punjab and to aid in the diffusion of useful knowledge through vernacular languages, the *Anjuman-i Punjab* was closely associated with the *Anjuman-i-Kasur* and the *Anjuman-i Peshawar,* both of which had originated as its out-station branches. The *Anjuman-i-Punjab* was not, strictly speaking, a "native" society as its membership represented the various groups in the Punjab: European and Indian: Hindu, Muslim, Christian, and Sikh, allopathic and indigenous practitioners. Initially, lay members and allopathic practitioners believed that the Punjab as a whole was not ready for medical registration but did support extension of registration to municipalities. Indigenous practitioners within the *Anjuman* supported the enactment of medical registration and its extension to all areas of the Punjab as they believed it would help do away with quacks.[31]

While the various associations might be convinced that the bill as proposed would cover all systems of medical practice, many government officers felt that this was not the intent of the originators of the idea. The bill, as written, appeared to cover any physician who possessed any qualifications registerable with the General Medical Council in Great Britain or who possessed an Indian University medical qualification. These included the M.B. degree, the License in Medicine and Surgery from the University, and the Titles in Oriental Medicine of the Punjab University. The bill did not include those allopathic physicians whose only qualification was a License or Certificate from one of the government medical schools.[32]

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[29] Punjab, *Proceedings in the Home Department* (Medical and Sanitary Branch), A-2 March 1883. (Hereafter cited as *P.G.P. Home M & S.*)

[30] *Ibid.*

[31] *P.G P Home M&S* , B July 1883, no. 41 "Report on the Meeting of the Anjuman-I-Punjab, 1 February 1883."

That it was not clear who might be covered by the medical act is shown by the confusion of medical and civil officers outside Lahore who voiced approval of the bill, some because they assumed it excluded indigenous practitioners while others believed it was a good idea because it would distinguish qualified *ḥakīms* from the many quacks and thus lead to the gradual improvement of medical relief in the Punjab. The idea of registration did not, however, win the approval of the Medical Faculty of the Punjab University nor the approval of the civil or medical members of the Punjab Government's administrative headquarters in Lahore.

The Punjab Government notified the Government of India that it could not support extension of medical registration to the Punjab and, while it noted the general approval of members of the native societies which were consulted on the subject, the Lieutenant Governor was not willing to accept those opinions "as conclusive index of native opinion generally."[33] The defeat of the bill in many ways reflected a growing hostility on the part of the Punjab Government to the *ḥakīms* and the system they practiced. This hostility was in no small part due to the successful efforts by some allopathic practitioners, both Indian and European, to discredit the use of *ḥakīms* in the government medical service.

There were two levels upon which allopathic physicians opposed *ṭibb*. On the theoretical level, allopathic practitioners sincerely believed Western medicine was superior, i.e., more scientific than *ṭibb*. Beyond this was the professional level, and many allopathic physicians objected to programs such as Mercer's since they seemed to encourage the use of men who possessed the barest knowledge of any medical system, giving them in addition a hodgepodge exposure to Western medicine without regard to standards of training or qualifications, and then turning them loose on the patients. Some allopathic physicians wanted a complete ban of *ḥakīms*. Others, like A.M. Dallas, believed that it was futile to ban *ḥakīm* practice but felt that government should not give the slightest hint of approval of *yūnān-i ṭibb*. These men believed that such approval was explicit in government-sanctioned educational programs which awarded Titles in Oriental Medicine and implicit in government medical relief efforts which employed *ḥakīms* in professional roles.

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[32] *P G.P Home M&S* , *op cit* (n 29 above).

[33] *Ibid.*

The early opposition to the *ḥakīm* programs primarily consisted of efforts by many allopathic physicians to avoid any contact with the program. Mercer, for example, had to entrust supervision of the program to Indian allopathic physicians, and to turn to John Johnstone, the Civil Surgeon of Gujrat District, *not* his own Sialkot District, when he needed advice. Even the medical department at Lahore was not overly co-operative, for when Mercer requested that he be supplied with two native doctors to work in Sialkot until the district could provide its own men, the medical department replied that it was not in a position to obtain native doctors for him and that in any case they understood the strength of his program rested in its volunteer nature.[34]

Still the success enjoyed by the *ḥakīm* programs and the support those programs received from many leaders of government could not be ignored. Accordingly, allopathic practitioners had to develop additional tactics to combat the further intrusion of *ḥakīms* into the medical system. The primary technique of combat was the use of pages of the *Punjab Dispensary Report* as a vehicle to point out deficiencies in the *ḥakīm* programs or to neutralize praise of these programs through appropriate editorial comments. The report was an annual document written under the supervision of the Inspector-General of Prisons and Dispensaries, who from 1860 through 1888, except for a four-year period, was Alexander M. Dallas. The reports were sent by Dallas to his superior, the Lieutenant Governor of the Punjab, then to the Governor-General of India, and finally to London and the Secretary of State for India. No such method of communication was open to proponents of the *ḥakīm* plan. All communication regarding strictly medical matters had to be routed through the Inspector-General's office and he decided what information to forward. Nor could the Punjab Government object to remarks Dallas made in the report so long as they were not grossly insubordinate.[35]

Dallas himself was ambivalent towards the *ḥakīm* program prior to 1868.[36]

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[34] Punjab *Gazette*, *op. cit* (n. 18 above), paragraph 21 *P.G.P General*, A-3 June 1870.

[35] The dispensary reports from 1853 to 1861, intermittently thereafter, are available in manuscript at the Punjab Archives under the title *Punjab Dispensary Reports*. These manuscripts contain the original local reports with comments at the various supervisory levels The reports after 1860 were printed and bound. They appear under a variety of titles (See Cyril P.K. Fazal, *A Guide to Punjab Government Reports and Statistics* (Lahore: Board of Economic Inquiry, Punjab 1939), for a complete publishing history.)

[36] See *P G P. General*, B-4 June 1864, nos. 32-38 "Punjab Dispensary Report 1863: Employment of Native Hakeems." *P.G.P General*, *op. cit*. (n 17 above)

In that year's *Dispensary Report* he noted that the system existed in its full form in seven districts and that in all but one district it was doing well and receiving the support of the district officers. He remarked further that the *ḥakīm* system existed in modified form in many other districts. However, Dallas added that, though the system was still in the experimental stage, he was not satisfied with it as he felt that there was a serious danger that people in the district would come to expect gratuitous advice, care, and medicine.[37] A year later Dallas added that he opposed turning loose on patients men sponsored by government who were poorly trained even in their own system. He stated that even those few *ḥakīms* who were adequately trained should not be allowed to practice away from the dispensary although he had to be aware that this would defeat the whole purpose of the program, which was to provide medical relief for those not reached by the dispensaries.[38] Prior to 1880 Dallas had to admit that the majority of reports on the system were favorable. After that time Dallas stepped up his criticisms and tended to omit favorable comments or reports (if there were any) from the *Dispensary Reports*. He noted in 1880 that there were reasons to believe that in many places the *ḥakīms* were merely "the personal attendants of influencial [Indian] members of the Committees by which they were paid, and that their work among the poor for whom they were supposed to be paid amounts to little or nothing."[39]

The mid-1880s brought many communications from allopathic medical practitioners critical of *ḥakīms*. These Dallas forwarded to his superiors. The letters complained about the behavior of *ḥakīms*, about their unscientific methods, and about their general untrustworthiness.[40] Finally, in the *Dispensary Report* of 1886, Dallas quoted critical remarks of four allopathic physicians, two European and two Indian, all of whom were long standing critics of the use of *ḥakīms*. The four men condemmed the various programs as inefficient and unscientific. Dallas suggested forcefully that government should take some action on these complaints.[41] In response to the constant barrage of complaints, particularly those of the 1886 *Dispensary Report*, the Government of India suggested that the Punjab Government discontinue the employment of *ḥakīms*.[42]

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[37] Punjab, *Report of the Inspector-General of Dispensaries for 1868*. Lahore, 1869. (Hereafter cited as *P D.R*)

[38] *P.D.R 1869*, p 7; also *P D R. 1871*, p. 5, *P.D R 1874*, p 6, *P D R 1876*, p 3.

[39] *P D R 1880*, p. 8. There is no direct evidence available to support Dallas' charge.

[40] *P G P. Home M&S* , A 1-2 December 1885, B-32-33 August 1886, A 18-19 July 1887, A 1-9 December 1887, A 6-45 March 1889.

[41] *P D.R. 1886*, p. 14 The officers were the Civil Surgeon of Amballa, Surgeon George Thomson, M.B. C.M., Abingdon, 1864, Crawford *Roll.* #B1884, the officiating Civil Surgeon of Ferozepore, Surgeon Donald D.S.J.D Grant, B.A. M B B.Ch., Trinity College, Dublin, 1881, Crawford *Roll.* #B2170; the officiating Civil Surgeon of Jhang, Assistant Surgeon Chetan Shah, the officiating Civil Surgeon of Kangra, Assistant

The initial reply of the Punjab Government in 1869 to critics of the *ḥakīm* system stated the predicament of the lay observer by pointing out that

> the most opposite of opinions have been expressed to its [*ḥakīm* system's] utility; by some, the benefits are said to be "immense" while others declare it to be "worse" than useless; the fact probably being . . . that its success or failure mainly depends on the amount of encouragement and co-operation it receives from the District Officer and Civil Surgeon . . . so far as the Lt. Governor can judge the establishment of Hakim arrangements has in most cases led to a large increase of subscriptions from the native community, and insofar as it has been voluntary, this forms an additional recommendation of the measure.[43]

The Punjab Government's standard reply to remarks critical of the use of *ḥakīms* was to state that government only planned to use *ḥakīms* so long as there was a shortage of trained allopathic physicians.[44] The last attempt by the Punjab Government to defend, publicly and officially, its experiments in the use of *ḥakīms* was in 1876 when it argued that though the Oriental system of medicine was in many ways inferior to that of the West, still medical science was empirical and there was no doubt that *yūnān-i ṭibb* did some good as "there is no reason to doubt that the native treatment of many diseases is to some extent effective and highly appreciated by patients."[45] The next pronouncement of the Punjab Government on *yūnān-i ṭibb* was somewhat less enthusiastic as it requested the University Senate to consider the question of whether Oriental College should "teach those systems of medicine which Government can in no way approve, and which are antagonistic to the principles and teachings of its own medical institutions."[46] No action was taken by the Senate.

Surgeon Radha Kishen. Chetan Shah and Radha Kishen were members of the first full graduating class of the Lahore Medical School in 1866. Chetan Shah was the first Assistant Surgeon in the Punjab to be given a reserved Civil Surgeoncy. Radha Kishen served as superintendent of *ḥakīms* in Shahpur District from 1867 to 1871. Chetan Shah and Radha Kishen had very successful government careers and both were awarded honorary titles Chetan Shah was made Khan Bahadur in 1877 and Honorary Assistant Surgeon to the Viceroy in 1881 Radha Kishen was made Rai Bahadur upon his retirement in 1895. Both men were Hindu Arora of Shahpur District and might be expected to oppose *ṭibb* as representing Muslim interests as well as being "antiscientific."

[42] *P G P. Home M&S* , A-6-9 August 1887. "Review of the Government of India on the Punjab Dispensary Report of 1886 "

[43] *P.G.P Home General*, A-4 October 1869.

[44] *P.G P Home General*, B-8 August 1868, nos. 22-28.

[45] *P G P Home General*, A-5 November 1876.

[46] *P G P. Home General*, A-10 July 1879

During the 1880s senior civil administrators in the Punjab Government reassessed their position regarding the use of *ḥakīms* and needed little encouragement to discontinue their use. The change in attitude represented the coming together of several factors: first C.U. Aitchison, an opponent of the Oriental configuration of Punjab University, became Lieutenant Governor of the Punjab; second was the discovery by government that the medical school was producing more graduates than the government could afford to employ on its own.[47] These factors combined with the criticisms by members of its own medical establishment, and finally with criticisms from above, prompted government to end support to *ḥakīm* programs.

In 1887, Aitchison suggested government withdraw support from any program in the university which taught subjects antagonistic to reason and science and specifically named *yūnān-i ṭibb* as such a program.[48] In response to the Government of India's suggestion that the Punjab Government end its employment of *ḥakīms*, James Lyall, the new Lieutenant Governor, stated that the employment of *ḥakīms* had generally been discontinued but that to enact a law prohibiting their employment would be to intrude on the rights of local boards and would "strain the meaning of the law as it now stands."[49] Instead Lyall drew up new rules concerning the employment of *ḥakīms* and ayurvedic practitioners which were so restrictive as to make it nearly impossible for any local boards to employ indigenous practitioners, thus effectively ending the *ḥakīm* experiments.[50]

The controversy over the use of *ḥakīms* in government-sponsored medical programs arose as a difference of opinion about proposals to extend medical relief into rural Punjab, i.e., whether some amount of poor quality relief was preferable to no medical relief at all. The decision by the Punjab Government to allow local officials to attempt development of medical relief systems using the personnel at hand was a compromise which reflected government's ambivalence toward the problem. On the one hand, it did not want to make radical changes on the local level; on the other hand government wanted to improve the quality of life of its subjects. The *ḥakīm* system

[47] *P.G.P. Home M&S.*, A7-8 August 1888

[48] *P G P Home M&S* , B-63-63 March 1887. *Convocation Addresses of the Universities of Allahabad and Punjab.* ed. K.B. Thapar (Lahore: Mushi Gulab Singh and Sons, 1895) Sixth Convocation Address of the Punjab University, 10 March 1881, p 50

[49] *P.G.P Home M&S* , *op cit.* (n 42 above).

[50] *P.G.P Home M&S.*, A 6-45 1889. "Draft Rules for Appointment of Hakims and Baids by Boards and Committees." In fairness to Dallas it must be emphasized that he opposed any outright ban of *ḥakīms* or *vaidak* practitioners as suggested by Government of India He did not want anyone employed in a government sanctioned program who was not a graduate of an approved allopathic school or college meeting certain basic standards

seemed to provide a reasonable solution; it was inexpensive, used local people, and provided previously untouched rural areas with a degree of medical relief. When encouraged and closely supervised the program worked reasonably well, perhaps too well. The willingness of *ḥakīms* to participate in the program and to take advantage of the new educational opportunities presented first by the dispensary schools and later in the *ḥakīm* class at the Lahore Medical School was viewed by government as a positive reaction initially. However; government's reaction caused regular practitioners to fear that the use of "lower" quality medical men would diminish the allopathic profession's position. This fear was strengthened by the Lieutenant Governor's approval in 1872 of a plan to grant Titles in Oriental Medicine at Punjab University College. That this might lead to a day when "qualified" indigenous practitioners could attain equal status with all allopathic physicians was borne out ten years later when a medical registration bill was proposed. The bill was supported by *ḥakīms* and opposed by most allopathic physicians because it seemed to grant just such equality.

The reaction of the regular profession throughout the period was to suggest that, by supporting the *yūnān-i ṭibb* in any way, government was encouraging the most regressive tendencies in Indian society. Lay administrators were accused of attempting to revitalize and restore a system incapable of such change. Yet even the *ḥakīm* system's most ardent proponent, T.W. Mercer, was not concerned to restore or revitalize *yūnān-i ṭibb*. Mercer, in fact, planned "the gradual substitution of English Medicine for useless native drugs, the attendance of the sick of all classes, to afford prompt medical relief, and ultimately, the subversion of the system of medicine as practiced by the natives."[51] His aim was no different than that of the allopathic practitioners—the establishment of a medical system in Punjab based entirely on Western medical principles. But to insist, as many allopathic practitioners did that this must be done without concession to local belief and custom was in Mercer's opinion "to insure resistance on the part of the population and to slow the spread of Western medicine for years to come."[52] Neither Mercer's long-range plan to subvert indigenous medical practice nor his argument against the all or nothing approach to the extension of medical relief was accepted by allopathic practitioners nor by their supporters in the civil administration.

The Punjab Government's decision in 1889 which led to the virtual exclusion of *ḥakīms* from government medical service appeared to be a victory for scientific medicine. It certainly was a victory for allopathic practitioners to the extent that it gave them a monopoly of government medical positions. However, insofar as it was designed to aid development of a medical system based solely on "scientific" principles, exclusion was a failure.

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[51] Punjab *Gazette*, *op. cit.* (n 18 above) paragraph 7.

[52] *Ibid.*, paragraphs 17-18.

Many *ḥakīms* were certified as proficient in both Western and Oriental medicine. These hybrid practitioners dominated private practice in the cities and towns for many years to come.[53] However, it was in rural areas that the pro-allopathic decision had its most adverse effect for, as Lyall conceded, so long as a dispensary-oriented system of medical relief continued it would be impossible to reach most people in rural areas unless *ḥakīms* were used.[54] Thus by excluding the *ḥakīm*, Punjab Government decided, in effect, that if the choice was between poor quality medical relief or no medical relief at all in rural areas, then government must support no medical relief.

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[53] The conflict between the various systems continues, see Paul R. Brass "The politics of ayurvedic education. a case study of revivalism and modernism in India" *Education and Politics in India*. eds Susanne H. Rudolph and Lloyd I. Rudolph (Delhi: Oxford University Press, 1972) 342-371 ; Charles Leslie "The professionalization of ayurvedic and unani medicine," *Transactions of the New York Academy of Sciences*, 1968, *30*. 559-572.; Charles Leslie, "The professionalizing ideology of medical revivalism," *Enterpreneurship and Modernization of Occupational Cultures in South Asia*. ed. Milton Singer (Durham: Duke University Press, 1973) 216-242.

[54] *P.G P. Home M&S., op cit* (n. 42 above)

ANIMAL SACRIFICE IN AMERICAN FOLK CURATIVE PRACTICE*

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Animal sacrifice has not yet outgrown its primitive roots in many parts of the world, but it will come as a surprise to many people interested in folk medicine that this inveterate practice should still be encountered in twentieth-century America. The term sacrifice as used throughout this paper generally does not refer to any religious act or ritual involving the propitiation of Gods and other divinities, nor does it denote measures to neutralize the hostility of demons or other powers thought to underlie impaired physical condition or disease.[1] Specifically, animals are killed so that the medical practitioner may gain medicinal products, or so that, among other things, the patient may benefit from the absorptive power of warm viscera or quivering flesh as an application to ailing parts. It is in the choice of animal to be dispatched, and the manner of killing, that ritualistic vestiges of sacrifice are to be seen in this country, rather than in religious or cultic acts of propitiation and worship. Although it is nowhere hinted at in the literature, the forfeit of the animal's life may once have been construed, and still might be in the minds of some, as a miraculous means of restoring health to a disease-ridden body. Within the memory of man since the time of colonization, however, the taking of the animal's life was never likely thought of, except among the aboriginal peoples, as a sacrificial act whereby sinful and diseased man could be rid of his malady as he was brought into harmony and favor with sacred powers specifically through the treasured offering of an animal's life. Since much is known about sacrificial practice among the North American Indians,[2] I am limiting my discussion of the subject to American customs deriving, in the main, from European tradition.

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[1] James Hastings, ed., *Encyclopaedia of Religion and Ethics,* 13 vols (Edinburgh T & T. Clark, 1908-1926), XI, 1-39, s.v. "Sacrifice," esp pp. 1-7; Eduard von Hoffmann-Krayer and Hanns Bachtold-Staubli, eds., *Handworterbuch des deutschen Aberglaubens,* 10 vols. (Berlin and Leipzig: Walter de Gruyter, 1927-1942), IX, Nachtrag, 19 ff., s.v. "Opfer" (hereinafter abbreviated *HDA*); E. O. James, *Sacrifice and Sacrament* (London Thames and Hudson, 1962), Northcote W. Thomas' entry on "sacrifice" in the 13th edition of the *Britannica* (1926), XXIII, 980-984, is exemplary

[2] Frederick Webb Hodge, ed., *Handbook of American Indians North of Mexico,* 2 vols (Washington, D.C : Smithsonian Institution, Bureau of American Ethnology, 1912), II, 402-407, s.v. "Sacrifice "

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Treated in the present paper are only cases involving the actual killing of animals. The wide range of animal products, often called "simples," if they are not compounded with other ingredients, cannot be treated here. However, in passing, it must be noted that these medicaments are usually gained from the animal after it has been killed, either specifically for the purpose at hand, or as by-products of an animal slaughtered for meat, or birds and fish taken for the same purpose. In these cases the killing of the animal itself, or its death, if natural, is never mentioned. Products constituting the zoological materia medica range from all kinds of internal parts to hide and hair, hooves, horns, feathers, claws, beaks, scales, and the like, not to mention the excrement and other effluvia such as saliva and urine.

Blood, perhaps more than most of the animal products mentioned, is thought to possess special potency, and may, in the minds of many patients and healers, have a special magical efficacy as well. This special power is supposed to reside in the blood itself as the life principle, as well as the seat of human vitality and the physiological basis of life itself.[3] In some parts of the world the power of the soul is believed to be resident in the blood,[4] and in many religious systems blood figures importantly, if not centrally, in notions of sacrifice,[5] and in doctrines of sin and expiation.[6] Blood as a life token, of course, is well attested in folklore, where clear water, for example, may become bloody, or likewise a sword, when a friend or kinsman is in mortal danger or slain in a far distant place.[7]

The drinking of animal blood is not common in American folk medicine, and, where this practice is resorted to, it does not appear to have any special ritual significance. The consuming of blood in this way, most often for tuberculosis itself, takes place where animals are slaughtered.[8] I remember as a boy in Salt Lake accompanying my father with a consumptive who went to a slaughterhouse to imbibe the warm blood of a calf. The drinking of blood of creatures other than meat-producing animals would, in the ordinary scale of things, move into the realm of magical, if not sacrificial, healing. In the Rio Grande country, for example, a black cat was pressed into service for the cure of consumption. The cat was killed, the bones extracted, and the consumptive rubbed with the flesh from head to foot. Finally he was fed the cat's blood mixed with warm water.[9]

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[3] James, *op. cit.* (n. 1 above), pp. 60-63, Hastings, *op. cit.* (n. 1 above), II, 714 ff., *HDA*, I, 1434, s.v. "Blut."

[4] *HDA*, I, 1435, s.v. "Blut."

[5] Hastings, *op. cit.* (n. 1 above), II, 719.

[6] James, *op. cit.* (n. 1 above), pp. 104-114, esp. pp. 110-111.

[7] Stith Thompson, *Motif-Index of Folk-Literature*, 2nd ed., 6 vols. (Bloomington, Ind.: Indiana University Press, 1955-1958), E761.1.

[8] Paulme Monette Black, *Nebraska Folk Cures* (University of Nebraska Studies in Language, Literature, and Criticism, No. 15; Lincoln, 1935), p. 14, no. 81 (consumption); Joseph D. Clark, "North Carolina popular beliefs and superstitions," *North Carolina Folklore*, 1970, *18*: 17, No. 333 (consumption); Thomas R. Brendle and Claude W. Unger, *The Folk Medicine of the Pennsylvania Germans. The Non-Occult Cures* (Proceedings of the Pennsylvania German Society, XLV), Norristown, 1935, p. 106 (epilepsy). Negroes in the South drank the blood from the heart of a young heifer for bronchitis (Newbell Niles Puckett, *Folk Beliefs of the Southern Negro* (Chapel Hill, N. C.: University of North Carolina Press, 1926), p. 370).

More ritualistic kinds of bloodletting, all resulting in the death of the animal in question, is seen in an account from Lunenberg County, Nova Scotia, where a child with asthma was made to stand in the hot blood of a slaughtered animal;[10] from Tennessee, where the gushing blood of a freshly killed pig was thought to help eczema;[11] and from Illinois where the warm blood from a freshly killed beef was made to drip on hands afflicted with eczema.[12] In their general outlines all three of these cures are not too unlike the *taurobolium* of antiquity,[13] where the blood of a slain bull was made to flow freely over communicants huddled beneath a sacrificial scaffold. To return to the point made earlier, even though sacramental regeneration did not figure in any of the American blood rituals listed above, physical renewal was at the basis of every single one. Once more I must emphasize the fact that this explanation is one which I myself have supplied. Never was the life of the animal itself thought of as effecting the cure; rather, the cure was expected as a result of the special power of the blood as a medicine.

The use of the blood from a black cat for the cure of shingles is too well known for me to dwell upon it here. Whether the cat is killed outright,[14] or its blood is gained from an amputated tail,[15] there is a ritualistic and even magical character to the whole procedure. This aura is heightened when it is specified that the cat must be killed in the light of the moon,[16] or that the animal must be without a single white hair, as reported from the eastern and central parts of the United States.[17] An instructive item from western New York is worth giving in its entirety here for the light it sheds on the ritual aspects of the cure: "Shingles (Herpes zoster) is said to be cured by applying the skin of a black cat. The animal must be without a white hair; its throat must be cut, the blood collected in a cup and then poured into the freshly removed skin, which is to be applied to the diseased surface and will cure in a few hours. A correspondent says, 'This is no hearsay matter with the writer, for in his boyhood he was afflicted with the disease and passed a night with the blood of his favorite pussy covering his left side and the pit of his stomach.' "[18] As one further example of unadulterated quality sought in the animal, and also of the imbibing of blood from animals not used for food, a treatment for consumption from the Pennsylvania German country is cited. It dates from the early 1890s. "For curing consumption, catch a black cat without a single white hair; a teaspoonful of blood from the tail will surely cure."[19] More primitive in its impact on the average person, perhaps, is the following belief from Oklahoma: "Find an animal of absolute solid color, and bring it to the bedside of the patient. Kill the animal and quickly apply the hot blood to the affected place."[20] This practice, if indeed it was widespread enough to deserve the term, dates from the 1930s.

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[9] John G. Bourke, "Popular medicine, customs, and superstitions of the Rio Grande," *Jour Am. Folklore,* 1894, *7:* 123

[10] Helen Creighton, *Folklore of Lunenberg County, Nova Scotia* (National Museum of Canada, *Bulletin,* No. 117, Anthropological Series, No 29), Ottawa, 1950, p 86, No 4.

[11] Jerry S Parr, "Folk cures of middle Tennessee," *Tenn Folklore Soc Bul.,* 1962, *28*: 10.

[12] Harry Middleton Hyatt, *Folklore from Adams County, Illinois,* 2nd ed ([New York]. Alma Egan Hyatt Foundation, 1965), p. 263, No 5819

[13] James, *op. cit*. (n 1 above), pp. 247-248.

[14] Wayland D. Hand, ed., *Popular Beliefs and Superstititions from North Carolina* (constituting Vols VI-VII of *The Frank C Brown Collection of North Carolina Folklore,* 7 vols.; Durham, N C.· Duke University Press, 1952-1964), VI, 271-272, No. 2096. (Hereinafter cited: *Brown Coll.*)

[15] *Ibid* , No 2097

[16] Daniel Lindsey Thomas and Lucy Blayney Thomas, *Kentucky Superstitions* (Princeton, N J. Princeton University Press, 1920), p 123, No. 1443; Clark, *op. cit* (n. 8 above), p. 33, No. 815.

We have already seen examples of the cutting off of a cat's tail to combat shingles, so we can turn to other examples of a kind of ritual killing and torture. Ignoring cases of the use of the legs of various kinds of animals, once the animal has been dispatched, I cite only examples involving the severing of members while the animal is alive. In Illinois, within the last few years a cure for warts was prescribed which involves the cutting off of a frog's leg, and rubbing the leg to warts while the flesh still quivers.[21] Contact of the blood with the wart, though not stated, must be presumed. A related item from Indiana, collected only thirty years ago, does make clear the blood part of the office. "To facilitate the cutting of teeth, pull off the head of a black hen, and rub the child's gums with the blood neck."[22] The pulling out of the legs of insects is accomplished with less exertion, for which reason the ritual aspect of the dismemberment might be somewhat lessened. Among southern Negroes, the legs of live bedbugs and grasshoppers were pulled off, wrapped in dough, and swallowed alive to combat the chills.[23] The same process was followed in North Carolina, with the sacrifice of a "granddaddy."[24] This last ritual is rare because, as I suspect, the spider is an insect that enjoys immunity from pursuit in Anglo-Saxon countries, as attested by the old verse,

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[17] Fanny D. Bergen, *Animal and Plant Lore Collected from the Oral Tradition of English Speaking Folk* (Mem. Amer Folklore Soc., VII, Boston and New York, 1899), p 68, No 764

[18] *Ibid* , p 147, note to No 764.

[19] Frederick Starr, "Some Pennsylvania German lore," *Jour Am. Folklore*, 1891, *4* 322

[20] Oklahoma Writers' Project, unpub MS, p. 51 (Copy in the author's possession through the courtesy of the Oklahoma Historical Society, Oklahoma City, Okla)

[21] John W. Allen, *Legends and Lore of Southern Illinois* (Carbondale, Ill Southern Illinois University Press, 1963), p 61.

[22] Paul G Brewster, "Folk cures and preventives from southern Indiana," *Southern Folklore Quart* , 1939, *3*. 39, No. 3

> If thou wouldst live and thrive,
> Let the spider run alive.

Since crickets enjoy the same favor and immunity, I am not surprised to find few examples of the mutilation of this fabled denizen of the hearth. An example from Texas, collected within the last five years, involves the killing of the cricket before its leg is cut off.[25]

The dry picking of birds and fowls, one of the lesser barbarities in killing and healing rituals, fortunately, is not widespread. The first of the following two examples, an item from West Virginia, involves the plucking of the feathers from the breast of a live pigeon. The exposed part of the bird is placed on the stomach of a person suffering from convulsions, for the purpose of drawing off the seizure. The absorption of the malady definitely harms the bird, but apparently does not kill it.[26] The dry picking of a chicken in an Indiana folk medical prescription for scarlet fever, however, is only a prelude to flaying a strip of skin three inches wide from the breast bone and back while the chicken is warm. The skin is then placed around the patient's neck to draw out the poison. The reader is left to guess what finally betides the hapless fowl.[27]

Flaying alive is a brutality that even exceeds dismemberment. Widely known in animal sacrifice elsewhere,[28] this barbarity is little known in this country. Cases of freshly-skinned animals, however, are numerous in American folk medicine, but only flaying alive is adverted to here. In Newfoundland, lump cramp is treated by twisting an eel, skinned alive, around the affected muscles.[29] A Kentucky cure for warts involves the tearing off of a toad frog's skin, but one cannot tell clearly from the entry whether the creature is supposed to be alive or dead at the time the brutal flaying takes place.[30]

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[23] Puckett, *op cit.* (n. 8 above), p. 366.

[24] *Brown Coll.*, VI, 144, No. 1047

[25] John Q. Anderson, *Texas Folk Medicine 1,333 Cures, Remedies, Preventives, & Health Practices* (Austin, Tex.· The Encino Press, 1970), p 81.

[26] Ruth Ann Musick, "West Virginia Folklore," *Hoosier Folklore*, 1948, *7.* 7.

[27] W Edson Richmond and Elva Van Winkle, "Is there a doctor in the house?" *Indiana History Bull*, 1958, *35* 131, No. 231.

[28] *HDA*, VII, 1076, s v. "schinden."

[29] Bergen, *op. cit* (n. 1 above), p. 68, No. 770.

We have already learned of the consumption of animal blood and have considered cases of blood applied for maladies of the skin and for other external uses. We have also seen the animal's skin employed in various applications, with or without emphasis on the bloody component of the freshly skinned pelt. Now we come to what is perhaps one of the major uses of animals' parts freshly killed, namely, the application of the flesh and internal organs of animals applied to wounds of all kinds, swellings, and other conditions where the warmth and the supposed absorptive power of the animal flesh is thought to exert a sort of natural healing quality. The magical healing power of the flesh is never mentioned, if, indeed, there is any indication that such power is resident in the warm and bloody mass itself. The magical character of the ritual must be deduced from the nature of the animal, as seen in its total folkloric configuration. The details of the sacrificial ritual itself, the special actions of the officiant, if any, and other details may add up to enough information to distinguish these "ritualistic" killings from the ordinary slaughtering of animals, but there are no clear-cut criteria to follow.

In the case of larger animals, they are usually killed before any cutting open and evisceration can be undertaken. In the case of birds, barnyard fowls, and other small animals, however, the cutting open, the tearing apart, or the wringing of the neck is often the means of death itself. This ritual act is carried out in all parts of the country, and Foster assures us that this practice is also widespread throughout Spanish America.[31] In the United States, for example, the flesh of a chicken freshly cut open is applied not only to absorb the poison of various kinds of snakebites and insect stings and to reduce swellings and inflammations of all kinds, but it is used as a specific, among other things, for such assorted maladies as epilepsy, burns, shingles, pneumonia, and the like. Black chickens, magical animals widely used in witchcraft and conjury, are often preferable to ordinary ones. In North Carolina, for example, a black chicken is cut open while alive and bound to the foot of the patient to draw out the fever.[32] Erysipelas is treated in

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[30] Genevieve Pope, "Superstitions and beliefs of Fleming County," *Kentucky Folklore Record*, 1965, *11* 45.

[31] George M. Foster, "Relationships between Spanish and Spanish-American folk medicine," *Jour Am Folklore*, 1953, *66*. 206

[32] *Brown Coll.*, VI, 187, No. 1415

Louisiana by cutting open a black frizzly chicken and applying its warm parts to the inflamed area.[33] In the same state, special care is taken in the treatment of bites, that the black hen be applied while still jumping. When the chicken has stopped fluttering, the poison is supposed to have been withdrawn.[34] In Nebraska an option is given as to whether or not the hen should be placed on the bite after it has been killed, or while still alive.[35] This cure is specified for snake bite.

Frogs and toads, both highly magical animals, are cut open and bound to afflicted parts, for drawing out poison, inflammation, soreness, and the like, in parts of the country as far removed from each other as the eastern seaboard,[36] the gulf states,[37] and the Rocky Mountain country.[38] An item from Texas specifies, for example, that the frog must be applied to a sore finger while the animal is still alive and kicking. In the Alleghenies, the bodies of living toads, successively applied to cancer, it is believed, will gradually remove the growth. At first the toads die very rapidly, but mortality is lessened with each application and finally ceases altogether. At this point, recovery is then complete.[39] The use of frogs, split open in the manner stated, dates from the time of Pliny the Elder, when it was noted as a cure for podagra.[40] The cutting open of household pets such as cats and dogs, known in Europe,[41] apparently is little encountered in America. A case of cutting off a cat's tail for use in curing shingles, however, was noted earlier in this paper, and we shall shortly see the sacrifice of a dog for the curing of erysipeleas.

Cutting open is one thing. Tearing in half is quite another. This brutal business, perhaps more magical and ritualistic than cutting asunder, is certainly more primitive, and is to be found in different parts of the country. It must nevertheless be considered a rarity. In Texas, a jet black chicken is specified. Such a chicken is torn open, and the blood rubbed on chicken pox sores.[42] Although I have resisted citing European material in the body of this paper, a custom in Dorsetshire, still practiced in the early 1920s, shows what a firm hold the savage killing of animals still has on the minds of people in the modern civilized community. Udal writes: " 'Cunning men' used to hold an annual levée in the neighborhood of Stalbridge, and sold to the crowds legs torn from the bodies of living toads and placed them in a bag which was worn around the neck of a patient, and counted a sovereign remedy for scrofula and the 'overlooked.' It was called 'Toad Fair.' "[43]

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[33] Hilda Roberts, "Louisiana superstitions," *Jour. Am. Folklore*, 1927, *40* 167, No 425.

[34] Lyle Saxon, *Gumbo Ya-Ya* (Boston: Houghton Mifflin, 1945), p. 525.

[35] Roger L. Welsch, *A Treasury of Nebraska Pioneer Folklore* (Lincoln: University of Nebraska Press, 1966), p 357

[36] T. Horace Fitchett, "Superstition in North Carolina," *The Crisis*, 1936, *43* 360.

[37] Anderson, *op. cit* (n. 25 above), p 11

[38] Austin E Fife, "Pioneer Mormon remedies," *Western Folklore*, 1957, *16*: 162.

[39] J Hampden Porter, "Notes on the folk-lore of the mountain Whites of the Alleghanies," *Jour Am. Folklore*, 1894, *7* 112

[40] Pliny, *Natural History*, XXXII, 36.

[41] John Moncrief, *The Poor Man's Physician, etc.*, 2nd ed (Edinburgh, 1716), p 67; Ignacio Maria Barriola, *La medicina popular en el pais vasco*, (San Sebastian Biblioteca Vascongada de los Amigos del Pais, 1952), p 58.

An extension of the application of warm viscera to a wound is the placing of an ailing member into the body cavity of an animal killed for the purpose. Larger animals, mammals usually, are pressed into service. In the Ozarks a steer is killed, and the bare feet of a patient suffering from asthma are thrust into the warm bodily cavity until the entrails cool.[44] For snakebite, according to a Nebraska folk medical belief, an animal should be cut open, preferably a cow, and the bitten area should be buried in the middle of the animal until the poison is drawn out as the carcass becomes cold.[45] One must assume that the animal is killed specifically for this cure, as people would be unlikely to eat meat which is permeated by the poison of the snake. Among the Pennsylvania Germans the custom once prevailed of killing a dog, cutting it open, and then placing the feet of a patient suffering from erysipelas into the body cavity upon the entrails.[46] Dykstra mentions the use of cats as well as dogs for a similar office in Friesland.[47] Chickens, though smaller and little suited to this kind of a healing procedure, are nevertheless used in Illinois for the cure of rheumatic afflictions.[48] If both feet or legs are affected, two live chickens are cut open to encase one foot each.[49]

The rendering out of the fat of various kinds of worms, snails, spiders, and other kinds of insects, either by corking them up in a bottle to die of suffocation or starvation, and finally to be rendered out by the rays of the sun, or by other kinds of warmth, is a practice well known in all parts of the country.[50] Some of these cures derive, no doubt, from the so-called *Egyptian Secrets* of Albertus Magnus, cheap editions of which were once available in all parts of the country. In Indiana, oil was even tried out of toads in this conventional way, a little salt being placed into the sealed can with the animal.[51]

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[42] George D. Hendricks, *Mirrors, Mice & Mustaches A Sampling of Superstitions and Popular Beliefs in Texas* (Austin. University of Texas "Paisano Books," 1966), p. 37

[43] John Symonds Udal, *Dorsetshire Folk-Lore* (Hertford: S. Austin & Sons, 1922), pp 215-216 For American examples of tearing apart see Ray B. Browne, *Popular Beliefs and Practices from Alabama* (University of California Publications, Folklore Studies, IX, Berkeley and Los Angeles, 1958), p. 94, No. 1603: Frost Woodhull, "Ranch remedios," *Publ. Texas Folklore Soc* , 1930, *8*: 64

[44] Vance Randolph, *Ozark Superstitions* (New York· Columbia University Press, 1947), p 135

[45] Louise Pound, "Nebraska snake lore," *Southern Folklore Quart* , 1946, *10* 168

[46] Emma Gertrude White, "Folk-Medicine among Pennsylvania Germans," *Jour Am. Folklore*, 1897, *10* 79

[47] Waling Dykstra, *Uit Frieslands Volksleven van vroeger en later*, 2 vols. (Leeuwarden Hugo Suringar, n.d.), II, 259

[48] Hyatt, *op cit* (n 12 above; 1935 ed), pp 261-262, No 5328.

[49] Hyatt, *op. cit* (n 12 above, 1965 ed.), p. 256, No 5691.

Akin to corking up a creature for the purpose of rendering out its fat, as treated above, is another form of sealing the animal up. In this latter instance, however, a magical cure, rather than one brought about by the use of an animal product, seems to be involved. Although the spider, caterpillar, woodlouse, or other kind of insect is placed in a thimble, a nutshell, or sewed up in a bag to be worn around the neck, its use as an amulet is contingent on the insect's death, for not until the creature dies is the cure effected. Cures of this kind, well known in Europe,[52] have been reported from the province of Ontario, throughout the South, and as far west as Illinois, Indiana, and Texas.[53] Not only are such diseases as whooping cough, thrush, and common coughs and colds involved in the cures, but these amulets are worn to aid dentition in children.[54]

Because of its affinity to rubbing and squeezing, mashing is taken up here rather than under mutilation, which was treated above. The mashing of sowbugs, angleworms, and spiders, often in one's own hands, is done either to alleviate pain magically through the act of crushing the creature to death,[55] or to provide a substance for salves, teas, or other medicaments destined for later use.[56] Holding a creature in one's hand until it dies, rubbing it to death; or squeezing the life out of it are all done to the same general effect. These magical practices are related to the ancient practice of squeezing a mole to death to obtain power as a healer. This whole subject deserves special treatment, but its salient features may be noted here. Before discussing moles as magical creatures in curative practices, we can briefly allude to the squeezing to death of a mouse in Alabama for curing of pain in general,[57] the throttling of weasels for the cure of felons and other maladies in Indiana,[58] use of frogs in North Carolina and Georgia for the cure of chills,[59] and bullfrogs in Georgia for the same malady.[60] A slower and more painful death comes to a toad used to cure warts in the Pennsylvania German country. Employed for this purpose is the first toad encountered in the spring. The creature is simply rubbed on the wart until the toad itself dies.[61] Contagious as well as symbolic magic, of course, is seen in this cure.

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[50] Bergen, *op. cit.* (n. 17 above), p. 73, No. 836, Musick, *op. cit.* (n. 26 above), p. 5, Edwin Miller Fogel, *Beliefs and Superstitions of the Pennsylvania Germans* (Americana Germanica, XVIII, Philadelphia, 1915), p. 336, No. 1781

[51] Brewster, *op. cit.* (n. 22 above), p. 35.

[52] Sidney Oldall Addy, *Household Tales With Other Traditional Remains Collected in the Counties of York, Lincoln, Derby, and Nottingham* (London: David Nutt, 1895), p. 91, W. Branch Johnson, *Folktales of Normandy* (London: Chapman and Hall, 1929), p. 191.

[53] W. J. Wintemberg, *Folk-Lore of Waterloo County, Ontario* (National Museum of Canada, *Bulletin*, 116, Anthropological Series, No. 28, Ottawa, 1950), p. 15, Brendle-Unger, *op. cit.* (n. 8 above), pp. 132-133, Richmond and Van Winkle, *op. cit.* (n. 27 above), p. 135, No. 311; Allen, *op. cit.* (n. 21 above), p. 83, Hendricks, *op. cit.* (n. 42 above), p. 54

[54] Collins Lee, "Some Negro lore from Baltimore," *Jour. Am. Folklore*, 1892, *5*: 111

[55] Fogel, *op. cit.* (n. 50 above), p. 302, No. 1598.

[56] Saxon, *op. cit.* (n. 34 above), p. 536; Hyatt, *op. cit.* (n. 12 above; 1965 ed.), p. 325, No. 7031; Randolph, *op. cit.* (n. 44 above), p. 101.

Perhaps the prototype of all rituals of rubbing to death and squeezing to death of small animals as a means of gaining healing power on the part of aspiring healers is the lowly mole. This idea, fathered on Paracelsus, but really centuries older, is known everywhere in Europe,[62] and it is also well known in many parts of America. Accounts of how the creature is killed vary from holding it until it finally dies,[63] rubbing it to death,[64] crushing it,[65] smothering it,[66] or dispatching it quickly.[67] In Louisiana it was believed, for example, that a live mole was caught and rubbed to death. If one succeeded in killing the mole in this manner without being bitten, he would gain the power of healing.[68] Healers who have acquired the magical gift perform the ordinary kind of ministrations for various ailments, or it is simply stated that they are able to cure such and such a disease, sometimes by the mere touch, at other times by squeezing such a growth as a felon, as in Maryland,[69] or rubbing pains out of the body, as in South Carolina, and elsewhere in the South.[70] In the Allegheny Mountains, sprains are cured by applying interrupted pressure to the affected area with the hand in which a mole has been squeezed to death.[71] The curing of warts is accomplished in the Ozark country by smothering a mole and then holding the animal above the head for a moment.[72] In addition to being credited with the ability to cure all manner of excrescences, including the allaying of common festerings and so-called risings,[73] a healer who has gained his curative power from a mole is supposed, in different parts of the country, to be able to cure such diverse ailments as rheumatism,[74] headache,[75] and milk leg.[76]

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[57] Browne, *op. cit.* (n. 43 above), p 123, No 2149.

[58] Richmond and Van Winkle, *op. cit.* (n. 27 above), p 126, No 147.

[59] *Brown Coll*, VI, 144, No 1050; Saxon, *op cit* (n 34 above), p. 527.

[60] Marie Campbell, "Folk remedies from south Georgia," *Tenn Folklore Soc Bull*, 1953, 10· 2

[61] Fogel, *op cit* (n 50 above), p 323, No. 1720)

[62] HDA, III, 1393, s v. "Hand", VI, 20, s v. "Maulwurf"; Paul Sébillot, *Le folk-lore de la France*, 4 vols. (Paris: Librairie Orientale & Américaine, 1904-1907), III, 48, *Revista de Dialectología y Tradiciones Populares*, 1947, *3*: 398, Johnson, *op cit* (n. 52 above), p 195 Importance of the mole in matters pertaining to folk medicine, including a wide range of sacrificial customs and rituals, can be seen historically in a paper by Jaan Puhvel and Wayland D. Hand, "The mole in folk medicine· a survey from Indic antiquity to modern America," in Wayland D Hand, ed , *American Folk Medicine A Symposium* (Publications of the UCLA Center for the Study of Comparative Folklore and Mythology, 4; Berkeley and Los Angeles, 1976), pp. 31-48.

[63] Lee, *op. cit.* (n 54 above), p 111. Cf *Brown Coll*, VI, 110, No. 750

[64] *Brown Coll.*, VI, 114, No 778

[65] Browne, *op cit.* (n. 43 above), p 97, No. 1668.

[66] *Brown Coll*, VI, 110, No. 750

[67] Browne, *op cit.* (n 43 above), p. 123, No 2148 Cf. *Rev. Dial y Trad Pop.*, 1947, *3* 398

[68] Lyle Saxon, *Old Louisiana* (New York: Century Co , 1929), p 352

Ritual magic in connection with the sacrifice of moles is seen in the innocence of the healer receiving the highly prized gift of healing. In America, as well as in Europe,[77] this boon is thought best acquired by a child before his seventh year. In Pennsylvania and Kentucky, among other places, a youngster in this state of innocence, if he kills a mole with his own hands, is said to have the power to cure felons.[78] In Germany the gift of healing may be acquired, it is believed, even in the cradle.[79]

We have just considered the matter of innocence of the healer himself as a factor in the magical transmission of the gift of healing from the dying animal to the human recipient of the divine gift. There is not space to develop the whole notion of innocence, unadulterated quality, freedom from taint and blemish, and other states of perfection or acceptability that are often said to attend the healing office. This I have done in a recent paper,[80] but the emphasis in that effort was entirely on the healer. It is fitting in this present paper to call attention to certain of these qualities in the sacrificial animal. As we saw earlier in the discussion of shingles, they deal not so much with innocence as with unadulterated quality, solid color, and the like. In Alabama, a madstone gained from the stomach of a solid-colored cow that has never given birth, for instance, is used to combat hydrophobia.[81] The animal is not dispatched, of course, but in a cure for high fever, reported from Louisiana, a pigeon which has never flown from the cage (and hence is in the pristine state), is cut open and laid on the patient's head. The fresh blood is supposed to cure the fever.[82] The remaining examples deal with animals of solid color, in every instance of which store is laid by the fact that there is not a single white hair in the coat of the black cat in question, or the black dog, or the black hen. The blood of a black cat of this superb coating is credited, for example, with curing styes in Newfoundland,[83] fits in Nova Scotia,[84] and consumption and erysipelas in the German tradition of Pennsylvania.[85] In central Maine fat obtained from a black dog, without a single white hair, is cooked and eaten for consumption,[86] while in Maryland an entirely black chicken is cut in two and bound to the breast of of a sufferer with fits.[87] Ritual magic is seen in all these cases of sacrifice.

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[69] Annie Weston Whitney and Caroline Canfield Bullock, *Folk-Lore from Maryland* (Mem. Amer. Folklore Soc XVIII; New York, 1925), p. 83, No. 1707; Puckett, *op. cit.* (n 8 above), pp 378-379; Fogel, *op cit.* (n. 50 above), p. 293, No 1551

[70] Fitchett, *op cit.* (n. 36 above), p. 360

[71] Porter, *op. cit.* (n. 39 above), p. 111; *Brown Coll* , VI, 320, No. 2479.

[72] Randolph, *op cit* (n. 44 above), p. 130.

[73] *Brown Coll* , VI, 299, No. 2314.

[74] Puckett, *op. cit.* (n. 8 above), p. 362.

[75] *Brown Coll* , VI, 207, No. 1587

[76] Browne, *op cit* (n 43 above), p. 80, No 1351.

[77] *HDA*, VI, 20, s.v "Maulwurf", Marcelle Bouteiller, *Médecine populaire d'hier et d'aujourd'hui* (Paris: Maisonneuve et Larose, 1966), p. 282

[78] E. Grumbine, *Folk-Lore and Superstitious Beliefs of Lebanon County* (Papers and Addresses of the Lebanon Co. Historical Soc., III, 1905-1906), p 280, Thomas and Thomas, *op cit* (n. 16 above), p. 97, No 1105.

[79] *HDA*, VI, 20, s.v "Maulwurf"

[80] Wayland D. Hand, "The folk healer: calling and endowment," *J. Hist. Med.*, 1971, *26* 263-275

Details of ritual often enhance the magical and sacrificial character of the killing of animals for curative purposes. Especially suited is night-time, midnight itself, or times coinciding with different phases of the moon, as seen in examples from West Virginia and North Carolina.[88] The killing often takes place in such numinous places as a graveyard,[89] or at a crossroads.

That there is not more material in the corpus of American folk medical practice dealing with animal sacrifice may be due in part to the fundamental religious and philosophical cast of mind in this country. In the religious teaching of the New Testament, where the sacrifice of one's faith and the dedication of one's life to the Christian ideal counts far more than the sacrifice of animals,[90] as under the Old Covenant, even the word "sacrifice"

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[81] Browne, *op cit.* (n. 43 above), p 73, No 1210

[82] Saxon, *op cit.* (n 34 above), p 529.

[83] Bergen, *op cit* (n. 17 above), p 68, No 761

[84] Arthur Huff Fauset, *Folklore from Nova Scotia* (Mem. Amer. Folklore Soc , XXIV, New York, 1931), p 197, No 334

[85] Starr, *op. cit* (n. 19 above), p. 322, *Pennsylvania Dutchman,* Mar 15, 1954, *5* (14) 6

[86] Bergen, *op. cit.* (n 17 above), p. 72, No 823

[87] *Jour. Am. Folklore,* 1907, *20* 160

[88] *West Va. Folklore,* 1964, *14* 47, No 22, *Brown Coll ,* VI, 118, No. 812, Cf. *HDA,* VI, 20, s v. "Maulwurf"

[89] Elza E. Fentress, *Superstition of Grayson County* (Kentucky), unpubl M.A thesis, Western State Teachers College, Bowling Green, Ky , 1934, p 58, No 11

[90] Philippians 2.17

MICHEL FOUCAULT: THE KNOWLEDGE OF POWER AND THE POWER OF KNOWLEDGE

JEAN CLAUDE GUÉDON

Presenting Foucault's work to an Anglo-American audience poses two different kinds of difficulties. First, Foucault's studies are extremely rich, but their very richness rests on a number of complex, interconnected arguments, not easily summarised within the narrow compass of an article. Second, Foucault's quest emerges from a long series of philosophical and political debates heard much more clearly on the banks of the Seine than near the Thames or, *a fortiori,* in the vicinity of the East River. Phenomenology and Existentialism never penetrated the philosophical circles of the English-speaking world and it is not surprising, therefore, to find out that Structuralism, itself a reaction against both of these philosophical stances, is viewed as a very cryptic exercise by most Anglo-Saxons. Only linguists, anthropologists and literary critics have paid some attention to the message of Structuralism; but listening, as everyone knows, does not necessarily imply acceptance or even hearing correctly. To complicate matters further, Foucault is not a Structuralist, strictly speaking, even though some of his approaches have obviously been inspired by Structuralism.

For reasons touched upon later, Foucault's works have been translated into English. This, however, proved to be a dubious honour indeed, as they emerged literally out of context, an isolated archipelago of studies lost in a sea of staunch empiricism and pragmatism. As a result, they have been perceived as very curious objects indeed; at least this is the impression derived from reading reviews of his books in English-language journals.

Nearly all English and American critics agree: Foucault, whatever he may be trying to do, is so very obscure! Alfred Lars Aronson, for example, generously provides us with the first sample of a long series of convergent appreciations: "Foucault's book [*Birth of the Clinic*] is extraordinarily difficult to read. Sentences, even paragraphs, are sometimes so convoluted as to escape analysis. . . . "[1] Peter Caws echoes the same sentiment while reviewing *The Order of Things*[2] and adds even a sense of "linguistic

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[1] Alfred Lars Aronson, "Medicine: history and theory," *The Yale Review*, 1974, *63* 476.

[2] Peter Caws, "Language as the human reality," *New Republic*, March 27, 1971, *164* 34

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outrage" in his account of *Birth of the Clinic*.[3] Presumably not to be outdone by his American counterparts, the English reviewer for the *New Statesman* complains that "great stretches of it [*The Order of Things*] are arid and impossibly obscure . . .";[4] and his colleague Zeldin, commenting on the *Archeology of Knowledge* for the same magazine, writes somewhat wryly: "It is no easy matter translating Foucault. He is obscure enough in his own language; in English, he is odder still . . . . In translation, he emerges as the archetypal foreigner—barely comprehensible, vaguely suspicious, best avoided."[5]

The list could go on at the cost of monotony, but the point is clear, I trust: most English speakers find Foucault either obscure, or difficult, or both. Yet, it is important to decide whether Foucault is genuinely obscure or merely difficult: Boileau reminds us that obscurity is the reflection of a confused mind and, therefore, represents a defect attributable to the speaker or writer;[6] difficulty, on the other hand, may be unavoidable in some cases where the subject matter is particularly complex.

Few French critics, if any, have ever raised the question of Foucault's obscurity. François Russo provides a particularly good example of French reactions to Foucault's writings, especially since he cannot be accused of cavorting with the more esoteric coteries to be found in Paris: such people do not generally spend time, as he did, putting together a bibliography on the history of science, or on any other subject for that matter. All this does not prevent him from giving a very positive review of Foucault's *Archeology of Knowledge*—clearly his most abstract and difficult work to date. For Russo, "this work deserves a precise and elaborate review all the more that, unlike most works in the same vein, it is well written and extremely clear. The author controls his topic; he knows where he is going; he is intent on defining the exact meaning of the notions which he uses and he explains himself with all the precision and rigour that may be asked for."[7]

Clarté en-deçà des Pyrénées; obscurité au-delà? Perhaps so, but David

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[3] Peter Caws, "Medical change," *New Republic*, November 10, 1973, *169*: 29-30.

[4] John Bossy, "Abstract acrobat," *The New Statesman*, June 4, 1971, *81*: 775.

[5] Theodore Zeldin, "An archeologist of knowledge," *The New Statesman*, December 7, 1973, *86*: 861-2. The same view is expressed in one of the better reviews of *Birth of the Clinic*. See Toby Gelfand, Review of *Birth of the Clinic*, *The Minnesota Daily*, October 8, 1973. My thanks to Professor Gelfand for having put this very interesting text at my disposal.

[6] In his *Art poétique*, Boileau wrote two verses known by every *Lycéen*:

Ce que l'on conçoit bien s'énonce clairement
Et les mots pour le dire arrivent aisément.

[7] François Russo, "L'archéologie du savoir de Michel Foucault," *Archives de Philosophie*, 1973, *36*: 71-72

Matza may have put his finger on one reason behind this remarkable difference of perceptions: "*Madness and Civilisation* is a difficult book," he writes.

> Sometimes such difficulty results from differences in style, and sometimes from a writer's cultivation of obscurity. Neither, I think, is the main reason for the difficulty experienced in seriously reading Foucault's book. The main reason is that we have grown discontentedly accustomed to reading books that apply well-known methods and concepts to phenomena that are hardly problematic to begin with. An encounter with a work that differs on both counts is apt to be arduous and demanding.[8]

Yes, Foucault is difficult, and he is so because he has completely shifted the mode in which the past can be questioned. Both the position occupied by the Foucaldian historian, i.e. the archeologist, and the landscape he can thereby uncover, seem very alien at first. Therein lies the difficulty of Foucault's analyses. It will be the burden of this paper to show how Foucault differs from historians of ideas in general, and historians of science in particular. It will also be the burden of this paper to try to demonstrate why Foucault's explorations are important for the doing of history of science.

Although viewed as obscure or difficult, Foucault has been translated into English and is being read in that language. As years go by, his books crowd more and more shelves of the universities' bookstores as they are incorporated into the reading lists of a growing number of courses. Like it or not, more and more discussions centre about him. But why? One answer is that he is an exciting writer, and most critics seem pretty well agreed on that point too.[9] The problem areas studied by Foucault are clearly of interest to many readers and the more fundamental methodological issues raised by him contribute even more to his appeal. As will be seen in this paper, his books ultimately lead to several important issues, including the status of knowledge, its function and the role of the knowledge-producer, or intellectual, in society.

The growth of Foucault's fame has had little to do with the field of history of science, history of ideas and, in general, history. And yet, most of his works ostensibly deal with historical topics. Actually, it is through psychiatry that Foucault has penetrated the English-speaking world—or more accurately, it is through the anti-psychiatric movement that this process took place. Apparently, R. D. Laing was the central figure in this respect,[10] and it is he who helped launch *Madness and Civilisation* on its English career. Yet Foucault does not owe any particular allegiance to the anti-psychiatric movement: for him, the question of insanity is only one facet of a much more general problem—namely the relationship between knowledge and power. But the publishing of Foucault's thesis coincided in time with the emergence of anti-psychiatry in the public eye. As a result, Foucault's book was able to skip over the Channel and land in England. To be sure, the resistance has been and still is fierce, but it is already too late: a beachhead has been secured and the invasion is on.

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[8] David Matza, "Review of *Madness and Civilisation*," *American Sociological Review*, 1966, *31* 551-52. Other reasons tied to intellectual and political factors will be investigated later.

[9] See Aronson, *op. cit.* (n. 1 above), 476, Caws, *op. cit* (n 2 above), 34; Caws, *op. cit.* (n 3 above), 30, Matza, *op. cit.* (n. 8 above), 551; Michael Peters. "Essay Review of Madness and Civilisation and of Piaget's Structuralism," *Sociological Reviews*, November 1971, n s. *19* 637.

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Many historians of science tend to place their discipline within intellectual history. But what is intellectual history? If we follow someone like Crane Brinton, intellectual history can be characterised in three different but essentially complementary ways. First, it deals with the question of who wrote what when. Second, it attempts to delineate a cartography of ideas, in the vein of Arthur O. Lovejoy, for example. Finally, it sets out to investigate the relationship between what men say and what they do.[11]

What is interesting in Brinton's description of intellectual history is less its possible accuracy than the vantage point from which it is written. And that vantage point, I believe, is fairly representative of intellectual historians.

Brinton's description—and this is quite striking—does not seem to consider that the social act of writing might have something to do with the kind of ideas ultimately embedded in a given text. Instead, authors are identified mainly to serve as a convenient short-hand label for a constellation of ideas. Thus Newton can stand in the place of notions like force, attraction or, alternatively, unitarianism. To the specific name, Newton, corresponds a specific notion of force, attraction, etc.

If, for Brinton, the social practice of writing seems to play little or no role in the emergence of ideas, ideas on the other hand may influence human events, and it is the task of the intellectual historian to understand how such a process may take place. In other words, Brinton and all those who share his assumptions tend to think that the history of ideas should be idealistic history; that is to say they tend to favour the power of ideas over the material world rather than the reverse.

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[10] Peters, *op. cit.* (n. 9 above), 636; R. D. Laing, "The invention of madness," *The New Statesman*, June 16, 1967, *73:* 843.

[11] Crane Brinton, "Intellectual History," in the *International Encyclopedia of the Social Sciences* (New York: MacMillan and the Free Press, 1968), vol 6, pp. 464-5. Crane Brinton was selected only because he happened to pen the article on Intellectual history in the *Encyclopedia* It is this latter work which determined my choice, as it constitutes a standard and respected reference

Transcribed into a vocabulary familiar to historians of science, Brinton's approach could read: certain scientists write certain scientific works at specific times and places. These embody theories and concepts which can be classified. On some occasions, some of these theories and concepts are used to generate new concepts and theories by and out of themselves; alternatively, they lead either to technical applications or are transferred analogically to other cultural domains such as art, literature or politics.

All of this is well known among historians of science: their many hours of research spent in the great libraries of the world aim at fulfilling this task. Internalists or externalists, they ultimately strive to write a history of scientific ideas which, to repeat myself, tends to take the side of idealistic history.

But there is a problem: *Ideas, almost by definition, do not possess a history.*

—Ideas do not possess a history? What nonsensical paradox is this? Scientific concepts and theories are made up of ideas. Given this limited claim, is it not obvious that the theories and concepts used by a Kepler, for example, are different from those enunciated by Copernicus? But they are not unrelated either. As a result, a text can be written retracing the transformation leading from the latter to the former. Now, how can you say that ideas do not possess a history?

—And so texts such as you describe can be and are written. But is the existence of such texts sufficient to guarantee their historicity? Surely not. If they are descriptive, we are faced with a simple chronology; if they attempt to include causes, then they are ideologically loaded. But to prove this last point, I suggest we explore the weaknesses inherent in the concept of history of ideas. The archeological method offered by Foucault provides a critical alternative to it. Its value, as we shall see, depends on the perception scholars have of themselves, whether they see themselves as privileged but alienated observers of society, or whether they intend to take an active part in it, including its transformation.

Foucault's studies are not disconnected, but rather must be viewed as an ongoing process of exploration. It is from this perspective that the *Archeology of Knowledge* must be approached. Coming as it did, after *Madness and Civilisation, Birth of the Clinic* and *The Order of Things,* it presents both a method and a methodical re-evaluation of Foucault's previous works. Unlike the three other books just mentioned, the *Archeology of Knowledge* is not based on archival research; nor does it try to cope with specific historical situations. Rather, it offers the sketch of an "archeological" method and ends by examining its implications for the status of knowledge.

The *Archeology of Knowledge* represents but a temporary stop in Foucault's intellectual movement, and it is already fairly old,[12] but it does provide an unusually sharp outline of his theoretical stance as well as a focused critique of the history of ideas. Consequently, the *Archeology of Knowledge* will provide the basis for most of the developments in the remainder of this paper. But I will not limit myself to this work alone and, as the case warrants, will draw on most of Foucault's studies, including his recent and admirable study of prisons.[13]

Let me recall that Brinton set the first task of intellectual history as responding to the question of who wrote what when. Foucault demonstrates that the who's, the what's and the when's are not the clear categories they appear to be. As an example, let us follow Foucault in his analysis of the "what."

The something which someone writes must somehow possess some unity to retain its identity of *something*. What are the units usually utilised by the historian of science in this respect? Most commonly, they are the book and the works (of an author).[14]

The book, despite its appearances (e.g. it possesses a well-defined volume; it has precise economic value), forms an ambiguous unit. For example, is an anthology as monolithic as a collection of posthumous fragments, or is it more so, or less so? What about a mathematical treatise on conic sections, or the first volume of Michelet's multi-volume history of France? Is the book the same kind of object when it recounts a trial, when it encloses a contemporary novel, or when it is a prayer-book? Consequently, the book as physical object does not maintain a one-to-one, or even a fixed, relationship with its verbal content. That difficulty alone undermines whatever claims that could be made on behalf of the book (still viewed as physical object) as a unit of analysis.

If the book cannot form a coherent unit of analysis, then why not look for units at the verbal level of the book. In other words, let us focus on what could be called *text* so as to examine whether it would form a viable unit of discourse. But the text is not without its own problems. For one thing, it does not always display the same kind of unity. There Foucault contrasts a novel by Stendhal to Dante's *Divine Comedy,* or Homer's *Iliad* to the *Odyssey*. Texts are caught within kinds of networks which differ according to the nature of their content. Mathematical texts do not relate to each other in the way that nineteenth-century French novels do. As a result, the retreat from books to texts has only made more obvious the elusive nature of the fundamental units which are nonetheless uncritically manipulated by historians every day when they treat documents as if they always functioned in a similar manner in the society which produced them.

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[12] Michel Foucault, *L'Archéologie du savoir* (Paris: Gallimard, 1969).

[13] Michel Foucault, *Surveiller et punir. Naissance de la prison* (Paris, Gallimard, 1975).

[14] The two paragraphs which follow essentially paraphrase and summarise pp. 33-6 of *L'Archéologie du savoir*

If books and texts evaporate, so to speak, before our eyes, the works of an author do not fare any better. First, works come in all kinds of forms and shapes, ranging from the piece of writing published under the author's name to the anonymous fragment; from the posthumous sketch to bits of correspondence. As Foucault writes: "Establishing the complete works, or *opus* presupposes a definite number of choices which are not easy to justify or even to formulate."[15]

Foucault's critique of the book, the text and the works leads to two consequences: on the one hand, intellectual history which relies almost exclusively on these categories finds its whole approach—objects manipulated as well as strategies of manipulations—fundamentally threatened. But at the same time, a sort of liberation takes place: all texts free themselves from their old form of organisation. As books and works lose their assumed natural evidence, texts can, in a sense, scatter themselves anew. As such, they cannot be approached yet, but new questions can arise on the ashes of the old order: in particular, what kinds of units could or should be reconstituted which would have some objective and tangible validity?[16]

In answer to this question, Foucault refuses to be seduced by ready-made solutions. For example, the use of the constitutive units of linguistics is one of the first temptations to be avoided. Linguistics, to be sure, does take a sample of texts actually spoken or written (*parole*) and then strives to recreate or discover the rules or laws which allow the construction of such texts. However, the laws of a *langue* can lead to an infinite variety of performances, and should in fact anticipate all aspects of *parole*. On the other hand, the collection of texts recently scattered by the "death" of books and works can be very large indeed, but never infinite, so that the constitutive units of discourse, unlike those of language, can and must generate only a limited number of discourses. Moreover, while linguistics would ask: "what are the rules used to construct such and such an enunciation?" Foucault would investigate how it is that this enunciation and none other appeared when and where it did.[17]

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[15] Ibid , p. 35 The same question is broached in M. Foucault, "Qu'est-ce qu'un auteur?" Bulletin de la *Société française de philosophie*, 1969, *63* (3). 73-104.

[16] *Ibid.*, pp. 38-39

If the texts just scattered by Foucault's analysis cannot be treated by linguistic means only, it is equally clear that the history of ideas cannot answer his question either. While the history of ideas attempts to reconstruct systems of thought from a judicious selection of texts, it does so by appealing to notions like the intentions of the author or authors, and even to their unconscious. In other words, the history of ideas tries to unveil what is really being said behind the surface of the texts surveyed.[18] Foucault, however, is looking for a form of analysis devoid of all references to psychologism, be they direct or indirect.

If texts are scattered into units yet to be identified, they must also be caught within some kind of boundaries. In response to this problem, Foucault is led to examine the "assumed continuities with which the discourse intended for analysis is already organised."[19] In other words, Foucault is weary of historians' strategies which, by their very nature, prejudge the results obtained in the course of the investigation. Obviously the ways in which documents are selected can be determinant and such a task must not be performed blindly. Historical continuities, for their part, provide ways to interfere with or justify certain kinds of selection patterns. As such, they must be examined very critically.

For Foucault, continuities impose themselves under two disguises: on the one hand, the historian looks for a hidden and more obscure origin to any event which gives the appearance (or is it reality?) of abruptly departing from existing conditions; on the other hand, he is constantly tempted to interpret the texts he reads in such a way as to bring to the fore a second text situated somewhere beyond the ostensible meaning of the discourse itself. Moreover, the hidden message is often perceived as the repetition or the elaboration of something said before. A good example of this approach is Koyré's "discovery" of Platonism behind the texts of Galileo. By transforming Galileo into a ventriloquist, Koyré is able to weave a continuous thread from Greek antiquity to the early seventeenth century, with all the ideological consequences such a method carries with it, like the unity of Western culture, its venerable age, etc.

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[17] *Ibid.*, p. 39.
[18] *Ibid.*, p 40.
[19] *Ibid.*, p. 36.

In short, Foucault argues, historians of ideas constantly try to erase discontinuities by refusing to take the brutal irruption of certain events at face value and by never listening to the author's ostensible message. Instead, they choose to listen to a hypothetical and repressed message, a sort of counterpoint to the first one, which must be brought to the surface thanks to their skill and the methods at their disposal, such as interpretation and exegesis. If influence and diffusion are introduced as the organising principles of the discourse of the historians of ideas, then it becomes possible to see culture spreading like a thin film of oil over the sea of time. The historians of ideas, in other works, consider that if the historical events he deals with proceed at all, then they proceed smoothly. And since he pays little or no attention to the actual mode of functioning of the documents he studies in their social setting, he can rest satisfied with a limited selection of "representative" works evenly spread in time, preferably but not necessarily penned by great men. His task consists in bridging these works together, preferably in a new and clever way, for public performance plays a great role in the practice of intellectual history. However, as Foucault has shown, the books, texts and works which constitute the foundation of such an exercise are very fragile indeed, and a reconstruction of the past cannot rest on such casual principles of selection.

Having criticised the traditional units organising the discourse of historians of ideas, Foucault is then faced with the problem of elaborating an alternative to them. To this end, Foucault devotes a great deal of space, nearly a third of the *Archeology of Knowledge*,[20] and, consequently, following him in all the meanders of his argument would lead us too far afield. Nevertheless, it will be useful to depict the units searched for by an archeology of knowledge, however sketchily, to provide a full view of Foucault's method.

The task of the archeologist consists in *describing enunciations* (*énoncés*). But what does Foucault mean by *describing* and by *enunciations?* Let us turn to the question of enunciations first.

To think about enunciations as *units* leads to thinking about them as *atoms* of discourse.[21] As such, they can be compared to similar units of discourse constructed from the vantage point of various disciplines such as logic, grammar and linguistics. In so doing, Foucault achieves two ends at once: on the one hand, he specifies the nature of enunciations by underlining what they are not; on the other hand, he demonstrates the autonomy of the archeological approach by showing that it cannot be reduced to any of the discourses of potentially competing disciplines.

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[20] *Ibid.*, pp. 44-173

[21] *Ibid*, p. 107

After having shown that enunciations are not logical propositions, grammatical sentences or, finally, speech acts, Foucault is led to conclude that enunciations do not exist at the same level as language.[22] But this discovery leads to a new question: the signs used to represent a language graphically do not exist at the same level as language either: where do enunciations situate themselves between signs and language? Could it be that enunciations are but the graphic or *material* precondition of language?

Foucault responds to this question by pointing out that if the material *types* found on a typewriter are but material instruments which allow the transformation of language into a material object, on the other hand the series Q, W, E, R, T, Y, found in a typewriter manual, is the enunciation of the alphabetical *order* adopted by typewriters in the English-speaking world.[23] As a result, an enunciation is neither entirely a linguistic entity (as propositions, sentences and speech acts are) nor a completely material object as are the lead types used in the printing process. The enunciation is not a *structure* either; that is to say, it is not a set of relationships between variable elements which could yield a number, perhaps an infinite number, of concrete models. Instead, *it is that which allows justifying the presence of certain signs among others, their inter-relationships, their referents, and whether they have meaning or not.* Moreover, and this is crucial for an understanding of Foucault's approach, enunciations are that which allow seeing what kind of *act* is involved in the very (written or oral) formulation of a sign.[24]

A last detail merits being underlined: I wrote that the enunciation *allowed* understanding a number of variables ascribed to signs; but what is involved in this process of *allowing*? How does the archeologist function, in short? Foucault's answer is clear and will be important for the conclusion of this paper: the archeologist acts by *analysis or by intuition.*[25]

In conclusion, enunciations are a function of existence, to use Foucault's very words; that is to say, enunciations act like a mathematical function, transforming the set of all signs into a set of allowable signs which, thereby, take meaning (in the case of sentences) or possess truth (in the case of logical propositions).[26]

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[22] *Ibid.*, p. 112 and p. 113.

[23] *Ibid*, p. 114

[24] *Ibid.*, p. 115

[25] *Ibid.* Later on, in the *Archeology* . . . , Foucault writes: "It is just so that I have never presented archeology as a science or even as the first foundation of a future science" *Ibid*, p 269

After having dealt with enunciations, Foucault proceeds to describe the enunciative function just outlined and maps out the result of what is allowed by enunciations—namely, discursive configurations.[27] This step finally leads Foucault to the important notion of *discursive practice* which he defines as follows: "It is a set of anonymous and historical rules, always specific as to time and place, and which, for a given period and within a social, economic, geographic or linguistic zone, define the framework within which the enunciative functions are exercised."[28]

Such are, very roughly sketched, some of the constitutive concepts of archeology. Let us now turn to the aims of the archeological method, among which Foucault sees four fundamental ones.[29]

1. Archeology addresses itself to available discourses seen not as passive receptacles of information, but as *enunciative practices* submitted to rules of formation. Archeology does not deal with ideas or thought. It does not seek to interpret documents as signs pointing to something else. Rather, it reads texts as if they were monuments, i.e. objects amenable to systematic descriptions. In so doing, it deals with the materiality of the text, itself made up of two parts: linguistic materiality and physical appearance.

2. Archeology tries to identify the rules which have guided the elaboration of the discourse under examination (as an archeologist, in the more common sense of the word, would examine the architectural practices which presided over the construction of a temple for example). One of Foucault's goals consists in seeing whether the set of rules corresponding to a given discourse is irreducible to the set of rules ascribed to another discourse. "Archeology," as Foucault puts it, "is a differential analysis of the modalities of discourse."

3. Archeology refuses to be an "anthropology (in the etymological meaning of the word) of creation." By this, Foucault means to refuse the projects of explaining the creation of a work by appealing to psychology, sociology and all the other available sciences of man. For one thing, the unit

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[26] *Ibid* , p. 115.

[27] *Ibid* , pp. 139-154

[28] I.e. rules which belong to the domain of history, not rules which determine the historical process *Ibid* , pp. 153-154.

[29] *Ibid.,* The four points which follow are taken from *ibid* , pp. 182-183.

work is not very secure, as we have already seen. Secondly, the sciences of man, according to his own analysis in *The Order of Things,* are but the transient consequences of a peculiar cultural structure, or epistemic configuration, to use his own concepts. They constitute, therefore, a very unreliable foundation for any theoretical analysis.

4. Archeology is the systematic description of discourse *qua* object. It refuses to deal with what may have been thought, wanted, aimed at or desired by men contemporary to the examined discourse. It only deals with discourse as a material object produced by certain types of human activities.

From this capsule description of the aims of archeology arises an obsessive theme: discourse is taken as a thing, resulting from a practice, and nothing else. Here is certainly a very unusual approach to the past, and a justification for such extraordinary steps must be discovered.

The answer, I believe, is linked with my earlier remark that, *stricto sensu,* ideas do not possess a history. This consciously provocative statement can now be dissected in the following manner: a text does not embody ideas; it embodies words. Ideas originate not in the text, but in the brain of the reader. The confrontation of the reading activity of a man with a text gives rise to ideas; but once the activity itself ceases, once the text returns to its shelf, it does not possess ideas, only its own materiality.

Although not alone in this regard, historians have entertained an extraordinary illusion for many years, not to say decades. With an obstinacy admirable in almost any other circumstances, they tend to believe that the text they pull out of archives acts as a neutral vessel filled to the brim with *ideas*. With an honourable, but unrealistic humility, they have seen their task as the transfer of ideas from texts to other texts (their own) via their brains. In effect, they pour some ideas from one series of texts into another. And as sometimes the archival vessel is a little rusty and hard to open up, they display a good deal of ingenuity to extract the ideas from it all the same. In the process, they look right through the vessel itself and, in effect, refuse to see it. Historians, in other words, spend an enormous amount of time (and of money) *not to see* what the great libraries of the world have kept in store for them. If historians drove their cars as they read documents, historical problems would be quickly solved *faute de combattants*. Foucault simply says: please open your eyes at long last and see.

The goals of the archeological method already point out how different it is from the history of ideas; its modes of analysis underline this contrast even more. In fact, the archeology of Foucault does not simply differ from the history of ideas: it actually stands in self-conscious opposition to it.[30] Foucault himself sees this opposition revolve around four broad questions: (a) the role and place of novelty; (b) the analysis of contradictions; (c) comparative descriptions; (d) identification of transformations.

When it comes to analysing what is new, the history of ideas performs what amounts to two contradictory tasks. It attempts to identify what is new, but immediately proceeds to discover the causes which must stand behind this apparent emergence. However, causes allegedly leading to the emergence of new ideas are exactly what prevents these ideas from being really new.[31] Archeology, on the other hand, refuses to deal with the question of novelty as such. Instead, it focuses on the *regularity* (i.e. the obedience to specified rules of formation) of given discourses, be they second or third zone text, or, on the contrary, so-called major discoveries. As Foucault puts it, and I will let the reader judge his mood: "What archeology looks for in the texts of Linnaeus or of Buffon, of Petty or of Ricardo, of Pinel or of Bichat is not aimed at devising the list of the holy founding fathers; instead, it aims at unveiling the regularity of a *discursive practice*" (my emphasis).[32]

Since Hegel and Marx, contradictions are at the centre of historical writing. But not so for the history of ideas. To contradictions, the history of ideas responds in two ways. In a first move, it denies their existence:[33] If texts display contradictions, such as incoherent use of vocabulary or incompatible propositions, the historian attempts to find an angle from which the contradiction disappears. But, in a second phase, if the contradiction has survived this first line of work, then it suddenly takes on a new and fundamental role: it becomes the very basis of the discourse under scrutiny. By their very polarity, the contradictory elements are then viewed as the agents manufacturing in effect a conceptual space which makes the discourse under scrutiny possible in the first place. The text then becomes part of a debate, sometimes also known as a crisis, a term used fondly by historians who differ as much as P. Hazard and T. S. Kuhn.

Contradictions are, of course, disquieting for any historian engaged in the task of understanding the past. It is not surprising, therefore, to see the history of ideas either refuse the existence of contradictions or relegate them to such a deep level as to forfeit analysis altogether. Archeology, on the other hand, fully accepts the fact that contradictions, like discontinuities, do not make sense. Instead of trying to create sense at all cost, archeology makes two important points: first, ideas do not generate ideas, with or without dialectics:[34] second, contradictions must be treated in themselves, as objects to be described.[35] In short, archeology explores the different types of contradictions in terms of the levels at which they affect the constitutive elements of discourse.

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[30] *Ibid.*, p 181.
[31] *Ibid.*, pp. 184-188
[32] *Ibid.*, p 189.
[33] *Ibid.*, pp. 195-198.

One of the constitutive units of analysis acceptable to the archeological method is the concept.[36] This unit is interesting not only because it can be a potential site for contradictions, but also because its presence seems in itself to contradict many of the tenets of the method devised by Foucault. Are concepts so very different from ideas? And if not, what place can they occupy in Foucault's archeology?

Not surprisingly, Foucault is led to recast the notion of concept. To do so, he appeals once again to his fundamental notion of discursive practice. Within this framework, concepts are no longer the organising principles of discourse; rather they are small discursive elements dispersed within a given discourse according to the inherent rules attached to a specific writing practice. Contradictions between concepts arise when their relationship with their abstract referent conflicts. In other words, concepts can contradict each other at the level of meaning. However, the way in which these concepts function within the discourse may be perfectly equivalent, despite contradictory meaning. Between the discursive practice itself and the concepts, there exists a preconceptual level made up of rules of formation and of distribution of concepts. Thus *attribution, articulation, designation* and *derivation* form the preconceptual schemes identified by the archeologist in his analysis of general grammar. They allow him to describe how different grammatical analyses are made possible in the seventeenth and eighteenth centuries; how an activity like grammatical analysis can define a domain of validity for itself; how it also creates a normative domain allowing the inclusion and the exclusion of a number of enunciations, etc.[37] The kinds of contradiction which may arise between some of the concepts of general grammar can then be gauged by reference to this preconceptual level constructed by the archeologist.

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[34] A dialectical process cannot be applied to ideas, the archeologist argues, because ideas have no tangible reality of their own. This does not mean that Foucault refuses to use dialectics It only means that he will insist upon dialectics being applied to real objects, thereby doing to historians of ideas what Marx did to Hegel

[35] *Ibid.*, p. 198.

[36] *ibid.*, pp 198-204.

[37] *Ibid.*, pp. 80-84. Enunciations are the fundamental new units constructed by Foucault and on which the archeological method rests. On the *énoncé*, see *ibid.*, pp 105-65

After novelty and contradictions come comparisons: there again the history of ideas differs markedly from the archeological method.[38] Moreover, this topic is particularly important for Foucault's own work as it provides him with an opportunity to correct either misinterpretations of his *Order of Things* or overenthusiatic formulations found within it. In the history of ideas, the comparison of two ideas always tends to support either the theme of continuity in time or that of diffusion in space. Not so in archeology; instead, comparisons are ways to *confront* simultaneous discursive practices in order to examine whether they maintain between themselves a number of relationships amenable to systematic description.[39] In no case does this project attempt to salvage notions such as *worldviews, spirits of the age,* or similar metaphysical entities. Where history of ideas tries to "smooth out" the historical landscape, archeology, on the contrary, tends to sharpen its asperities.

Quite often, the comparison of two ideas leads the historian of ideas to construct a causal sequence leading from idea A to idea B. Rather than looking for causes, the archeologist, although this is not a necessary part of his project, may try to see how the rules which preside over the formation of discourses may be linked with non-discursive systems such as institutions, political events, economic processes and the like. However, the discursive and non-discursive levels can never be mediated by a consciousness, for that would undermine the whole archeological project once and for all. For example, Foucault argues that the political life of the French Revolution did not determine the meaning and the form of revolutionary medicine; instead, the political practices of the Revolution must be analysed as conditions which make possible the emergence of a specific medical discourse; instead again, the archeologist looks for a specific mode of relationship which can be described in time and space, and it is his task to identify this relationship, specify its points of application in both the discursive and the non-discursive areas; in short describe its modalities. Thanks to this crucial development, Foucault can confidently refute the notion sometimes ascribed to him that discourses are purely ideal or formal entities, independent of history. On the contrary, he argues, archeology is an integral part of a general history yet to be constituted.[40]

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[38] *Ibid* , pp. 205-15.

[39] *Ibid* , p 206

[40] *Ibid* , p 215 General history must be contrasted with global history in that the latter attempts to create either a unifying or a totalising system. General history, on the contrary, respect specificities of situations,

The appearance of *The Order of Things* created many controversies, not least of which being the enigmatic way in which discursive configurations found themselves abruptly transformed from one moment to the next. In that book, the reader was confronted with three domains of study, natural history, study of wealth and general grammar. Suddenly, and apparently inexplicably, these three areas of knowledge transformed themselves or found themselves transformed into respectively biology, economics and historical linguistics. The simultaneity of these three conversions, moreover, could and did induce some to believe that whole cultures were abruptly appearing and disappearing.

In a chapter on change and transformation,[41] Foucault clarifies both the fundamental character and the limitations inherent to these abrupt discontinuities so often present in his other books as well. Limitation: the discontinuities unveiled by Foucault, far from affecting whole cultures, only touch the exact zone circumscribed by his analyses. They should not be extrapolated to whole cultures. Limitation again: the discontinuities affect only the archeological level of analysis and no other level. Thus the discontinuities described by Foucault are discontinuities in the discursive practices being scrutinised. But while a discursive practice may change, other elements of the discourse may remain the same. As an example, a change in discursive practice does not necessarily coincide with a shift in language, although the converse is often true.

It must be noted that the historian of ideas does not explain discontinuities any more than does the archeologist. He either erases them or places them at such a fundamental level that they become the uncaused cause of a whole set of historical events (as in the case of revolutions as this term is used in that discipline). The archeologist, on the other hand, identifies discontinuities and describes them. Only by moving from the discursive level to the non-discursive level can he hope to insert this systematic description of the discontinuity within a more encompassing system of description which might play a role analogous to that of cause. But cause is here taken in a sense both more general and more abstract than is usually the case. Causes are meant to explain. In Foucault's archeology, explaining means inserting a descriptive system into a more encompassing descriptive system. Finally, it must be remembered that, for the archeologist, discontinuities act as

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and does not impose a *priori* limits on the number of relevant variables. In short, its conceptual tools are as open-ended as the field it claims stakes for—namely all men from all times and all areas.

[41] *Ibid.*, pp. 216-231

safeguards against such mystifying concepts as progress, human nature and so on, all concepts constitutive of a mystifying history.

In summary, the enigmatic nature of the transformations witnessed in the archeological descriptions is a product of the boundaries imposed by the archeologist to his own field of investigation. Thus, in *The Order of Things* transformations do just take place without any explanation or apology as to why; but this is due to Foucault's limiting himself strictly to a discursive domain. Had he chosen to introduce non-discursive elements in his analysis of the three domains of knowledge which concern him in that book, he would have explained how such non-discursive factors as the French Revolution could lead to historical junctions where some kinds of discourses are no longer possible (disappearance or reorganisation of institutions, new forms of education, new classes of population affected by the form of knowledge under scrutiny, etc.)

Foucault's analysis of change and transformation does something more as well. By identifying areas of discourse whose practice remains unchanged for a number of years or even decades, he brings to light the non-homogeneous nature of time as it functions within a particular discursive zone. In other words, instead of assuming that historical events are legislated by the flow of homogeneous time, Foucault demonstrates that certain events keep on repeating themselves for a while before disappearing to leave room for new categories of events. Segments of duration (or temporality, to use a more standard terminology now current in French historical writing at least), therefore, come to replace the even ticking away of Newtonian time. Why did historians subscribe to a notion of homogeneous time? Foucault ascribes this illusion to anthropomorphism: the linear structure of spoken language in time and the self-consciousness of a stream of thoughts are both unconsciously projected on outside reality.[42]

The aims and the methods of Foucault's archeology are bound to lead to strange new panoramas, very different from those usually revealed by the history of ideas, and this is indeed what Foucault's separate studies achieve. In particular, in the place of the familiar history of science, an archeology of knowledge has emerged. If archeology is to replace history, is knowledge to replace science?[43] And if so, what are the implications for the status of both science and knowledge? What can be known, and how secure is that knowledge? Also, what is knowledge for?

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[42] *Ibid.*, p 220

[43] The French words *connaissance* and *savoir* are both rendered into English by knowledge However, Foucault reserves the former for the constitutive activity of a consciousness (knowing subject), while the latter is reserved for archeological knowledge. Were it not for unfortunate technical connotations, these two terms might be rendered fairly adequately by knowledge and know-how respectively.

For the historian, knowledge and science differ because the canons of arguments which obtain in each domain differ as well. Arguments used in the case of knowledge may not require the empirical data scientific argument would; or perhaps they obey a less rigorous logic or a less exacting testing procedure; or they may even be so different as to defy comparison. Whatever the case may be, for the historian, science generally remains the fundamental yardstick by which to judge and evaluate all other forms of knowledge. At the same time, the distance which separates science from knowledge can be bridged by a series of transformations affecting the constitutive elements of knowledge and allowing it to accede to the status of science.[44]

For the archeologist, knowledge is merely what can be rendered by language as modulated by a given discursive practice which, thereby, finds itself specified.[45] Knowledge, in other words, is the ability to say something about certain objects, be they material or conceptual objects. This ability, needless to add, is not a creative characteristic attributable to an individual, but refers instead to the rules embedded in the discursive practice itself. The only other requirement to add is that knowledge corresponds to an *ordered* discursive practice, order susceptible of being described by the archeologist. Finally and most crucially, knowledge is further characterised by its potential for use and by the ways in which it can be appropriated by certain groups for certain ends.

The passage from knowledge to science does not correspond to the transition from non-truth or half-truth to truth; instead, the archeologist considers that the transition to science simply corresponds to an intensification within a given discursive practice.[46] What is being intensified are descriptive variables such as the density of a conceptual network, the precision with which these concepts are defined and the degree to which they are criticised, the degree of formalisation and so on. The intensification in practice corresponding to the establishment of a peculiar science leads to specific relationships between knowledge and science, and it is exactly at this junction that ideology affects the scientific discourse; it is also at this point that science can play its own ideological role.[47] In other words, when science arrogates to itself the right to judge all other forms of knowledge, it can, in the same movement, serve ideological aims which reach far beyond the quest for truth which provides its ostensible goals.

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[44] This transformation has been the object of repeated and detailed studies by Gaston Bachelard who also introduced the idea of an epistemological threshold, about which more later.

[45] *Ibid.*, p. 238. The different characteristics of *savoir* are found on this page and following one in *L'Archéologie* . .

[46] This point does raise a problem which can only be mentioned here—namely whether an intensification of certain aspects of a given discursive practice (such as greater systematicity, more formalised concepts and strategies, etc.) does not really amount to the creation of a *different* discursive practice

This analysis leads Foucault to support the thesis that, far from being mutually exclusive, science and ideology are indissolubly bound together.[48] However paradoxical an expression like "scientific ideology" may look or sound, it does make sense in the context of archeology, if it is remembered that science cannot claim anything more (and anything less, for that matter) than being one of the products among many of a given discursive practice. Indeed, from the angle of a practice, it would be difficult to see how any discourse could claim any kind of pristine purity, the claim itself being clearly ideological in nature; but I shall come back to this point later.

The description and classification of discursive practices are performed by the archeologist in terms of a number of reference points or thresholds; together they specify the stage-like mode in which the scrutinised text is functioning. A threshold of *positivity* is reached whenever a discursive practice becomes individualised among others.[49] An *epistemological* threshold is reached in turn when some of the units of discourse start to organise themselves coherently and start acting as standards for the corresponding form of knowledge. Other thresholds such as scientific and formal thresholds correspond to other configurations of the discourse analysed by the archeologist.[50] The specific temporal order in which they appear is not constant; the steps which lead from one threshold to the next vary from one form of discourse to another. In fact, answering these questions constitutes one of the major tasks of the archeologist.[51] At the same time, these thresholds are not meant to distinguish knowledge from science; rather, they determine the kinds of relationships existing between the two, with ideology always present as the mediating factor.

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[47] *Ibid.*, p. 241.

[48] *Ibid.*, p. 242. On this topic, see also G. Canguilhem, "Qu'est-ce qu'une idéologie scientifique? "*Organon*, 1970, 7: 3-13, and J. Hyppolite, "Le Scientifique et l'idéologique dans une perspective marxiste," in his *Figures de la pensée philosophique* (Paris: Presses universitaires de France, 1971), pp. 360-71.

[49] *L'Archéologie du savoir, op. cit.* (n. 12 above), p. 243

[50] *Ibid.*, p. 244.

[51] Foucault's emphasis on the specificity of each situation clearly shows how intent he is on correcting some impressions drawn by a number of commentators from his *Les Mots et les choses*, and in particular the notion that he had been attempting to describe cultural totalities. See p. 271 below.

Up to this point, I have mainly attempted to demonstrate that Foucault's way of raising questions was not only significant, but also very different from the approaches generally used by historians of ideas. However, there remains an important question to be raised, one suggested by Foucault's own method—namely, how legitimate is Foucault's approach? In other words, what epistemic foundations allow Foucault to speak as he does?

As could be expected, the answer to such a question cannot be found entirely in Foucault's writings, at least not explicitly. In fact, at the end of his *Archeology of Knowledge*, Foucault expressly states that his "discourse, far from specifying the locus from which it speaks, avoids the ground on which it could lean."[52] And indeed, this is a necessary precaution on the part of Foucault, for otherwise his analyses would find themselves co-opted by the very disciplines or, more generally speaking, discourses using the same ground as his. As a result, his critical explorations would transform themselves into the fitting of "facts" into pre-ordained slots, and his whole quest, instead of opening new ground, would simply tend to validate pre-existing discursive strategies.

This said, it is nevertheless possible to note that Foucault's line of questioning intersects with a number of problems raised in other contexts. When archeology, for instance, tries to unveil the kinds of relationships linking subjects with enunciations, it encounters a problem already present in psychoanalysis. But archeology also crosses the path of epistemology when it attempts to specify the rules regulating the formation of concepts; and it also meets the question of social configurations when it deals with the formation of objects—scientific objects, that is—and their emergence within specific fields and institutions.[53] Yet, archeology does not coincide with any of these questions, for the general theory it seeks cannot be incorporated in a general psychoanalytical theory, or a theory of epistemology or of sociology; instead, it looks for a problematic *general theory of production*.[54]

It may sound paradoxical to say that the overall theory of archeology should be a general theory of production, when it is well known that Foucault focuses much of his attention on problems like rules of formation of types of discourses, possibility of existence of the same, etc. Such problems do lend themselves to being solved by concepts like repetition, duplication and so on. In other words, it would seem that Foucault is led to create a vast generative machine allowing discourses to reproduce over and

[52] *Ibid.*, p 267

[53] *Ibid*, p 170.

[54] *Ibid.*

over again, and from within.[55] In other words again, Foucault's questions seem to preclude either the possibility of change, or understanding why change appears when it does; it seems to allow for a proliferation of similar individual discursive practices, and only that.

The metaphor of the generative machine, however, is quite misleading in that it brings to mind a false connotation—namely that of such a dynamic necessity that producing a discourse would ultimately seem to be easier than not producing it at all. A discursive configuration would then appear to function in either a mechanical or compulsive fashion. But this metaphor is not Foucault's; actually, he argues exactly the contrary: to speak, for him, is to act, and the process of production is always complex and costly.[56]

The metaphor of the generative machine is misleading in yet another way. Far from denying the possibility of change, Foucault, in point of fact, shows and even underlines the importance of change: the order of a "family" of discourses can be modified, transformed and, to use the word, revolutionised. And precisely because archeology has focused on the very element constitutive of the order of discourse, it is in an excellent position to evaluate the specificities of a given transformation and the extent to which it affects a given kind of discourse. As a result, archeology can describe change more precisely than any method based on the vague problematics of the history of ideas; moreover, it does so while rejecting any appeal to organising principles such as new ideas, creativity or mentality. Instead, it looks at the transformations taking place within a given practice, or within neighbouring practices.[57]

The paradox does not stop here: not only is archeology capable of dealing with change better than history of ideas, despite all appearances, but it is in fact difficult to see how history of ideas can analyse or even describe change, except through the manipulation of mythical entities. In the end, far from being on the defensive, Foucault can turn against the opposing historian and ask in his turn: "What is this fear which makes you answer in terms of consciousness when you are addressed in terms of practice, of its conditions, of its historical transformations? What is this fear which . . . makes you look for the great historico-transcendental destiny of the West?"[58] His answer is brief and rather significant by the different dimension it injects into the debate: this fear is *political* in nature.[59] At this point, Foucault has actually identified the locus which he had so painstakingly avoided before. The question which remains now is the following: how did he find himself speaking from a political stance? What discursive practice made his own discourse appear in this particular mode?

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[55] For a brief, but significant, comparison between archeology and generative grammar, see *ibid.*, p 269-70.

[56] *Ibid*., p. 272

[57] *Ibid.*

[58] *Ibid.*, p. 273.

In accordance with his own tenets, Foucault opened his inaugural lecture at the *Collège de France* by invoking a voice, already there, and guiding his own modulations a little like a carrier wave in radio transmissions. That voice, in this particular instance, turned out to be an elegant reference to his predecessor at the Collège, Jean Hippolyte;[60] but in actuality, it is a Hippolyte speaking less by himself than being spoken through, for he too is subjected to the rules and regulations of discourse formation. In this particular case, Hippolyte is the man who speaks the crucial response to Hegel—namely, how is it still possible to do philosophy where Hegel is no longer possible?[61]

Hippolyte's quest is not specifically his, of course. His own discursive practice intersects with that of other individuals, among whom Marx and Nietzsche are most notorious. Without their prior philosophical interventions, Foucault's and Hippolyte's own discourses could not have emerged. Let us see how this is the case for Foucault.

In the case of Nietzsche, Foucault himself has outlined the connections running from Nietzsche's genealogy to his own archeology.[62] If Nietzsche's genealogy is not the twin of archeology, it nevertheless shares with the latter a common opposition to history. For example, genealogy refuses to grant any ideal meaning or teleological implications to a historical narrative.[63] Genealogy denies the validity of looking for the essence of things through an investigation of their origin; instead, genealogy demystifies history by showing either that historical objects have no essence, or that their apparent essence is not intrinsic to them, but, in fact, was constructed from elements foreign to them in the first place.[64] Genealogy also rejects the notion that a historian could monopolise a synthetic viewpoint permitting him to subsume the past or even part of it under a single, closed totality.[65] As an alternative, genealogy is attentive to the power relationships attending the emergence of certain kinds of events, including—and the notion of event is itself transformed in the process—values, morality, asceticism and knowledge.[66] As a result, and consistently enough, genealogy is also attentive to the constraints acting on genealogy itself,[67] but instead of hiding away the very ground on which he stands, the genealogist proclaims it, along with his denying the possibility of neutral objectivity: " . . . the question is no longer to judge our past in the name of a truth owned only by our present; the question is to risk the destruction of the knowing subject in an infinitely deployed *will to know*" (my emphasis).[68]

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[59] *Ibid*

[60] Michel Foucault, *L'Ordre du discours* (Paris. Gallimard, 1971), *passim*.

[61] *Ibid.*, pp. 74-76.

[62] Michel Foucault, "Nietzsche, la généalogie, l'histoire," in *Hommage à Jean Hyppolite* (Paris, PUF, 1971), pp 145-172

[63] *Ibid.*, p. 146.

[64] *Ibid* , p 148 and p 150.

[65] *Ibid.*, p 159

Thematic affinities between genealogy and archeology must be obvious by now, but this does not mean that Nietzsche should be viewed as a precursor of Foucault. What is at stake is far more fundamental than just a filiation of ideas between two thinkers separated by some eighty years of history; what is at stake is the existence of a mode of functioning of signs first encountered in Nietzsche, Marx and Freud and underpinning in a complex way Foucault's own work.[69] In effect, signs have undergone a radical displacement with these three thinkers: instead of being passive objects offered to interpretative rituals, signs are actually already interpretative objects in themselves. In a parody of Sartre's famous claim that existence precedes essence, Foucault sees our historical period as one in which "interpretation precedes signs."[70] Moreover, signs are interpretations attempting to justify themselves.[71] Thus, money gives itself as the sign of value, but Marx shows in *Capital* that taking money literally at its face-value is to play into the hands of the capitalists since this conception of money occults the process by which surplus-value is extracted from the proletariat. A similar analysis could be carried out in the case of Freud's interpretation of dreams. Finally, Nietzsche argues in the *Genealogy of Morals* that the idea of good "did not originate among those to whom goodness was shown. Much rather has it been the good themselves, that is the aristocratic, the powerful, the high stationed, the high-minded, who have felt that they themselves were good. . . . It was out of this pathos of distance that they first arrogated to themselves the right to create values for their own profit, and to coin the names of such values."[72] Clearly, Nietzsche presents an argument where words already contain in themselves an interpretation of societal order.

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[66] *Ibid.*, p 156, p. 161. It is clear that in unravelling the processes producing events like morality, Nietzsche undermines their ability to organise a societal ethos, by showing that far from being universal and eternal categories, they actually belong to specific spatio-temporal units with their attendant societal instances and their modes of relationship

[67] *Ibid.*, p 163, p. 165.

[68] *Ibid.*, p. 172

[69] Michel Foucault, "Nietzsche, Freud, Marx," in *Nietzsche* (Paris Editions de Minuit, 1967), pp. 183-192. My thanks to Camille Limoges for having drawn my attention to this early but important text by Foucault (1964).

[70] *Ibid.*, p. 190.

[71] *Ibid*, p. 191.

It is relatively simple to understand now how the *Archeology of Knowledge* becomes possible: it used to be that the surface of things was duplicated by the surface of signs, and it also used to be that signs were viewed as the trustworthy and neutral tool needed to apprehend things fully. But now, the sign takes on a much more complex appearance as the surface carrying it transforms itself, as it were, into a volume made up of an infinite number of layers piled up on top of each other. What has happened is that the couplet "interpretation-sign," along with the modalities guiding the relationship between interpretation and sign, are now offered simultaneously as a new object-to-be-interpreted. And the process can repeat itself indefinitely. But Foucault refuses to be caught up into this never-ending process characteristic of hermeneutics; and he also refuses to move to the position consisting of the negation of hermeneutics—that being the systematic description of signs and of their inter-relationships, both being apprehended from the outside. The latter attitude rests on the assumption that signs exist in themselves and that, as such, they are the proper objects of study of another systematic analysis—namely semiology. Instead, Foucault opts for a third solution, one resting on the same epistemic ground as both hermeneutics and semiology: he retains the semiological project of a systematic description of external relationships, but instead of applying it to signs only, he focuses on both signs *and* interpretations. Between the sign as *interpretans* and the sign as *interpretandum*, Foucault discovers, or rather constructs, the locus for the archeological description of discourse.[73]

There is another way to formulate this result which may be more telling. Treating signs as neutral reflections of natural objects is to eliminate language in favour of epistemology. Consequently, the archeological approach works in the space between semiology, hermeneutics and epistemology. And because archeology can function effectively between these three domains, it can act as a meta-discourse for them. In other words, archeology, by its very locus, can claim a critical role with respect to semiology, epistemology and hermeneutics.

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[72] F. Nietzsche, *The Genealogy of Morals* (London: T.N Foulis, 1913), Transl H B Samuels, pp 19-20.

[73] Foucault, *op cit* (n 61 above), p. 192

In many ways, the purest attempt to make archeology work exactly as was just described is found in *The Order of Things*. That is to say that *The Order of Things* is, paradoxically, Foucault's least successful and most successful book. It is most successful in that it comes closest to a complete archeological description of several contemporary discourses, showing in the process that isomorphic configurations can obtain in widely varied domains like economics and biology. It is least successful in that it ultimately remains simply descriptive, even though this limited result is masked by the great originality of the description and by its scandalous outcome—namely that the object "man," as an object of the so-called human sciences, is a very recent object, bound to disappear in the near future. In the end, archeology could perhaps be critical of semiology, epistemology and hermeneutics, but it could achieve its critical aims only in one way—i.e. by exhibiting cultural objects constructed by the archeological approach. How these enigmatic objects could appear, disappear or even maintain themselves was left in the realm of mysteries. To put it otherwise, in *The Order of Things,* Foucault had succeeded in identifying an interesting strategic locus for a critical approach, but he had done so at a terrible cost: he could no longer understand the process of emergence of what he was exhibiting.

The Archeology of Knowledge can be viewed as Foucault's exploration of the limits of archeological descriptions; but it can also be viewed as the exploration of the non-discursive factors contributing to the production of certain types of discourses. In other words, this book displays a very conscious effort to reorganise the discoveries made in *Madness and Civilisation, Birth of the Clinic* and *The Order of Things* and fit them within a coherent scheme. That scheme, while retaining the acquisitions of the archeological method, also introduces the idea of a production of the discursive level by the non-discursive level.

The phrase "production of the discursive level by the non-discursive level" can be misleading if interpreted simply mechanistically or even as a simple causal relationship. Actually, the process works both ways and is a good deal more complex than a causal relationship, as Foucault explains clearly in his introduction to the publishing of a series of historical documents pertaining to an early nineteenth-century mass murder.[74]

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[74] This in itself is extraordinary, given that Pierre Rivière claimed that he was hardly able to read or write See *Moi, Pierre Rivière, ayant égorgé ma mère, ma soeur et mon frère—un cas de parricide au XIX^{e}siècle* (Paris Archives Gallimard, Julliard, 1973), p 266

> Documents such as those of the Rivière affair should allow analysing the formation and the interplay of a form of knowledge (such as medical, psychiatric or psychopathological knowledge) with institutions, as well as the roles prescribed by them (judicial institutions with their experts, accused and criminals, etc.).
>
> They allow decyphering the relationships based on power, dominance and struggle, relationships within which discourses are formed and function; they allow, therefore, the analysis of discourses (including even scientific discourses) both at the level of events and of politics, and, therefore, at the level of strategies.[75]

In this quotation, Foucault is seen as proceeding in two phases: first, starting from documents, as any historian would do, he notes the steps by which certain forms of knowledge are created. In this first phase, he applies an archeological mode of reading[76] leading to an archeological *description* of these documents.

In the second phase, Foucault does not deal with documents any more, i.e. the raw materials piled up any which way in libraries; instead, he deals with discourses and examines how they are produced. This he does by treating the non-discursive realm as a system of signs located in the specific social relationships unveiled beyond the edge of the discursive domain.[77] These relationships are few and simple: they are power and dominance relationships and they bear the mark of social struggles.

Treating the social arena in terms of struggle is not a new theme introduced for the first time by Foucault. But if this point is obvious, it would be equally wrong to say that Foucault simply followed in the footsteps of Hobbes, via Nietzsche or Marx. In Foucault the struggling elements are neither individuals, nor classes: either solution would reintroduce the kinds of questions solved by the introduction of an active subject. Instead, and this is where Marx becomes crucial for Foucault's recent studies, the struggle is grounded in the relationship between the productive forces. But, unlike Marx, Foucault does not see the relations of production as being sufficient to account for the proliferation of diversified discourses at different levels. Something else is needed—namely the concept of power, itself accompanied by a series of concrete events subsumed by Foucault under the term: technologies of power.

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[75] *Ibid.*, p. 13. Strategies are not to be construed as the expression of the creative powers of an individual; instead they refer to a complex, trans-individual subject made up by the intersection of social, economic, political and libidinal instances Strategies from the conditions of possibility for particular discursive practices.

[76] On theories of reading, consult Karel Williams, "Unproblematic archeology," *Economy and Society*, 1974, *3* (1), 41-68. In this article, Williams tries to demonstrate that Foucault's Archeology is an attempt to construct a new form of reading, but that it does not stand up to scrutiny due to internal incoherence

[77] G Canguilhem, *Le Normal et le pathologique* (Paris. Presses Universitaires de France, 1972), 2nd edition, p. 150. This book dates originally from 1943. M. Foucault, *Naissance de la clinique* (Paris, Presses Universitaires de France, 1972), 2nd edition, p. XV. The analogy between Canguilhem's stance and Foucault's is not just chance. Foucault attended a number of Canguilhem's seminars in the earlier part of his career.

In other words, if Marx is crucial to account for Foucault's recent orientation, he is not sufficient. And if he is not sufficient, it is because Foucault cannot treat a social relationship as a fact. Once again, the treatment of signs which opened up the space for an archeological description of documents, also informs the decyphering of social relationships. Indeed, social relationships are signs, but like all signs, their very existence points to their already present interpretative capacity. Power is already informing the sign although it may give itself the appearance of a simple social relationship.

In summary, the theory of signs implicitly used by Foucault to open up a space for archeology is found to function again at the non-discursive level. The phrase "technology of power" becomes significant in this respect, for technology is both a technique of power and a reflexion about such a technique: it is at once theory and practice. And it is because of its being both that it can mediate the non-discursive level with its discursive counterpart.

* * *

Foucault's later book, *Surveiller et punir,* ends on a grave note: ". . . it is necessary," he tells us, "to listen to the roar of battle."[78] Yet early signs indicated that his meandering path would ultimately lead him to this conclusion.[79]

Ironically, Foucault's treatment of texts as social acts, with all the consequences that such an interpretation carries with it, was not clearly perceived at first. On the Left, he was even flayed as a "desperate positivist" when his *Order of Things* appeared.[80] He himself admits that this particular book could lead to misinterpretations since "the absence of methodological references may have given the impression of analyses carried out in terms of cultural totalities."[81] Such criticisms, however, are no longer possible since the appearance of *The Archeology of Knowledge* and particularly *Surveiller et punir* with its scathing denunciation of the role prisons play in controlling whole populations.[82]

By 1972, Foucault was expressing very clearly what the role of an intellectual in society ought to be. For one thing, he says, "what the intellectuals have recently discovered . . . is that the masses don't need them in order to know. They [the masses] know pretty well and much better than the intellectuals—and they say it very well."[83] Given this state of affairs, Foucault argues that "the role of the intellectual" is no longer to place himself a "little ahead or a bit to the side" so as to speak the silent truth of all. Rather it is a struggle against the forms of power in relation to which he is both object and instrument: within the domain of 'knowledge,' 'truth,' 'consciousness' and 'discourse.' "[84]

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[78] *Surveiller et punir, op. cit.* (n. 13 above), p. 315

[79] *L'Archéologie du savoir, op cit* (note 12), p. 273.

[80] Sylvie LeBon, "Un positiviste désespéré, "*Les Temps modernes*, January 1967, 248· 1299-1319. See also John M. Moore's review of *The Order of Things*, *Science and Society*, Winter 1971, *35*. 490-4

[81] *L'Archéologie du savoir, op cit* (n. 12 above), p. 27.

[82] *Surveiller et punir, op cit.* (n. 13 above), pp. 282-90

As in the case of *Surveiller et punir,* Foucault ends his conception of theoretical activity on the note of battle: ". . . theory will never express, translate or apply a practice: it is a practice. . . . It is not a struggle for suddenly attaining consciousness (. . .) but for undermining and capturing authority with all those who struggle for it, and not by withdrawing in order to enlighten them. A 'theory' is the regional system of this struggle."[85]

It is interesting to contrast Foucault's recent pronouncements with one of the most elaborate analyses of his works yet to emanate from American academic circles—namely Hayden White's substantial article which appeared in 1973. One year after the 1972 interview, White could find himself writing: "Foucault's suspicion of reductionism in all its forms is manifested in his professed lack of interest in the relation of a work or a corpus of works to its social, economic and political contexts."[86] Instead, so White argues, Foucault believes he has invented a way of treating texts which is really nothing more than paying attention to the rhetorical dimensions of language—a dimension which was lost precisely when science emerged by disengaging itself from rhetoric and which has been obscured ever since because of science's dominant position in the general area of knowledge. But actually, White continues, Foucault only repeats Vico's own analysis based on the tropological structures of texts,[87] and he would be aware of it "if he understood correctly what he has been about."[88]

After dismissing Foucault in the above manner, White then shows that

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[83] "The intellectuals and power A discussion between Michel Foucault and Gilles Deleuze," *Telos,* Summer 1973, *16.* 104. This interview was done on March 4, 1972, and was first published in *L'Arc*

[84] *Ibid*

[85] *Ibid*

[86] Hayden V. White, "Foucault decoded. Notes from underground," *History and Theory,* 1973, *12* 30 White manages to contradict himself, however, when he later writes. "What caused this change? In Foucault's view, the change had very little to do with the advancement of theoretical knowledge about the true nature of mental illness Rather, If there was any advancement at all, it came as a result of *more basic transformations in society.*" *Ibid.,* 40 (my emphasis).

[87] *Ibid* , 48-49.

[88] *Ibid* , 49

Foucault's works cannot be expected to be read widely in the Anglo-American philosophical community. For one thing, he " . . . is a metaphysician, however much he may stress his descent from the positivist convention."[89] Besides, White adds, he "does not work within the mainstream of Western historiography."[90]

The reason why Foucault is not in the mainstream of Western historiography, according to White, is because, contrary to most historians, Foucault makes the past *unfamiliar to us*. Unwittingly, White may have a point here: Foucault is indeed trying to demystify history, very much as Marx or Nietzsche did. But perhaps they are not part of the mainstream of Western historiography either; nor are many members of the *Annales* school who would certainly endorse the notion of the unfamiliar in the past.

White could have argued, and indeed did argue, that Foucault is not a normal historian, whatever that may be, because of the way in which he uses (or fails to use) sources.[91] But this is hardly a good criticism for, if Foucault treats his sources differently, he does so in accord with his own explicit precepts, which is more than can be said of many historians. Moreover, he generally uses a range of sources sufficiently wide to satisfy even the most demanding historian.

The curious thing about White's criticism of Foucault is that one senses that the real point of contention between the two men is never addressed directly. In a way, saying that Foucault has done nothing more than pay attention to the rhetorical dimensions of language, and by adding that Foucault is not very conscious of that fact, White is stating that Foucault's *The Order of Things* is little more than a poor attempt at writing White's own *Metahistory*.[92] But

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[89] *Ibid* , 49

[90] *Ibid.*, 50.

[91] White has apparently not read or even looked at the long version of *Folie et déraison à l'âge classique*, two editions of which exist This careless attitude allows him to add one more preposterous statement to the long series which I have already referred the reader to in notes 87 through 90 He writes: "From a consideration of a very limited body of data, Foucault purported to contrive a true account of the 'underside' of thought about both 'reason' and 'madness,' ." *ibid* , 38 Far more to the point, Michael Peters writes: "The erudition, if one challenges its authenticity (e g on the selection of sources) is at least as authentic as anything one will find employed anywhere else to back up more orthodox theses in the history of ideas. The point of saying this is merely to emphasize that to find empirical inaccuracies or gaps in the picture Foucault offers of some four centuries of speculation on the subject of madness cannot be assumed to imply that the orthodox picture rests on any firmer evidence." Peters, *op. cit.* (n. 9 above), 637

[92] See Hayden V. White, *Metahistory—The Historical Imagination in Nineteenth Century Europe* (Baltimore: The Johns Hopkins Press, 1973) In that book White argues that historical discourse, unlike scientific discourse, is made possible by a pre-critical, pre-conscious, poetic act constructing a given historical field both in terms of the objects to be found in it and in terms of the tools used to analyse it. As a

Metahistory argues against history ever having any objective value; rather historians' strategies are dictated by ideology or aesthetics, and those two factors can be read off the rhetorical forms adopted by any given historian.

Clearly, White's approach stands in stark contrast to that of Foucault: while the latter sees the intellectual intervening in the political struggles of his own social sphere, the former writes a very large book so as to free the imagination of the historian. Where Foucault sees archeology as a social practice efficacious within specific social configurations (the intellectuals, the universities, etc.), White hopes to see the golden age of nineteenth-century history reappear so that the historian may have again full freedom to frolic among the four tropes, metaphor, metonymy, synechdoche, and irony.

In effect, White attacks Foucault, so it seems to me, because the latter thoroughly rejects the notion of an ivory tower, a last asylum for serenity and neutral, but elegant, objectivity. For Foucault, scholars who choose to treat their work as a sacrifice formally placed on the altar of truth and performed for its own sake, according to rules defined internally by the profession,[93] must be either deluded, or are perpetuating myths for reasons of their own. If they are deluding themselves, then it is time their eyes be opened. If they are creating myths, then these must be exposed for what they are. This is done by asking some very simple and straightforward questions like, Whom does value-free, neutral history serve? How is it allowed to maintain itself and, specifically, who finances it? These questions relate, painfully to be sure, to the academic institutions, their internal hierarchies, their role in society and particularly their clientele. Ultimately, all of these questions lead to a very fundamental one, rarely posed explicitly within the hushed walls of Academe: we are historians of science for what—and, more to the point, for whom?

The archeology of knowledge of Michel Foucault is a critique of the assumptions guiding the work of many historians of science. More fundamentally, however, it also confronts all of us at the level of values and of our

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consequence, White argues that there is no way of arguing in favour of this or that historical approach on epistemological grounds; only esthetics or ideology determines the choices used. Consequently, the value of historical discourses does not lie in their being good representations of reality. If histories cannot be judged in terms of their relationship with reality it seems strange to attack Foucault for his bad or insufficient use of sources; if historical writing stems from a pre-conscious act, it also seems strange to attack Foucault for his lack of lucidity This is why I have the feeling that these arguments are present only to mask the true stakes involved in White's attack to Foucault.

[93] In his article on History of Science, T. S. Kuhn, speaking undoubtedly for many of his colleagues, writes "Like any other scholarly discipline, the field's primary responsibility must be to itself," *The International Encyclopedia of the Social Sciences, op cit.* (n 11 above), vol. 14, p 81.

motivations for being historians of science. And I suspect that it is on this very point that Michel Foucault will be resisted most vociferously—that is if he can no longer be ignored.

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Appendix

In this appendix, the main works of Foucault, as well as some of the crucial secondary literature on him have been listed. This list is in no way construed to be exhaustive. It is only meant as a preliminary guide for further reading.

A. *Main works by Michel Foucault.*

1. *Maladie mentale et psychologie* (Paris: P.U.F., 1961). Several editions of this work have appeared subsequently.

2. *Folie et déraison: Histoire de la folie à l'âge classique* (Paris: Plon, 1961).

A second edition was issued with two additional texts (Paris: Gallimard, 1972). An abridged edition was printed under the title: *Histoire de la folie* (Paris: Collection 10-18, 1961). *Madness and Civilisation* is a translation of the abridged edition.

3. *Naissance de la clinique* (Paris: P.U.F., 1963). A second edition appeared in 1972.

4. *Raymond Roussel* (Paris: Gallimard, 1963).

5. *Les Mots et les choses* (Paris: Gallimard, 1966).

6. *L'Archéologie du savoir* (Paris: Gallimard, 1969).

7. *L'Ordre du discours* (Paris: Gallimard, 1971).

8. *Moi, Pierre Rivière, ayant égorgé ma mère, ma soeur et mon frère.*

Collective study under the direction of M. Foucault (Paris: Gallimard, 1973)

9. *Surveiller et punir. Naissance de la prison* (Paris: Gallimard, 1975).

10. *La volanté de savoir. Histoire de la sexualité* (Paris: Gallimard, 1976).

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3. Dominique Lecourt, *Pour une critique de l'épistémologie (Bachelard, Canguilhem, Foucault)* (Paris: Maspero, 1972).

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Michel Serres, "Géométrie de la folie," *Mercure de France,* August 1962, *1188:* 683-696; September 1962, *1189:* 68-81.

Special issue of *L'Evolution psychiatrique,* April-June 1971, *36,* 221-428.

2. *Birth of the Clinic.*

François Dagognet, *Critique,* 1965, *21:* 436-447.

3. *The Order of Things*

Pierre Burgelin, "L'Archéologie du savoir," *Esprit,* May 1967, *35:* 843-61.

Sylvie Le Bon, "Un positiviste désespéré," *Les Temps modernes,* January 1967, *22:* 1299-1319.

Michel de Certeau, "Les Sciences humaines et la mort de l'homme," *Etudes,* March 1967, *326:* 344-360.

G. Canguilhem, "Mort de l'homme ou épuisement du cogito," *Critique,* 1967, *24* (actually *23*): 599-618.

4. *The Archeology of Knowledge*

A. Krémer-Marietti, *Revue de métaphysique et de morale,* 1973, *36:* 495-502.

G. Deleuze, "Un nouvel archiviste," *Critique,* March 1970, *274:* 195-209.

F. Russo, "L'Archéologie du savoir de Michel Foucault," *Archives de philosophie,* 1973, *36:* 70-105.

5. *Surveiller et Punir*

G. Deleuze, "Ecrivain non: un nouveau cartographe," *Critique*, December 1975, *343:* 1207-1227.

François Ewald, "Anatomie et corps politique," *Critique*, December 1975, *343*, 1228-65.

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DOCTORS TO THE BARRICADES! AMERICAN PHYSICIANS AND THE BATTLE OF BRITAIN

E. H. BEARDSLEY

Historians, in explaining America's failure to lend greater aid to Great Britain in the early years of World War II (1939-41), have usually pointed to a combination of isolationist pressures and the restrictions of U.S. neutrality laws. While that assessment covers the ground adequately in most instances, the story of the Anglo-American effort of 1940 and 1941 to integrate U.S. physicians into Britain's civilian and military medical services suggests yet another barrier in the way of American aid. In the case of the volunteer American doctors, the limiting factor was the cultural and social differences between the two nations. What both sides failed to foresee was that in borrowing American doctors Britain also had to adjust to a whole range of peculiarly American characteristics, an adjustment that was always difficult and sometimes impossible to make.

As the "phoney war" ended and England's struggle began in earnest in June 1940, a number of American doctors began looking for ways to help. Some, no doubt, simply wanted to get into the fight, thereby ending what one American described as "a painful sense of frustration and impotence. . . ."[1] Others were moved by the force of example. Best known was the medical aid provided by the American Red Cross, which by the end of June had sent England large quantities of x-ray machines, surgical instruments, and sulfa drugs; but there were other ventures underway, too. Harvard that summer was making plans to send the British a fully equipped hospital and laboratory to fight infectious disease.[2]

Newly formed groups were also seeking a role. One organization calling itself the "Downtown Medical Unit" proposed to evacuate British wounded to New York City. More realistic and impressive were the plans of the "American Hospital in Britain, Limited." Backed by a coterie of prominent

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[1] Jerome Greene to Lionel Curtis, June 6, 1940, A3458; FO371/24223, Foreign Office Correspondence, Public Record Office, London.

[2] R. P. Linstead to Dr. Conant, July 16, 1940, A3458, *ibid*, Don Wharton, "What you can do to help the Allies," *Reader's Digest*, July 1940 *37*· 38. For a brief account of the Harvard Hospital unit which began to function in Sept. 1941, and which served the British for about a year before becoming a part of the American Army medical operation, see C. L. Dunn, *The Emergency Medical Service*, I (London: H M. Stationary Off 1952), 435-37

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philanthropists, including Winthrop Aldrich, Alfred P. Sloan, and Mrs. A. I. Dupont, the organization wanted to send a completely equipped surgical team abroad, to serve in any way the British wanted.[3]

In mid-June, owing to an increasing number of physician inquiries, London directed its Washington embassy to begin registering Americans for British service under the Ministry of Health. To insure the enlistment of qualified people, the Ambassador, Lord Lothian, sought an American group to screen the volunteers. The American Medical Association was his first choice, but because its officers were unwilling to do anything more than supply dossiers, Lothian had to look elsewhere.

Finally, he was able to turn the job over to the American Hospital in Britain organization. That group, in fact, sought the responsibility and was even interested in helping to recruit doctors. Its leading spirit, Dr. Philip Wilson, professor of orthopedic surgery at Columbia University and the head of the American Hospital's surgical team, was a fervent Anglophile who was eager to take his colleagues into war with him.

To direct the recruiting and evaluation operation, Wilson enlisted the services of neurologist Foster Kennedy, an Irish-born physician then associated with the Cornell Medical School. Kennedy not only enjoyed the confidence of the British Ambassador and the Ministry of Health, but he was also widely known and highly regarded among his colleagues in the U.S. Everything considered, he was just the man to head the search for American volunteers.[4]

By early September Kennedy and the American Hospital staff were ready to begin their solicitation in earnest. AMA members had been informed of the American Hospital's new role as agents of the Ministry of Health. The American College of Surgeons had agreed to poll its membership for volunteers. And every physician who had sent an inquiry to the Embassy that summer had received a reply from Kennedy and a form for enrollment in British service.[5]

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[3] Chancery (Washington) to General Department (FO), June 26, 1940, A3525, FO371/24223, *The Times* (London), September 4, 1940; P. D. Wilson to Royal Infirmary, Manchester, June 6, 1940, A 3212, FO371/24223 The story, in brief, of the American Hospital in Britain is also treated in Dunn, *EMS,* I. 433-35. It began operations October 1940, and like the Harvard facility it, too, was taken over by the U.S. Army in 1942.

[4] Lord Lothian to FO, June 14, 1940, A3212 FO371/24223. *J A.M A.*, June 29, 1940, *114*· 2556, Foster Kennedy to John Foster, August 3, 1940, A3756, *ibid.*, *The Times* (London), September 4, 1940, Lothian to FO, July 20, 1940, A3212, FO371/24223. For an index of American doctors' admiration of Kennedy, see New York *Times* January 8, 1952, p 27

[5] John Foster to Chauncey Truax, September 6, 1940, A3996, FO371/24223, Foster Kennedy to G. Bramwell, August 28/1940, *ibid,* C. S. Traux to Dr —, September 4, 1940, *ibid.*

Launched with large expectations, the drive soon began to falter. Although many Americans inquired, few joined the list. Among those who did, a number were professional rejects. As the embassy informed the Foreign Office, "in many cases the doctors . . . are unknown to the American Medical Association or else appear to be unsuitable on the grounds of insolvency or misbehavior."[6]

As the British were discovering, American medicine had only recently emerged from its "dark ages." Until the 1920s licensing standards were loosely enforced and medical education was a sorry affair in many states. Typical of the professional hangers-on who sought to serve the British was "Doctor" Ernest Ball of Sulphur, Oklahoma. Ball claimed a 1904 degree from the Dallas Medical College, although the AMA could find no trace of his having been there before that year. There was no question that Ball was legally licensed. Yet the Texas statute which welcomed him into practice was itself questionable: anyone (and everyone) who had practiced in Texas prior to 1907 was fully certified as a doctor.[7]

There were several reasons why more respectable doctors failed to respond to Britain's appeal. For one thing, American physicians were making much more money than their British counterparts, and as Kennedy told the British embassy, the war positions were simply "not good pay" for U.S. doctors. To make the offer attractive, salaries had to be boosted for the Americans. One way, Kennedy suggested, was to give them a sizeable tax rebate.[8]

But London would have none of it, and the terms of employment which finally emerged were hardly attractive to a class of Americans grown used to good living. Although they would receive free room and board, American doctors would draw the same salary as their English colleagues and pay the same high taxes too. Moreover, they would have to bear their own expenses to England as well as the costs of shipping their equipment. Clearly, a demonstration of loyalty to the Allied cause would be quite costly to most American physicians.[9]

To add to Kennedy's woes, British recruiters were not the only ones in the field. The U.S. Army, Navy, and Public Health Service were also adding more doctors then, and none was willing to defer to British needs. Nor were professional spokesmen willing to do so. When the editor of the AMA *Journal* published the announcement of the British enrollment drive, he squeezed it between two longer items detailing the Army and Navy needs. Moreover, in his editorial stressing the patriotic obligations of American physicians, he made no mention of the possibilities of British service.[10]

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[6] Chancery to American Department, FO, July 25, 1940, A3212, FO371/24223

[7] Chancery to FO, August 30, 1940, A3996, FO371/24224, On the state of American Medicine in the early twentieth century see Abraham Flexner, *An Autobiography* (New York. Simon & Schuster, 1960)

[8] Record of Foster Kennedy-John Foster conversation, August 7, 1940, A3756, FO371/24223, also Fraser to Kennedy, November 14, 1940, A4380, FO371/24224.

[9] Ministry of Health memo to Secretary of State for Foreign Affairs, August 30, 1940, A3996, *ibid*

The major recruiting obstacle, however, was Britain's unwillingness to put American doctors immediately to work, London preferring instead to put them on a reserve list for future call. In view of the sharp drop in England's pool of civilian practitioners (a result of mounting Service demands), such a policy seemed to work against the national interest. But the British were reluctant to use Americans in the private sector. Within both government and the profession, there was a strong fear that doctors of alien nationality would stay on after the war, crowding out the natives.[11] To avoid that, the Minister of Health planned a more limited use of the Americans, restricting them to hospitals of the Emergency Medical Service (EMS), an agency of the Ministry created in 1938 and responsible for the care of civilian casualties of the blitz and of Servicemen wounded in combat. Since there was no manpower shortage yet in the EMS and since no plans existed to use Americans in British military units, London simply saw no need for appreciable numbers of U.S. doctors. The same reservations applied with greater force to the large number of alien doctors (over 1000) who had fled to England to escape racial and political oppression in Europe.[12]

To the American Hospital group and others who shared its interventionist outlook, London's policy was self-defeating. Wilson, who almost thirsted for action, was sure that deferred enlistment would discourage volunteering. Kennedy agreed and pressed London to change its policy. Volunteers, he warned, were full of enthusiasm for the English cause, "but that spirit cannot be maintained by their names being put merely on file."[13]

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[10] *J A M A* , June 29, 1940, *114* 2556

[11] On the military's heavy demands for doctors see Minutes of Meeting of Ministry of Health, March 18, 1940, MH76/328, Ministry of Health Papers, PRO, also Ken MacGregor, minute, September 19, 1940, *ibid* ; on the feeling about alien doctors see Minutes of Home Office Medical Advisory Committee, August 21, 1940, MH 76/334, also "Robinson Committee. Constructive Suggestions," n d , but ca early 1941, MH 76/329

[12] "Emergency Medical Service Class I Applications," in K MacGregor to R Labouchere, December 21, 1940, A5211, FO371/25224; *J A M A* , June 29, 1940, *114* 2556, also Ministry of Health to American Red Cross, n d , but ca. November 1940, MH76/218, G. Bramwell to C. S Truax, September 16, 1940, A4380, FO371/24224. On the organization and purpose of the EMS and its relation to European aliens, see Dunn, *EMS*, I· VI, VII, 65, 424.

[13] Kennedy to F. R Fraser, October 5, 1940, A3996, FO371/24224; C Bramwell to Truax, September 16, 1940, A4380, *ibid*

Although unconvinced it was hindering recruiting, the Ministry of Health finally relented a little. In mid-September it authorized the enlistment of 20 seasoned surgeons and 30 less experienced general duty men for immediate service in the EMS. Although Kennedy had urged such a step, political considerations were the main cause of the Ministry's shift. Since mid-summer, the Foreign Office had been cautioning the Ministry against deferring all American doctors. That was bad politics, the Foreign Office insisted, for it could alienate Americans from offering other kinds of aid which Britain vitally needed. What the Ministry ought to consider, Lord Lothian thought, were the positive advantages of using Americans: doctors who saw service in England would be propagandists for the British cause once back home.[14]

Lothian might have been right had an appreciable number of Americans volunteered. But few did. The end of 1940 found Kennedy hard pressed to find even the 30 general duty doctors London had authorized. Often, the physicians whom the American Hospital could recruit failed to fit British needs. Boston surgeon L. E. Wolfson was one mis-cast American. As a plastic surgeon, Wolfson had skills far too specialized for the work required. The British, while they regarded him highly, found little for him to do and with some embarrassment had to send him home.[15]

By the end of 1940 the Ministry of Health had decided, however unfairly, that Kennedy and the American Hospital were chiefly to blame for such poor results. Ministry legal counsel Kenneth MacGregor did not believe that "Foster Kennedy's Committee carry heavy enough guns for this job . . . ."[16]

Sir Wilson Jameson, a recent recruit from the London School of Hygiene and Tropical Medicine and the Ministry's chief medical officer, agreed, and in December he began to consider alternative arrangements.[17]

In so doing, Jameson was not merely reacting to Kennedy's apparent shortcomings. His decision was also a result of pressure from the American Red Cross, which was eager to take over the volunteer doctors program. The Red Cross not only did good works but did them aggressively. Seemingly guided by a kind of "Robber Baron" instinct, it looked on agencies like the American Hospital in Britain as a monopolist would view an upstart business competitor. Bowen McCoy, who directed American Red Cross work in Great Britain, boasted to Jameson that his group "is the only organization that can effectively publicize and interpret the [British] needs . . . to the American public and American medical profession."[18] For its part, the ministry was fully willing to yield to Red Cross insistence. If Kennedy's recruiting trouble "induces him to throw in his hand," said MacGregor, "so much the better, if the American Red Cross can establish better arrangements. . . ."[19]

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[14] *Ibid* , also see T North Whitehead, minute sheet, August 7, 1940, A3512, FO371/24223; Lord Lothian to FO, July 4, 1940, *ibid* , also see Dunn, *EMS*, I: 393.

[15] MacGregor to E. G. Bearn, December 28, 1940, MH 76/218; William Jameson to Bowen McCoy, January 8, 1940, MH76/218.

[16] MacGregor to R. Labouchere, December 21, 1940, A5216, FO371/24224 On MacGregor see *Who's Who, 1962* (London, 1962), p. 1928.

[17] On Jameson, see *Who's Who, 1946* (London, 1946), pp. 1425-25.

Improved arrangements were important to Ministry officials by then because Britain was fast approaching a doctor crisis. Owing to rising demands for military medical staff, the reserve of civilian practitioners had disappeared. The only way open to the British to avoid a collapse of normal and emergency civilian medical care was to employ alien physicians. Already, plans were under way for using refugee doctors from Europe in EMS hospitals. As for the Americans, the thinking was to use them primarily in the Royal Army Medical Corps (RAMC), which estimated that it would need about 900 U.S. doctors (with another 100 to be placed in the EMS). If they could be found, their enlistment would greatly ease the strain on England's civilian doctor population.[20]

The American interventionists were delighted with Britain's new plans. Wilson, who had been in London with his surgical staff since September, had been pressing such a proposal in private talks, and the American Red Cross was pushing for larger enlistments in more official channels. The Red Cross was not only in a better position than Kennedy's group to furnish the 1000 doctors, the Health Ministry believed, but its standing as an international agency would, it was hoped, enable the British Army to get its physicians without running afoul of America's neutrality legislation.[21]

In mid-December the Ministry named the American Red Cross as its new agent and began severing connections with the American Hospital. F. R. Fraser, assistant director of the EMS and formerly a Rockefeller Institute

[18] McCoy to Sir William Jameson, December 24, 1940, MH76/218, on the Red Cross' aggressive style of philanthropy see Note on Interview between MH officials and British War Relief Society, December 16, 1940, *ibid*.

[19] MacGregor to Beams, December 28, 1940, *ibid*.

[20] Dunn, *EMS*, I. 390-93, 424-27. A sign of the growing doctor crisis was the dropping doctor to patient ratio, which declined from 1.2184 in 1938 to 2540 in 1940 and 2588 in 1941. See *ibid*, p. 401.

[21] On Wilson's talks see Ministry of Health to American Red Cross, n d., but ca. November 1940, MH76/218; MacGregor to Labouchere, December 21, 1940, A5211, FO371/24224; MacGregor, minute, January 16, 1941, MH76/329 (on number of doctors), Minutes of an Interview between MH officials and the British War Relief Society, Dec. 16, 1940, MH 76/218 Dunn, *EMS*, I: 433.

fellow, wired Kennedy that if he had not yet gathered the previously authorized contingent of surgeons, he was to stop work and put the project in the hands of the Red Cross.[22]

Setting the RAMC recruiting plan to work took far longer. Early in 1941 English civilian and military medical officials reached agreement that the scheme was desirable, and hopes were that it would attract enough newly minted American M.D.'s to fill the rolls. But financial authorization, Foreign Office backing, and terms of service had yet to be arranged, as did the more sensitive matter of U.S. Government approval. No one anticipated a major problem over neutrality, because a precedent already existed in the use of Americans as Royal Air Force pilots. Should difficulties arise, London believed that the American Red Cross, with its "enormous influence in the U.S.A. and unlimited financial resources" would be a valuable ally.[23]

Ultimately, the Red Cross played a key role. Chairman Norman Davis, a wealthy Southern aristocrat and long-time adviser to Democratic presidents, not only got Roosevelt's approval of the project, but also convinced the President to make an appeal for volunteers. In addition, the AMA agreed to be more helpful. So, when in April the British Red Cross formally requested her "sister society in America" to furnish "up to 1000 medical practitioners," the ground was well-prepared.[24]

Although the call was primarily for military duty, the British emphasized that the work was strictly a humanitarian service. London also stressed that the need was pressing and the "conditions of service good."[25] Apparently, medical officials finally realized that they had to up their ante if they were to attract qualified doctors, for the new contract terms were considerably more generous than before. A young practitioner bound for civilian service would now come to England at British expense, wear only civilian clothes, and pay no local income tax.[26]

Good planning and organization were no guarantee of results, however. Indeed, the Red Cross campaign, because it reached so many physicians, generated peculiar problems of its own. Those difficulties revealed with particular clarity the kind of basic cultural discontinuities between the two countries which helped to frustrate the volunteer enlistment program.

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[22] E. G. Bearns to McCoy, December 14, 1940, MH76/218 Fraser to Kennedy, December 26, 1940, *ibid*. On Fraser, see *Who's Who, 1946* (London, 1946). p. 959.

[23] "Robinson Committee: Constructive Suggestions," ca. early 1941, MH76/329; MacGregor, minute, January 16, 1941, *ibid.*

[24] FO to Embassy (Washington), cable telegram, April 30, 1941, A3175 FO371/26226 (contained the April 18 wire from Britcross to Amcross); British Embassy (Washington) to FO, telegram, March 10, 1941, A2636, *ibid ; Who's Who in America,* 21 (Chicago, 1940), 731, on the AMA attitude see MacGregor to Labouchere, December 21, 1940, A5211, FO371/24224

[25] FO to Embassy (Washington), April 30, 1941, A2475, *ibid.*

[26] A W. Neville to Jock Balfour, April 3, 1941, A2475, *ibid.*

One issue which plagued the effort from the outset concerned the recruitment of naturalized American doctors. The problem had its origins in the pluralism of American society and in the War Office's response to it. In the view of the War Office all Americans not of British parentage or unable to trace their U.S. ancestry back several generations were Americans in name only and had to be regarded as possible security risks. The fact that such persons might have lived in the U.S. for as long as 20 years did not alter War Office thinking. Products of the "once an Englishman, always an Englishman" school, its leaders could not conceive of loyalty being given to any country other than the land of one's birth.

War Office suspicions of German and Italian-Americans, of course, were understandable and widely shared. But its other reservations, concerning Americans from the captive nations of Europe, were hardly rational and dismayed the War Office's sister departments, for by the summer the EMS was already using several hundred doctors of alien nationality in civilian hospitals. Even German and Italian-born physicians were given a chance to work, provided they did so in the presence of British doctors and treated only civilian patients.[27]

The question of what to do with such Americans first arose in early summer 1941, when the American Red Cross approved the enlistment of five naturalized American physicians, all of whom had passed F.B.I. security checks. As soon as the War Office learned what had happened, it quickly overruled the Red Cross, justifying its action on the ground that "naturalized United States citizens are 'aliens' and as such, in the same category as refugees from Czechoslovakia, Poland, etc. . . . , who are regarded with some suspicion."[28] Apparently, the War Office did not know what was going on in its own civilian hospitals, or if it did, it disapproved.

Reactions came swiftly, as well they might, for the War Office action was insulting and unfair to the doctors involved. The case of Polish-born Phillips Marshall was typical. A 1921 immigrant to the U.S., Marshall had been an

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[27] On the WO attitude toward naturalized Americans see Minute Sheet Summary, July 19, 1941, A5582, *ibid*., on FO dismay at provinciality of WO see C. H Dickens to Directorate General, British Information Service, October 2, 1941, A10036, 371/26206 On British use of European doctors see Dunn, *EMS*, I 424-28. In June 1941, 300 doctors of alien nationality had been cleared and by year's end places for 600 had been found.

[28] C. S. King, minute sheet, July 22, 1941, A5582, FO371/26226. King was quoting the view of a Major Robinson of the WO.

American citizen since 1927. So eager was he to serve with the Allies that he had resigned his reserve commission in the U.S. Army and given up his practice, to free himself for duty in England. Lord Halifax, the new British Ambassador in Washington, recognized the injustice being done the Americans and protested to London.[29]

The implications of War Office intransigence, however, went beyond insult to individual doctors. Applications for British service had been "disappointingly few" up to then, and the Foreign Office believed that such rejections would make recruiting in America more difficult.[30] Furthermore, the action jeopardized London's relations with officials of the American Red Cross. When news of the War Office decision reached them, they had already completed arrangements with the five physicians. As a result, the Foreign Office noted, the Red Cross were "much embarrassed and inclined to be resentful." If the War Office did not shift ground, their "cooperation with us may be endangered. . . . "[31]

To head off that possibility, the Foreign Office pressed the War Office to modify its position. Speaking for his ministry, career diplomat Jock Balfour, who had served for a time in the U.S., reminded his War Office colleagues of a basic fact of American life. The U.S., he lectured, was a nation "which warmly welcomes, indeed virtually compels, the naturalization of the foreign-born, and a naturalized American is accepted by his fellow citizens in the U.S. on the same footing as a native of the country and is not subject to the social and personal disabilities and suspicions which attach to his counterpart in older countries. Any suggestion that we are discriminating in principle between native born and naturalized U.S. citizens in favor of the former is liable to cause offense not only to the victims of our discrimination but also to Americans in general, and it is unnecessary to stress the importance, in these times, of avoiding any action likely to irritate American sentiments against us."[32]

If War Office leaders were poor students of American civics, they were at least not stubborn ones. A week after Balfour's prodding, the War Office changed its policy. Henceforth, the only naturalized Americans not welcome in England and in the RAMC were those born in one of the Axis countries.

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[29] Viscount Halifax to FO, July 17, 1941, telegram, *ibid*

[30] King, minute sheet, July 22, 1941, *ibid*

[31] Minute Sheet Summary, July 19, 1941, *ibid*

[32] Balfour to Colonel S. Arnott, July 24, 1941, *ibid*, on Balfour see *Who's Who, 1962*, (London, 1962), p 138.

All others were acceptable, and on August 15 the matter reached a symbolic conclusion. That day Polish-born Phillips Marshall left for England.[33]

A more difficult problem, one in fact which never found a solution, involved American Negro doctors. The issue of Negro volunteers touched a sensitive nerve in both Washington and London, for mishandling could not only worsen an already tense American racial situation but also aggravate problems that Britain was having with her black colonials. The serious involvement of the Prime Minister and much of his cabinet with the issue testified to the seriousness with which London viewed it.

Initially, though, London was slow to recognize a problem. In early summer 1941 the first black American applied for medical service, only to be rejected by the Red Cross for lack of qualifications. That may or may not have been the case. While the American Red Cross had no clearly defined racial policy, it did feel that Negro recruitment "was undesirable."[34] A few days later, the British Embassy in Washington received a direct inquiry about England's willingness to accept Negro doctors. Seeking guidance from London, the Embassy's military attaché asked the War Office if it "could take one token negro doctor anywhere?" After a brief consultation with the Foreign Office, the War Office decided to follow its advice and simply ignore the inquiry.[35]

That sleeping dog refused to lie, however. In July another Negro applied, this time a candidate with abundant qualifications. He was Dr. Walter W. King, a 66-year-old physican who had had a distinguished career with the U.S. Public Health Service. Pressed by its attaché for immediate instruction, the War Office again solicited Foreign Office counsel. Old hand Victor Cavendish-Bentinck, who in his 25 years with the Foreign Office had served in many foreign capitals, had his eye on political repercussions in the U.S., and he recommended that King be accepted. He could serve, Cavendish-Bentinck suggested, in the Cardiff docks area, "where I believe he will find a number of patients of the same genus."[36] The War Office, however, declined the advice. Having assumed that King was volunteering for military rather than civilian duty, the War Office tersely informed Washington that it would "accept one token Negro doctor as evidence of cooperation, for employment in West Africa."[37]

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[33] J Robinson (Major) to Balfour, August 1, 1941, A6041, *ibid*, L. Leach to Secretary of State for Foreign Affairs, August 15, 1941, A6884, *ibid*

[34] Military Attaché (Washington) to WO, June 27, 1941, A5081, *ibid;* Attaché to WO July 10, 1941, A8364, FO371/26227. On the initial application see British Military Attaché to War Office, July 10, 1941, *ibid*

[35] Military Attaché (Washington), to WO, June 27, 1941, A5081, FO 371/26226; T North Whitehead, minute sheet, June 7, 1941, *ibid*.

[36] On King, see *New York Times* (October 11, 1950), p 33. V Cavendish-Bentinck to Brigadier P G Whitefoord, July 11, 1941, A8364, FO371/26227. On Cavendish-Bentinck, see *Who's Who, 1962* (London, 1962), p 517.

Neville Butler, an experienced American hand and head of the North American Department in Whitehall, sensed a blunder. "I have a hunch," he commented, "that the War Office are being rather 'blimpish' about this." F. E. Evans, who had spent years in the U.S. on various diplomatic assignments, had a deeper concern. If King actually preferred a civilian post, American Negroes would consider the West African assignment a rebuff, "as I fear it is intended, . . . and the result may be far-reaching and regrettable."[38]

The chances of causing a stir in America were good right then, for Negro doctors and those in allied fields had for some months been openly complaining about the way the U.S. Army Medical Department was treating its black professionals. Excluded from all the attractive positions (such as general and station hospital posts which were available to white doctors only), Negro physicians were accepted only in segregated units, mostly for service as regimental surgeons, low prestige jobs that other blacks derisively called "First Aid Surgeons."

The situation had long been a sore point for the all-Negro National Medical Association, which about 1940 began pressing for full integration of the Medical Department's professional staffs and greater use of Negro doctors. Surgeon General James C. Magee, however, refused even to consider the first proposal. Averse to using his department for social experimentation, he also argued that black doctors were more needed in civilian service. Privately, he and his aides also doubted the capability of most Negro physicians. The only concessions he would offer were promises to induct a few more black professionals, create separate wards for Negro troops in selected post hospitals (which would then be staffed with black physicians), and establish an all-Negro hospital. In March 1941 Negro medical spokesman grudgingly yielded to Magee's proposals. The end of the year, however, found the old patterns little changed—only two segregated wards were in operation and the all-black hospital idea had failed to gel—with the result that bitterness continued as before.[39]

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[37] WO to Military Attache (Washington), July 17, 1941, A8364,FO371/26227; also Military Attaché to WO, July 10, 1941, *ibid.*

[38] Butler, July 19, 1941, and E. E Evans, July 21, 1941, minute sheet, *ibid.* On Butler see *Who's Who,* 1941 (London, 1940), p 461; also *ibid* (1946), p. 400. On Evans, see *ibid,* (1946) p. 866

Although it knew nothing about the racial problems of the American Army Medical Department, the Foreign Office decided that it must "make a fuss" about the War Office position. Accordingly, Cavendish-Bentinck urged the War ministry to lift its ban on entry into Britain of American Negro doctors in the event that King preferred a civilian spot. He should have no trouble locating a position, even among white patients, for black doctors were "far from adversely viewed in the East End and the poorer districts of other cities."[40]

The War Office never had to make that accommodation, for unexpectedly the problem of Dr. Walter King disappeared. The American Red Cross told London that it had rejected his application on the ground that he was too old for the RAMC.[41] But if the War Office believed it had escaped a difficult situation, its relief was premature. In late September Walter White, the urbane and dogged national secretary of the NAACP, decided the time had come to protest what he saw as blatant British discrimination against his race.

In a sharply worded letter to Prime Minister Churchill (which would go unanswered for six months), White charged that physician King had tried to volunteer for civilian service, but was told by the American Red Cross that Britain wanted white doctors only. Furthermore, King's rebuff was only one of many. The Air Ministry rejected Negroes as Atlantic Ferry pilots. Civilian agencies refused to hire blacks for construction jobs in the British West Indies and for white collar posts with the British Purchasing Commission in Washington. Not even the blood of American Negroes was welcomed in England, White claimed. Surely, those lofty war goals known as the "Four Freedoms" were not to become inoperable "when they encounter the color line."[42]

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[39] For a concise but thorough treatment of the relationship between Negro doctors and the Army Medical Department up to 1942, see Robert J. Parks, "The development of segregation in U.S. Army hospitals, 1940-42," *Military Affairs*, (December 1973), *37*: 145-50 Two segregated wards finally materialized in May, 1941, but no others followed. The all-Negro station hospital, at Ft Huachuca, Arizona, finally opened in 1942. Parks notes that via those arrangements Negro physicians got their increased professional opportunities but the cost was increased segregation of enlisted men, for before the appearance of the separate Negro wards, the patient floors of Army hosptials had been fully integrated For an added discussion of why the Surgeon General's Office resisted NMA pressure (e g. the belief that Negro medical schools were substandard), see John McMinn and Max Levin, *Personnel in World War II* (Washington, D.C. Govt Print Off , 1963), p. 323. The story of the Army's use of Negro medical professionals for the whole war period is summarized here.

[40] Bentinck to Brigadier Whitefoord, July 24, 1941, minute sheet, A8364,FO371/26227.

[41] Stanley Arnot to Bentinck, August 2, 1941, *ibid*

[42] Walter White to Winston Churchill, September 26, 1941, A8221, FO371/26205.

To add to London's discomfort, that same month a homegrown civil rights group, the League of Coloured Peoples, who knew about the NAACP letter, voiced a similar concern about freedoms for His Majesty's own colored subjects. The combination of pressures convinced the British that White's letter merited serious attention. As Ambassador Halifax noted, while NAACP charges were not likely to do much damage to American race relations, they could exacerbate "our colonial coloured problems."[43]

To Britain's credit, White's charges received careful study. For the Foreign Office, which coordinated the investigation, the revelations were unsettling. Only on the matter of blood donors was White completely misinformed. Every other charge had substance. Regarding the pilot rejections, the Air Ministry frankly admitted its bias: dark-skinned people (Latins as well as Negroes) were more likely to be security risks than white men. Since it was impossible to check the loyalty of "all these people," it was simpler to exclude them all.[44]

"Oh dear!" was all one foreign officer could manage in response.[45] Foreign Secretary Anthony Eden was equally dismayed, and in late December Eden got the Air Ministry to drop its racial clause on the ground that it was politically objectionable. The Air Ministry need not fear an "embarrassing increase" in Negro applicants, however, for America's recent entry into the war would surely prevent that.[46]

The problem of Negro doctors refused to yield to so simple a solution. In fact, it proved insoluble. The essential difficulty was London's conviction that the admission of American Negro doctors into Britain ran against certain imperial and domestic interests. Among all the ministries involved, only the Foreign Office demonstrated a sensitivity to American Negro feelings and some regard for elemental fairness. Everyone else—including the Prime Minister—chose instead to conjure up scenes of vast Negro migrations should they fail to keep Britain's gates tightly shut.

The Ministry of Health considered the problem first, and after lengthy study it recommended against admitting black physicians. The EMS, it noted, in justifying its stand, "was provided for civilian air raid casualties and other classes of civilian patients who have no choice as to the member of the hospital staff by whom they are to be treated." The Ministry felt an obligation "therefore to take into account the susceptibilities of the patients and to provide a service similar to that which they would normally be accustomed."[47] In America, the Army Medical Department used an almost identical argument in rejecting Negro doctors' proposals for the integration of its professional staff.[48]

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[43] Halifax to FO, October 12, 1941, *ibid*; Regarding the League of Coloured Peoples' pressure see Dr Harold Moody (LCP) to Winston Churchill, November 13, 1941, A9357, FO371/26206.

[44] Dickens to Directorate General, British Information Service, October 3, 1941, A10036, FO371/26206

[45] *Ibid.*

[46] Evans to Secretary of State for Air, December 22, 1941, *ibid.*

The Colonial Office, too, had a stake in the admission issue, chiefly in its impact on England's colored colonials. As Assistant Secretary of State for Colonial Affairs, J. J. Paskin, explained, "the attitude adopted towards coloured persons of foreign origin is closely watched by the British Colonial coloured peoples." In any reply to the NAACP, London must avoid giving any encouragement to colored subjects to migrate to England, but it must do that without appearing to hold any color prejudice. In regard to the second concern, the Health Ministry's statement was generally satisfactory, but one part needed revision. The reference to the "susceptibilities" of patients, Paskin feared, might prove offensive. Rather, the statement should emphasize the desire of all "patients to be treated by doctors of their own race."[49] Even so, Paskin admitted, it was "not very easy to get the answer to [the NAACP's] complaint just right."[50]

The Prime Minister's Office saw another loophole in the evolving position statement. Anthony Bevir, Churchill's private secretary, handled the matter for his Minister. Bevir was experienced in such affairs, having served most of two decades in the Colonial Office; and perhaps because of that, his views closely paralleled Paskin's. He was concerned, however, about putting too much stress on the value of racial matching of doctor and patient. The reply to the NAACP, he warned, "will be examined word by word by the recipients, and I can see them basing a claim for coloured doctors for coloured people on it at some later date." To avoid that possibility, Britain's rejection of the Negroes' offer of service must be put a little differently. Bevir's revision showed how: "it must be remembered that the coloured population of this country is not large in comparison with that in the U.S., and some people might even never have seen those of a different colour. In an emergency, children, in particular, who do not know of the traditional kindness shown by coloured people to children might be uneasy through unfamiliarity; and emergencies are no time for such complications." To sugarcoat the rebuff, the Prime Minister's draft assured the NAACP that the British government intended no reflection on the professional abilities of black doctors.[51]

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[47] Frank Bliss to King, November 21, 1941, A9625, *ibid*

[48] Park, "Development of segregation in U S.A. hospitals," *Military Affairs*, (Dec 1973), *37* 146-47

[49] J. J Paskin to Evans, March 4, 1942, A2171, FO371/30665. On Paskin see *Who's Who 1946* (London, 1946), p. 2113

[50] Paskin to Evans, March 25, 1942, A2788, FO371/30665

In the spring of 1942 the NAACP finally received the Prime Minister's carefully worded reply. From London's perspective, it was well-timed, for it arrived after the U.S. had entered the war and thus past the time when black physicians were actually available for English service.

Probably all the fuss was needless. Walter White visited Britain early in 1944 to see how black American troops were faring, and he was pleased by the generally good relations between English people and American Negro soldiers. One incident that particularly encouraged him involved a community dance and the response of local people to the harrassment of black soldiers by some of their white American comrades. The master of ceremonies silenced the band and politely invited the white soldiers to leave. The local people cheered the decision.[52]

Whether needless or not, British concern to keep black physicians out of England was self-defeating. Despite a vigorous campaign, the British government and the American Red Cross were unable to recruit more than a fraction of the 1000 doctors they sought. Certainly many Americans made inquiries—the Red Cross reported 1456 by mid-July 1941—but few enlisted. Out of 671 doctors to whom the Red Cross mailed applications, through July 11, only 195 physicians responded, and just 54 ultimately passed all tests for service (age, citizenship, professional certification, and visa approval). Apparently not all of those decided to go, for in the whole period from early August until Christmas 1941 only 53 physicians left for England, 35 bound for the EMS and 18 for the RAMC—a total that was quite wide of the 1000-doctor target.[53]

Many things contributed to those disappointing results, of course. Competition from American military services, a strong national current of isolationism, and an understandable concern by doctors to avoid danger, all surely inhibited enlistment. But just as important was the fact of two distinct national medical communities, each reflecting the peculiar characteristics of the society of which it was a part. Bringing those communities together in 1941 simply proved more difficult than anyone foresaw.

[51] Anthony Bevir to G E Millard, March 18, 1942, A2788, FO371/30665. On Bevir see *Who's Who, 1946* (London, 1946), p. 226.

[52] R. J. Cruikshank to Angus Malcolm, February 11, 1944, AN587, FO371/38699, for White's observations and comments on White see files AN587 and AN159, *ibid*

[53] American Red Cross to American Embassy (London), July 11, 1941, A5719, FO371/26226; data on the doctors who actually served in England is found in a number of files in FO371/26227. See, for example, A7861 and A8311.

NOTES AND COMMENTS

REPATRIATION OF THE ROLLS PARK PORTRAIT OF WILLIAM HARVEY

WILLIAM C. GIBSON

When Sir Geoffrey Keynes was preparing his 1948 Vicary Lecture for the Royal College of Surgeons in London, he was at pains to follow up every likely lead in his study of "the portraiture of William Harvey." Thanks to the Librarian of the Royal College of Physicians and to the resources of their excellent collection, Sir Geoffrey had seen a photograph of the portrait room at Rolls Park, the home of the well-to-do Eliab Harvey, William's hospitable brother. Recessed into the wall of this room, and set in oval plaster surrounds, were the portraits of Thomas Harvey and his seven sons. Already Sir D'Arcy Power had reproduced in his *Portraits of William Harvey*, 1913, a supposed portrait of the great scientist noted as coming from Rolls Park at Chigwell in Essex. Subsequently this was shown to represent Eliab Harvey and not William.

With the thoroughness which had always characterized his bibliographic research, Sir Geoffrey set out, in 1948, to see for himself what might remain at Rolls Park 320 years after the appearance of *De motu cordis*. He had been warned by the current owner of that once-handsome country house that wartime military occupation and German bombs had left it in ruins. The visit was initially disappointing because the darkness within the remains of the portrait room made any identification hopeless. Two days later, however, Sir Geoffrey returned with a ladder and candle, by whose light he was able to make out the features of his hero in the recessed portrait highest on the right hand side. Already some of the portraits nearer the floor had been hacked out by vandals.

Those remaining were removed with care and taken to the National Portrait Gallery, and at a later date were displayed at the Royal College of Physicians. Then suddenly, in 1960, that of William Harvey was exported, illegally, to California as "Portrait of a Gentleman, artist unknown." When this infraction of the Board of Trade rules was discovered, action was taken by the review board headed by Lord Cottesloe, its distinguished and knowledgeable chairman. The exporting agents were severely dealt with by the

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The Rolls Park portrait of William Harvey

courts, but the portrait was by then hanging in the study of a Beverly Hills physician. And there it has remained ever since, lost to public view.

In the latter part of 1976, the owner became seriously ill and it was decided to sell the painting through the well-known booksellers Zeitlin and Ver Brugge in Los Angeles. Within minutes telephones began to ring in Vancouver. Soon they rang in London where, despite the drop in the value of the pound sterling, the National Portrait Gallery was willing to contribute a substantial sum towards the repatriation to London.

It proved harder than one could believe possible to interest North Americans in making contributions to right a wrong and to send this portrait, painted in Harvey's lifetime, several thousand miles away! There were offers to acquire it in Texas and in other parts of the country. One potential donor to the repatriation fund finally said that he had nothing but praise for Harvey, and that a good color photograph should satisfy anyone's requirements. Others thought the undertaking just right for an Oxford graduate in search of a lost cause.

More telephoning to London produced increased participation by the National Portrait Gallery trustees whose Chairman was keen on the enterprise. Mr. Zeitlin, afraid that the portrait was going to go elsewhere if things failed to progress at a more lively pace, negotiated a reduction in the price and threw in his expected commission. He wanted, above all else, to have the painting in London in time for the ninetieth birthday of Sir Geoffrey Keynes. In Vancouver some of the privately contributed trust funds of the historical section of the great Woodward Biomedical Library were reckoned up, and the decision was taken to provide the remainder of what seemed a king's ransom.

On December 22nd a dinner given by Mr. & Mrs. Zeitlin in Los Angeles and a viewing of the Harvey prize at the home of Dr. and Mrs. Jack Pincus completed the North American phase of the return of "little Harvey" of the bright eye and raven hair to his homeland. Attending l'envoi was another nonagenarian, Dr. Elmer Belt. The friends of the departing Harvey were for the first time able to examine the handsome face of their erstwhile neighbor, quietly gazing down from his oval frame!

From evidence provided by Keynes in his monumental Vicary Lecture it is possible to date the Rolls Park portrait as having been done—by an artist so far unknown—at about the time of the writing, if not the printing, of *De motu cordis*. It shows an alert, vigorous and quietly confident man—slightly reserved—certainly no back-slapping extrovert! Over his left shoulder the artist supplied the title, "Dr. William Harvey."

To become immersed in the story of Harvey's portraits—including some alleged portraits—is to be amazed at the number and disparity of them. Most, if not all, of those known prior to the Rolls Park discovery by Keynes, are posthumous, copied clearly from one or another. Then, of course, there are those innocently mistaken for William Harvey, as has now been shown to be the case with his brother Eliab. There remain, however, those "unabashed modern forgeries" which in Keynsian terms are "the least reputable result of the demand having created the supply."

For some reason Osler was offered more than his share of the less authentic variety. They seem to be hardy perennials, recurring in sales literature in phoenix-like array. The verbal gymnastics of the "pushers" of these creations would make a psychological story in themselves.

On February 22, 1977, a ceremony was held in the National Portrait Gallery, London, to receive back the too-long-alienated Rolls Park portrait in the presence of Lord Cottesloe, Lord Kenyon and their fellow Trustees. Others present were Jacob Zeitlin, Josephine Ver Brugge, Sir Miles Clifford and Sir Michael Perrin—both long associated with philanthropic trusts—the Director, Dr. John Hayes, who aged considerably under the duress of too many incoming transatlantic telephone calls, the staff and your author. Under the arrangements made, Harvey will, from time to time, be exhibited at the Woodward Library at the University of British Columbia, but his permanent home now becomes London again. He will undoubtedly be much admired at the 400th anniversary of his birth and the 350th anniversary of the publication of his revolutionary seventy-two pages. *De motu cordis*. Both events the Royal College of Physicians will celebrate on July 9, 1978.

REFERENCE

Geoffrey Keynes. *Portraiture of William Harvey*. With a Catalogue and Reproductions of the Pictures. London: Royal College of Surgeons of England, 1949.

MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

APPOINTMENTS IN THE HISTORY OF MEDICINE

Union College, Schenectady, N.Y.

For the Spring 1977 term, Union College appointed William F. Bynum as Mellon Lecturer in the History of Medicine. As Mellon Lecturer Dr. Bynum gave a course on the History of Medical Science (17th through 19th centuries) and offered a faculty seminar, meeting five times at Union College and four times at Albany Medical College. A graduate of Swarthmore College, Dr. Bynum received his M.D. degree from Yale and the Ph.D. from Cambridge University. He did post-doctoral research as a U.S. Public Health Service Fellow at the Instititute for the History of Medicine, University of Zürich. Dr. Bynum is Head of the Sub-Department of the History of Medicine at University College, London, and holds a joint appointment as Assistant Director of Studies at the Wellcome Institute for the History of Medicine. He sometimes acts as a Supervisor for the History of Science Tripos at Cambridge University. During his recent stay in the United States Dr. Bynum delivered the first Henry E. Sigerist Lecture in Medical History and Social Medicine at Yale University (see p. 301 below).

William F. Bynum

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University of Rochester

Dr. Theodore M. Brown has been appointed Associate Professor of History in the College of Arts, and of Preventive Medicine in the School of Medicine and Dentistry, University of Rochester, Rochester, N.Y. Dr. Brown, a native of New York, earned his B.S. degree at the City College of New York. He did graduate study in the history of science at the University of Cambridge and at Princeton University, receiving his Ph.D. degree from the latter in 1968. He was an NIH post-doctoral fellow at the Johns Hopkins Institute of the History of Medicine during the first semester of 1968-69, and returned to Princeton where he was Assistant Professor in the Program in History and Philosophy of Science. In 1973 he joined the faculty of the City University of New York as Assistant Professor of History, and organized and directed the Program in Health, Medicine and Society. At Rochester Dr. Brown has been appointed under the "Rochester Plan," the goal of which is to explore possible innovations in premedical and medical education and to stimulate contact among members of the Arts and Sciences and Medical School faculties. In addition to teaching responsibilities in both schools, Dr. Brown will be assigned certain student advisory and faculty committee responsibilities associated with the "Rochester Plan."

Theodore M. Brown

ANNOUNCEMENTS

AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE

Election of Officers

Secretary-Treasurer (2-year term): Charles G. Roland, Hamilton, Ont.

Council (3-year term): Leslie A. Falk, Nashville, Tenn.: Wilhelm Moll, Charlottesville, Va.; Ronald L. Numbers, Madison, Wis.; Ynez V. O'Neill, Los Angeles, Calif.

Awards

The Welch Medal was awarded to Dr. Lester S. King for his "outstanding contributions to the history of medicine."

James Allen Young, who has received his M.D. degree from Duke University School of Medicine, was awarded the Osler Medal for his essay, "Anthropometric Study of Human Growth in Nineteenth-Century American Medicine." Honorable mention was given to Lawrence G. Miller, Harvard University, for his essay "Pain, Parturition, and the Profession: The Twilight Sleep in America."

Annual Meeting, 1978

The next annual meeting of the Association will take place in Kansas City, May 11, 12 and 13, 1978. Persons wishing to have papers considered for presentation are asked to submit an abstract of 200-300 words (5 copies) to the Chairman of the Program Committee, Dr. Ronald L. Numbers, Department of the History of Medicine, University of Wisconsin, 1305 Linden Dr., Madison, Wis. 53706. The Committee also solicits suggestions for noon workshops.

Papers may deal with any aspect of medical history. They will be strictly limited to 20 minutes. Time for discussion will be provided at the discretion of the session's presiding officer. Contributions of non-members as well as members will be considered. Participants are reminded that the *Bulletin of the History of Medicine*, official organ of the Association, reserves the right to consider for publication any papers delivered at the meeting.

NATIONAL NEWS

History of Science Society

The 1977 annual meeting of the History of Science Society will be held jointly with the American Historical Association in Dallas, Texas, December 28-30, in the Hotel Adolphus.

Princeton University

For two years, 1978-79 and 1979-80, the subject of Princeton's Shelby Cullom Davis Center Seminar will be "The History of the Professions." The Seminar will stress the relationship of professionalization to Society through a series of studies of particular professions, their development, influence on society, etc. The Center will offer a limited number of Research Fellowships for one or two semesters, designed for highly recommended younger scholars, as well as for senior scholars of established reputation. It will also offer a one-year teaching fellowship. Inquiries and requests for application forms should be addressed to the Secretary, Davis Center for Historical Studies, 129 Dickinson Hall, Princeton University, Princeton, N.J. 08540. The deadline for the receipt of completed applications for 1978-79 is December 1, 1977. Scholars who are not applicants for Fellowships but would like to offer a paper to the Seminar are asked to write to the Director, Lawrence Stone at the above address.

Tulane University

Willard B. Marmelzat, Assistant Professor of Medicine (Dermatology), University of Southern California, was appointed Visiting Professor of the History of Medicine at Tulane in 1976. In April he delivered the 25th annual B. Bernard Weinstein Memorial Lecture at the Silver Anniversary Banquet of the Tulane University History of Medicine Society. A contributing editor of the *Journal of Dermatologic Surgery and Oncology,* Dr. Marmelzat regularly contributes a feature column, "Medicine and History."

University of Illinois Medical Center

The 34th annual D. J. Davis Lecture on Medical History was presented by Dr. Allen G. Debus, Director of the Morris Fishbein Center for the Study of Science and History, University of Chicago, on April 6, 1977. Dr. Debus' topic was "Medicine on Stage: Vaudeville and the Medical Profession 1900-1920."

University of Wisconsin

There are a limited number of reprints of the historical articles written by Dr. William S. Middleton which are still available to anyone who might like to have them. Requests and enquiries should be made to Mrs. Dorothy Whitcomb, Historical Collection, W. S. Middleton Health Sciences Library, 1305 Linden Dr., Madison, Wis. 53706.

Washington University

The Washington University Medical Center has started a new History of Medicine Club. Organized by Dr. Richard M. Ratzan of Jewish Hospital and Dr. Estelle Brodman, Librarian and Professor of Medical History, the Club meets at the Archives and Rare Book Building of the Washington University School of Medicine Library in St. Louis. Dr. Joseph Hanaway, Department of Neurology, will arrange the programs for the coming year.

Westport, Conn.

John C. Burnham, Professor of History and Lecturer in Psychiatry, Ohio State University, has been appointed by Greenwood Press the Series Editor of a new Greenwood series, "Contributions in Medical History." Two studies on the history of midwifery are already scheduled for publication. The Publisher and the Series Editor welcome manuscripts dealing with the history of medicine for consideration as part of the Series.

Yale University

The first Henry E. Sigerist Lecture in Medical History and Social Medicine, sponsored by the Department of History of Science and Medicine, Yale University, took place on April 4, 1977. The lecture, entitled "Hospital, Community and Disease: The London Fever Hospital, 1801-1850," was presented by Dr. William F. Bynum of University College, London. It will appear in a later issue of the *Bulletin of the History of Medicine.*

Appointments

Dr. George Rosen, Professor of the History of Medicine and of Epidemiology and Public Health, Yale University, has been appointed to the editorial board of the *American Journal of Surgical Pathology.* He will be responsible for contributions on the history of pathology. Dr. Rosen has also received an appointment as Associate in the University Seminar on the Renaissance of Columbia University.

Medical Historical Film

A new historical motion picture, *The Resurrectionists,* has recently been produced by the Yale University Media Design Studio in cooperation with the Yale School of Medicine. The film, which was shown at the Madison, Wisconsin, meeting of the American Association for the History of Medicine, considers the causes and history of grave robbing in England and Scotland from the late 18th century until passage of the Warburton Act in 1832. The film, produced by Thomas R. Forbes and Susan Wheeler and directed by Susan Wheeler, is especially designed to interest medical and nursing students, members of the health professions, medical historians, and biologists. The visual materials are authentic for the period, compiled from the Yale collections in history of medicine and other archives. The 15-minute sound film, in color, may be purchased for $150.00 or rented for $20.00 from the Yale University Media Design Studio, 305 Crown St., New Haven, Conn. 05420. Preview prints for purchase are avilable at the rental fee, deductible from purchase price if the film is purchased.

PROGRAMS OF MEETINGS

Handerson Medical History Society, Cleveland

March 23, 1977: "Fruitful Rivalries of Old Medical Schools in Cleveland," Josephine Robertson.

April 18, 1977: "The History of the Microscope," John Davidson.

Johns Hopkins Medical History Club, Baltimore

March 22, 1977: "Thomas S. Cullen," J. Donald Woodruff; "The Image of the Physician in the 18th Century," Diane Canala.

May 2, 1977: "Abel and Onward: The Early History of Hemodialysis," Barbara Gastel; "A War with the Molecules: Louis Pillemer and the History of Properdin," William D. Ratnoff.

Josiah C. Trent Society for the History of Medicine, Durham, N.C.

March 24, 1977: "Death and Disease in Renaissance Florence," Ann G. Carmichael.

Society of Medical History of Chicago

March 23, 1977: Annual Morris Fishbein Lecture. "Benjamin Rush, Man for the Third Century," Robert Erwin Jones.

April 20, 1977: "Memorial to Morris Fishbein," Dwight J. Ingle and Walter L. Palmer.

University of Virginia Medical History Society, Charlottesville, Va.

March 22, 1977: "Benvenuto Cellini and His Syphilis: Malevolent Mercurial Cure?" Glenn W. Geelhoed.

Washington Society for the History of Medicine, Washington, D.C.

April 7, 1977: "Medical Student Research Contributions; The Story of the Severely Gifted," Harold Stevens; Two Films: "Charles Darwin" and "The Physical Basis of Inheritance."

May 21, 1977: "Broken Bones in Prehistoric, Ancient and Modern Times," J. Lawrence Angel.

Washington University History of Medicine Club, St. Louis

April 5, 1977: "Reform of Medical Education: Harvard University," Kenneth M. Ludmerer.

May 5, 1977: "Influence of Disease on Military Campaigns," Royal J. Eaton.

William Snow Miller Medical History Seminar, Madison, Wis.

December 8, 1976: "The Self-image of American Physicians," Daniel M. Fox.

February 21, 1977: "Health and Society in Early Modern Europe," Andrew B. Appleby.

March 23, 1977: "Slavery and Disease: Were Blacks Medically Different from Whites?" Todd L. Savitt.

NEWS FROM ABROAD

Cambridge, England

The Wellcome Unit for the History of Medicine, a Sub-Department of the Department of History and Philosophy of Science, University of Cambridge, was established in 1971 on funds furnished by the Wellcome Trust. The Unit offers a range of undergraduate courses as well as instruction for those specializing in history of medicine for the M. Phil. degree in history and philosophy of science. The Wellcome Trust offers fellowships in history of medicine, some tenable at the Unit. Inquiries should be sent to Dr. R. K. French, Director, Wellcome Unit for the History of Medicine, Free School Lane, Cambridge CB2, 3RH, England.

Germany

At its September 1976 meeting in Mainz, the Council of the Deutsche Gesellschaft fur Geschichte der Medizin, Naturwissenschaft und Technik elected to corresponding membership Dr. Guenter B. Risse, Chairman of the Department of the History of Medicine, University of Wisconsin, Madison.

Greece

The Xth International Congress of Humanistic and Neo-Hippocratic Medicine will take place on the islands of Rhodes and Cos, 16-19 October 1977. The three sections of the Congress will have as general themes: "Deontology and Medical Ethics," "Physiology," and "Therapeutics." Participants wishing to present papers are asked to submit titles and summaries to the General Secretariat, Dr. S. M. Mantoridis, 5, Heracleitou str., Athens, 136 Greece. The official languages of the Congress will be only French and English.

London

The Royal College of Physicians Library has had on display an exhibit, "My Impossible Health or the Case of James Joyce." A catalog of the exhibit has been issued by the College, 11 St. Andrew's Place, London, NW1 4LE.

Paris

An Institute for the History of Medicine and Pharmacy has been created at the Université René Descartes (Paris V). Its aim is the establishment of efficient liaison between specialized libraries and curricula of the medical and pharmacy schools, certain Parisian centers of historical study and similar foreign bodies. As far as possible, the Institute hopes to serve as a center of information for researchers in the history of medicine and pharmacy, by means of a bureau and of seminars. The administrative council of the Institute is interdisciplinary in composition. Its general secretary is Dr. Marie-José Imbault-Huart. Correspondence is to be addressed to the director, Rector Pierre Huard, Service d'anatomie, Faculté de médecine, Cochin Port-Royal, 24, rue du Faubourg St. Jacques, Paris 75014.

Switzerland

The two-hundredth anniversary of the death of Albrecht von Haller is being commemorated in his native city, Bern, 6-8 October, 1977, with an international symposium, "Albrecht von Haller und seine Zeit." The Sekretariat for the organization committee of the symposium is the Deutsche Seminar der Universitat, Schützemattstrasse 14, Bern, Switzerland. The symposium will overlap with the 157th annual meeting of the Schweizerische Naturforschende Gesellschaft, 7-9 October, 1977.

Vienna

Prof. Dr. med. et phil. Erna Lesky, Director of the Institute of the History of Medicine at the University of Vienna, was awarded the Johann-Peter-Frank Medal by the "Bundesverband der Arzte des offentlichen Gesundheitsdienstes" of the German Federal Republic in Bad Durkheim on May 25, 1977.

BOOK REVIEWS

DOROTHY M. NEEDHAM. *Machina Carnis*: The Biochemistry of Muscular Contraction in its Historical Development. Cambridge: at the University Press, 1971. xv +782 pp. Ill. $55.00

Biochemistry has assumed the position of a separate science in recent years, but its roots go back to the philosophies of the ancient world. Man always speculated about his own body and how it functioned, and usually envisioned specific substances, sometimes called souls, spirits, humors, juices, whose interactions produced health or disease. Such speculations can be considered to be true biochemical concepts, even though the substances which produced the effects could not be known. Over the centuries, as more and more specific substances were isolated and studied, these speculations began to give way to observable mechanisms. This made possible a sequence of sciences in which actual bodily mechanisms could be used to explain observed facts, and so the sciences successively developed: physiology, physiological chemistry, biochemistry. This last, as a specific science was essentially a development of the twentieth century.

Since it was a new and rapidly developing science, its practitioners seldom felt the need to look into its past history. It was too recent in its concepts, most biochemists felt, to have a history, and they were too busy with their own researches to look into the older literature. Almost the only exception to this viewpoint was the publication of Fritz Lieben's classic *Geschichte der physiologischen Chemie* which appeared in 1935. Recently, however, biochemistry has reached a stage of maturity previously attained by the older sciences and a beginning has been made in considering its history. More and more articles concerning this are appearing in the historical journals, and books are beginning to appear tracing the antecedents and growth of modern biochemical concepts.

The book under review takes as its subject the historical development of one topic, the biochemistry of muscular contraction. It might at first seem that this would be rather limited in scope, but how wrong such a judgement would be! There is probably no other single topic which, if treated in detail, would cover so much of the essential biochemistry of today, nor are there many topics which could find an expositor such as Dorothy Needham. She has been involved as a major research worker in this field during almost the whole of its modern growth. She has been publishing important papers on the subject since the nineteen twenties. She has known the major workers in the field, and she is possessed of a historical sense which places them in their proper positions. The results of her researches, her evaluations, and her broad background have enabled her to produce a book which is almost frightening in its magnitude.

For a proper coverage of the topic of muscular contraction, consider what fields of biochemistry must be discussed: carbohydrate and protein structures, their detailed metabolism, the mechanisms by which they react, enzymes and coenzymes (including much of vitaminology), cyclic reaction chains, dynamic states, bioenergetics, respiration. Most of the important branches of biochemistry are included in the list, and most of these topics are treated in detail in this book. There is no mere skimming of the surface. Almost every important paper which relates any of these topics to muscle function is described thoroughly. Experimental results are reported extensively and the theories drawn from the experiments are evaluated. Over three thousand titles are listed in the biography, and Dr. Needham has obviously read and digested all of them during her long career.

She surveys the theories which held sway in each phase of development of each topic from classical or medieval days to the present. For example, after a preliminary review of the older theories of muscular contraction, she devotes a chapter to contraction mechanisms as accepted from 1939 to 1956. She considers five different groups of theories based on different assumptions, and gives the evidence for each. She then presents a lengthy chapter on the chemistry of myosin, actin, and tropomyosin. This leads to two long chapters on the sliding mechanism generally accepted today. Equally she gives attention to the chemistry and biochemistry of phosphate esters and the high energy bond, to the electrophysiology of muscle, to energy production and utilization, to such diseases as muscular dystrophy and glycogen storage diseases, to the comparative biochemistry of different types of muscle and the muscles of various vertebrates and invertebrates.

The magnitude of the work is amazing, no matter what aspect is considered: the 782 pages, the enormous bibliography, the rather startling price. The book is not merely a definitive history of the development of all phases of muscular activity, it is a survey of modern biochemistry itself. At the same time, its very magnitude will limit its circulation. This is not a book to read for recreation. It is so closely packed with facts, so well reasoned and logical in its conclusions, that it must be taken a little at a time. Few readers will be equally interested in all parts of the work. However, as a complete reference work it cannot be surpassed. Future research will go on from here, it will not replace this survey of the past. The book will probably not grace the shelves of many private libraries, but the knowledge that it exists and the opportunity to consult it will be essential for physiologists and biochemists for years to come.

Reviewed by Henry M. Leicester, Professor of Biochemistry, University of the Pacific Dental School, San Francisco, California.

Richard H. Meade. *In The Sunshine of Life: A Biography of Dr. Richard Mead 1673-1754*. Philadelphia: Dorrance & Company, 1974. 196 pp. $7.95

To Dr. Richard H. Meade is owed a debt of gratitude for the volume under review—the first full scale biography of Dr. Richard Mead (no relation), the eighteenth-century London physician, to appear in print. Dr. Mead's name is perhaps most notably connected with his collections. As a collector of books, objects of art, and antiquities, he was almost without parallel in the England of his day. Yet this is not to say his mark on his times derives wholly from these interests. A successful medical practice established Mead as the leading physician in London and earned his appointment as physician-in-ordinary to King George II. He gave lectures in anatomy at Barber-Surgeons Hall, served as governor in six major hospitals, and wrote a number of medical works, one of which (*Discourse on the Plague*) has earned him the title of father of public health in England. Dr. Meade's biography has filled a neglected gap in the medical history of this period.

Some highlights of Mead's career are related by way of introduction in the first chapter of the book. Notable are his role in the adoption of inoculation for smallpox in England and the establishment of the House of Hanover on the throne following the death of Queen Anne. The description of family genealogy and background, however, makes no mention of Mead's illustrious ancestor, the Reverend Joseph Mead.

This is followed with discussion of his education in the classics and medicine at Utrecht and Leiden, the Grand Tour of Europe with his brother and friends, and his early practice in his home town of Stepney. Meeting Dr. John Radcliffe helped Mead's career considerably. This prominent London physician took a liking to the young physician and became his patron. When Radcliffe died, Meade occupied his house in Bloomsbury Square and fell heir to much of his practice.

In the next chapter we are given a picture of the physician's private life. We learn of his two marriages, his children, his circle of friends, both medical and nonmedical. Among the latter was Samuel Johnson, who said that Meade "lived more in the broad sun-shine of life than almost any man." Through M. de Boze, secretary of the Academy of Inscriptions and Belles-Lettres in Paris, he exchanged gifts with King Louis XV. Not all of Mead's acquaintances could be counted as friends. The author relates his quarrel with the cantankerous Dr. John Woodward, remembered more for his contributions to geology than to medicine. A dispute stemming from a disagreement over treatment for a type of smallpox led to a duel with swords. Accounts vary, but both men survived it, and the issue was not resolved.

The third chapter focuses on Mead's medical writings and interest in the publication of learned works and humanitarian causes. Through the influence of his professor at Leiden, Dr. Archibald Pitcairne, he became an advocate of the iatromechanical school of medicine, evidenced in his early writings, though in later years he modified this position. No doubt his *Discourse on the Plague*, written at the request of the government when Marsailles, France, was in the throes of an epidemic of plague and there were fears it would strike England, constitutes his most important medical contribution. The author discusses this work at length. Mead's role in the establishment of the Foundling Hospital is also noted, showing his humanitarian concern for deserted children in an age that had little in the way of a social conscience.

Mead's collections are taken up in chapter four. His interest in antiquities was awakened when he went to Utrecht to study with the classical scholar Graevius, and we see signs of it as early as the tour of Italy following completion of his medical studies when he located the "lost" ancient *Tabula Isiaca* in a Turin museum. In later years his income permitted him to indulge his tastes for such objects, and he came to have one of the largest collections of paintings, art objects, antiquities, and items of scientific interest in England in his day. Among his more prized pieces were a second century B.C. bronze head of Homer (now believed to be Sophocles), two Watteau paintings done for him while he was treating the artist, and an original edition of the *Christianismi Restitutio* written by Michael Servetus, of medical as well as theological importance. The story of the goldheaded cane which Mead carried, a gift from Dr. Radcliffe, is related. The final chapter gives a brief discussion of the physician's last years and the author's evaluation of him utilizing handwriting analysis as a source of information.

The materials available for this biography are not extensive. The primary sources above and beyond the physician's own writings are mainly letters which don't reveal too much; the secondary sources are repetitious. Dr. Mead has made good use of what there is. The volume is enhanced by four appendices dealing with various topics such as Mead's anatomical library and family wills and by illustrations showing Mead's residences, hospitals with which he was associated, portraits. There are some mistakes, and they should have been caught. Examples include (historical) the reference to the Pretender's second attempt to seize the English throne (p. 26), Henry IV of England (p. 80); and (typographical) Pollbill and Pollett for Polhill and Pellett (p. 16), Myotonia for Myotomia (p. 79), Bulkley for Buckley (p. 81), Ogidius for Aegidius (p. 95). The errors do not, however, in this reviewer's opinion detract from the value of the volume.

Reviewed by ARNOLD ZUCKERMAN, Ph.D., Northeast Missouri State University, Kirksville, Missouri.

G. J. BARKER BENFIELD. *The Horrors of the Half-Known Life: Male Attitudes Toward Women and Sexuality in Nineteenth-Century America.* New York: Harper & Row. 1976. xiv + 352 pp. $15.00

What are serious historians to make of this book? Imaginative, original, in many ways unique, *The Horrors of the Half-Known Life* is also a persistantly annoying work—often ahistorical, frequently speculative, and only occasionally sensitive to the traditional principles of historical inquiry.

Benfield derives his central thesis from Alexis de Tocqueville's observations about the effect of American democracy on men and the relations between the sexes. No matter that Tocqueville's insights, suggestive as they are, have still been imperfectly studied by historians of the 19th century; Benfield accepts them as given. Democratic, industrializing America, he observes, subjected its men to increasingly relentless pressure to compete with one another. This ruthless, hostile, all-male world, which valued monetary success and individual self-reliance above all else, produced men particularly vulnerable to anxieties about their own sexual identities. Terrified by his own vision of the world, the representative American male masked his weakness in bluster and strength. "Hard," "cold and implacable," he set about to conquer the unknown. Acquisitiveness, constant motion, and the excited pursuit of chance characterized the American's subjection of the wilderness. But the frontier was not the only unknown, nor excessive striving the sole potential source of destruction Woman, too, became intrinsically threatening. Indeed, the very anxieties generated by American democratic conditions shaped this common suspicion of the female sex.

In an equal, competitive society, Benfield reminds us, men desperately needed a sharp delineation between the sexual spheres. Woman was put on a pedestal and removed from the world as a means of preserving stability in a helplessly disordered universe. Weak, subordinate, womanly women were a negative affirmation of men's masculine selves—whatever else their position on the social scale, at least they were not female. But women were dangerous for a more fundamental reason. In an era obsessively fearful of sex, when "to spend" sperm was to expend a vital, irreplaceable bodily energy, women, at the mercy of their bodies in a way that men were not, stood as a constant, menacing reminder of men's own insatiable sexual desires. Men remained safe, successful, and productive only when they managed to control their "passions" and subordinate their women. Benfield views the entire social history of 19th-century society through the prism of this dark, primeval struggle.

Such a thesis, though not particularly novel, leads Benfield to some ingenious distortions of the past. Apparently freed by his own a priori reasoning to pick and choose among the various bits of evidence, he remains undaunted by factors like chronology, data, statistics and methodology. For example, the evidence for his chapter on the decline of the midwife in the mid-19th century is drawn almost exclusively from one article (the excellent study by Frances Kobrin) detailing events which took place in the 1920s. To illustrate how the midwife controversy illuminates his (and Tocqueville's) observations about the behavior of American men in the 1830s Benfield requires, it seems, neither a strict sense of periodization nor additional data from the 19th century to prove his point.

It is, in fact, in his treatment of the American medical profession, symbolized for Benfield by the rise of surgical gynecology, that the author makes his most fantastic forays into the art of historical fiction. Although Benfield grudgingly admits that the emergence of modern gynecology "in some sense . . . may be construed to have advanced medicine," he prefers to see the development of the specialty as a conspiracy against women by hostile, insecure men. "The social breakdown following the Civil War," he asserts, "can be taken as a frame of reference for the rise of gynecology." In the *very year* of the Seneca Falls Convention, physicians like Dr. Charles Meigs were already plotting their attack on "female organs." Doctors, perhaps more than other men, increasingly perceived the disorderliness of women. "Body business," in fact, probably made physicians "even more sensitive to the 'sexual line' than other democrats." Indeed, "gynecologist's case histories are suffused with male anxieties over, and attempts to deal with women out of their place." Benfield's very language casts traditional gynecological advancements in a sinister light. James Platt White's "successful opening up of a woman's genitals to ocular observation," is juxtaposed with a particularly virulent protest by Dr. Augustus K. Gardner against Bloomers, feminists, and midwives. The AMA, the author points out, constituted an authoritative "Section" on obstetrics and diseases of women and children "in 1873, the year of the publication of Battey's invention of female castration." Even J. Marion Sims' founding of the New York Woman's Hospital comes under suspicion as "the first hospital for a specific physiological group, women." Sims' pioneering operation for vesico-vaginal fistula—which relieved thousands of women injured in childbirth and formerly condemned to a life of humiliating isolation—is termed a "particular cutting of a woman's body."

Benfield's generalizations about the medical profession are based upon a highly selective and idiosyncratic examination of the careers of just two men—J. Marion Sims and Augustus Kinsley Gardiner. Even allowing for the book's limited cast of characters, however, Benfield's Sims is a grotesque, imprisoned in a world of sexual fantasy. His interest in gynecology is portrayed as an outgrowth of both his hostility toward women and his ferociously competitive instincts. Indeed, Benfield claims, "women's generative tracts," became "the arena where he and his surgical rivals crossed swords for decades." Sims is pictured as incapable of purer or more complex motives. His founding of the Woman's Hospital, for example, is depicted as an effort to single out women as "human material" upon which he and his followers could "experiment." Yet Sims incurred much professional opposition when he proposed to establish a hospital devoted exclusively to women's diseases. Though European physicians had been concentrating for over a decade on the study of female problems, American doctors lagged behind. Many remained unconvinced that the treatment of women's diseases was a field sufficiently important to require the creation of a specialized hospital. It was only after Sims demonstrated that surgery need no longer be regarded as a last-resort measure in the treatment of fistula, cancer and sterility—that, above all, his pioneering methods yielded results hitherto undreamed of by doctor or patient—that skeptical colleagues ceased to consider Sims something of a charlatan. Benfield also insists that all Sims' hospital patients were destitute and ignorant, and consequently powerless to resist his aggressive surgical advances.Yet Sims' own vision for the Woman's Hospital was that it would be a place where "educated and cultivated women could go for relief when they had not the means to command it." No lady, he boasted, "is degraded by entering the Woman's Hospital." Its clientele included "the wives and daughters of clergymen, doctors, merchants, clerks, . . . college professors, teachers, druggists, artists, farmers . . . and many others of respectability and culture."

Benfield is ignorant of the history of medical therapeutics. His treatment of sexual surgery is a dreary repetition of former arguments. He produces little evidence to suggest that clitoridectomies were anything but rarities (the evidence he does provide—published, single case studies—are *by definition* oddities.) He ignores the fact that such procedures were opposed by a significant number of physicians. He has examined no cumulative hospital records to determine the incidence of ovariotomies done for psychological indications. (My own cursory investigation suggests that the overwhelming majority of cases were done for cancer, puerperal fever, or other types of infection.) Indeed, the author has no sense of the process by which gynecological surgery became a professional scientific discipline. Both Gardiner and Sims, and thousands of doctors like them, suffered from gross ignorance at mid-century. Sims himself had opposed ovariotomy as an act of "butchery" until he was convinced of its favorable results. His aggressive surgery was a means, admittedly primitive and tentative, of combating such ignorance. And thousands of women benefited from his boldness and temerity. Dr. Mary Putnam Jacobi, who, if Benfield were to be believed, might be expected to have opposed gynecological surgery, welcomed such advances and wrote to Elizabeth Blackwell in 1888: "When you shudder at 'mutilation' it seems to me you can never have handled a degenerated ovary or a suppurating Fallopian tube,—or you would admit that the mutilation had been effected by disease,—possibly ignorance or neglect of a series of physicians, before the surgeon intervened . . . . Why should not women be delighted if they succeed in achieving a difficult and useful triumph in technical medicine?"

Modern surgeons are still criticized for performing unecessary surgery on women. But gynecologists often argue that only the continuing professionali*zation* of medicine can provide the knowledge and high standards needed to end such practices. It may well be that the very progress of professionalization in 19th-century medicine helped sound the death knell to the worst abuses of sexual surgery. Dr. Howard A. Kelly, Professor of Gynecology at Johns Hopkins, indicated as much when he wrote in 1896 that "Conservatism . . . is undoubtedly the progressive spirit in gynecology: exsective and amputative gynecology has gone to its extreme limits, and the more thoughtful surgeons . . . have already sounded the keynote of the new advance."

One's disappointment with Benfield's failure to treat these topics adequately is all the more pronounced because he has raised some issues of crucial significance for our understanding of 19th-century culture. Perhaps for that achievement alone he should be commended. What was the nature and cause of the separation between the sexes in 19th-century society? Was this phenomenon a product of industrialization, or a carryover from "premodern" European-American patterns? Historians have begun to investigate 19th-century female culture; we need innovative, responsible studies of male culture as well. How did intense competition and democratic ideology affect men's perceptions of themselves? What role did class and ethnicity play in these self-perceptions? What was the relationship between sexual repressiveness and the pursuit of success? Did medical therapeutics reflect cultural anxieties, and if so, to what extent, and in what manner? What effect did the competitive atmosphere of 19th-century society have on the professionalization of medical practice?

Unfortunately, Benfield's cast of characters are not flesh and blood actors in the past. Their behavior is solely a function of their gender. They respond, not as doctors, lawyers, ministers, fathers, politicians, but exclusively and mechanistically as anxious males. Women, too, are merely figments of the male imagination. They exist solely to shape men's perceptions of reality. Let us hope that Benfield's idiosyncratic vision will not discourage others from asking the same questions, forewarned and forearmed, with a greater sense of responsibility to the evidence, and a keener understanding of what history is all about.

Reviewed by REGINA MARKELL MORANTZ, Ph.D., Department of History, University of Kansas, Lawrence, Kansas.

JOHN J. MCKELVEY, JR. *Man against Tsetse: Struggle for Africa.* Ithaca and London: Cornell University Press, 1973, xvi + 306 pp. Ill. $12.50.

In his book *Man Against Tsetse* John McKelvey, a member of the Rockefeller Foundation since 1945, has given us a comprehensive and fascinating account of tsetse fly and its devastating effect on cattle and man in Africa. Cattle succumbed silently to the sting by tsetse which transmitted the parasitic trypanosoma and deprived owners of much needed food and a means of transport. Long before the end of the 19th century, man in Africa and occasional explorers and travellers observed the disease and described it but they were unable to understand its cause until the beginning of more extensive contacts with the West. The struggle against tsetse sapped the energy of entire populations, burdened governments with expenses beyond their economic capacity to pay, attracted entomologists, biologists, physicians and successions of scientific researchers, and forced administrators to introduce social experiments in order to make the tsetse-affected villages comply with the changing strategies of the battle against the fly and the parasitic trypanosoma in man.

Having embarked on a subject with wide ramifications in many disciplines, such as medicine, the natural and social sciences, history and politics, the author avoided the style of a pedantic account of the taming of a pernicious killer. He succeeded in giving us a history of the slowly growing awareness of the causes of sleeping sickness and the unending frustrations which the scientific search for the control of trypanosomiasis and for means of separating the fly from man and cattle made necessary during the last eighty years. His book has the quality of an epic with many heroes. But beyond that, it is a scientific and historical essay on trypanosomiasis, tsetse, the natural habitat of Africa and a factual account of the state of tsetse and trypanosomiasis today.

The history of sleeping sickness is embedded in a complex nexus of scientific, socio-medical and administrative aspects of tropical Africa. To do justice to all of them is difficult, especially since, as the author emphasized in his preface, "the narrative springs from the massive amount of the material written about tsetse." In the course of the last hundred years the abundant material has been widely dispersed in government archives, scholarly journals, specialized reports and in the memoirs of those who actively participated in the struggle against tsetse. McKelvey has salvaged for the specialist and for the generalist the salient aspects of three major phases of the gigantic struggle of man and insect in tropical Africa. There is no straight line of development from one phase to the next. Europeans became aware of it around 1800 when naturalists, entomologists and collectors found specimens about which they reported. Among them were the British collector John Cranch in 1816, the Frenchman Count Pierre François Aimé Auguste Déjean and a German entomologist, zoologist and medical doctor, Christian Rudolf Wilhelm Wiedemann, who gave the fly the generic name glossina. The connection between fly and disease was made by Livingston during the middle of the 19th century. He had hoped to introduce legitimate trade into Central Africa as a means of driving out slave trade for ever. The loss of oxen and horses on his expedition concerned him and he concluded that a poison entered into the blood of big animals, such as oxen and horses, through the proboscis of the small fly. After 1870, with the establishment of colonial governments in hitherto unoccupied territories of East and Central West Africa, the second phase began during which attention was directed to epidemics incapacitating entire villages. Research teams of scientists and doctors were sent out, populations were shifted to "clean" locations, bush was burned, game was destroyed, but success was temporary at best. The third stage began with the use of chemotherapy. The book manages to include West, South and East Africa which are here tied together by a common fate, the struggle to abolish trypanosomiasis.

The chapters are arranged chronologically but as their titles indicate (The Curse of the Flies, Asking the Right Questions, Campaigns Against Tsetse and Trypanosomiasis, You Tell Me To Sit Still), they develop the theme of the book with all its contradictions and keep the reader in suspense. Not all chapters are even in quality. The last chapter which presents the institutional and organizational aspects of national and international trypanosomiasis research loses itself in recitation. But to dwell in depth on the ramifications of how to keep a dormant tsetse situation under control in today's third world economies and their changing emphasis on priorities, would require a separate book. Among the merits of the book is the author's ability to describe the struggle against tsetse as a whole with big strokes of the brush without sacrificing the scientific nature of his theme.

Reviewed by ANN BECK, Professor Emeritus of History, University of Hartford, West Hartford, Connecticut.

THE BULLETIN RECENTLY RECEIVED*

Chester R. Burns (Editor). *Legacies in Law and Medicine.* New York: Science History Publications, 1977. 310 pp. $7.95.

J. William Dawson. *Modern Ideas of Evolution.* Edited by William R. Shea and John F. Cornell. New York: PRODIST, 1977. 25 + 240 pp. $4.95 (paper).

Edward Ford. *Bibliography of Australian Medicine 1790-1900.* Sidney: Sidney University Press; Forest Grove, Oregon: International Scholary Book Services, 1976. xv + 348 pp. $40.00

Luis S. Granjel. *Literatura histórico-médica Española de postguerra (1940-1975).* Salamanca, 1977 (Cuadernos de Historia de la Medicina Española, Monografías XXX). 74 pp.

Carl Haffter. *Tagebuch des Zurcher Medizinstudenten Elias Haffter aus dem Jahre 1823.* Zürich: Verlag Hans Rohr, 1976 (Schriften zur Zurcher Universitäts- und Geistesgeschichte, 1). 72 pp. Ill. Fr. 9.80.

Huldrych M. Koelbing. *Arzt und Patient in der antiken Welt.* Zürich & München: Artemis Verlag, 1977 (Reihe Forschung und Deutung). 240 pp. Fr. 48.-

Francis Maddison, Margaret Pelling and Charles Webster (Editors). *Linacre Studies. Essays on the Life and Work of Thomas Linacre c. 1460-1524.* Oxford: Clarendon Press, 1977. liii + 416 pp. Ill. $26.50.

John J. Marchalonis. *Immunity in Evolution.* Cambridge, Mass.: Harvard University Press, 1977. xx + 316 pp. Ill. $18.50.

Bartholomaus Mettlinger. *Die jungen kinder wie man sy halten und erziehen sol von irer gepurt biss ey zu iren tagen komen* [Faksimile der Inkunabel "Ein regiment der jungen kinder," Augsburg: Hans Schaur, 1497]. Commentar von Peter Amelung. Dietetikon/Zurich: Verlag Bibliophile Drucke von Josef Stocker, 1976. 77 pp. [U.S. Distributor: Bernard M. Rosenthal, Inc., 251 Post St., San Francisco, CA 94108.]

Owsei Temkin. *The Double Face of Janus: Essays in the History of Medicine.* Baltimore: The Johns Hopkins University Press, 1977. x + 543 pp. $22.50.

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Medicine at Harvard

The First Three Hundred Years
Henry K. Beecher, M.D. and Mark D. Altschule, M.D.

Since 1782 Harvard Medical School has been associated with most of the world's developments in medicine, often either as creator or challenger This is a generously documented, illustrated account and assessment of Harvard's contribution to medical education and to medical practice. There are no chronologies of names, dates, and discoveries; instead the authors describe the Harvard past so as to reveal the trends leading to significant developments, and to define the ideas that made them possible. The emphasis is upon both the men themselves–in the classroom, in the laboratory, in the hospital–and the medicine they made. $27.50

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Translated and edited by Ernest Sachs, Jr., M.D. and Eva W. Valtin

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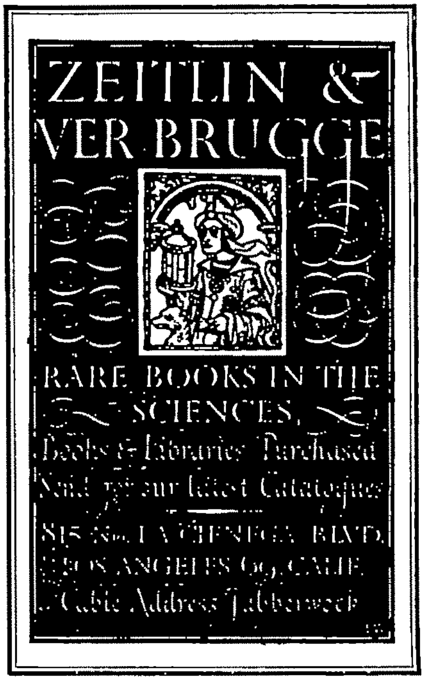

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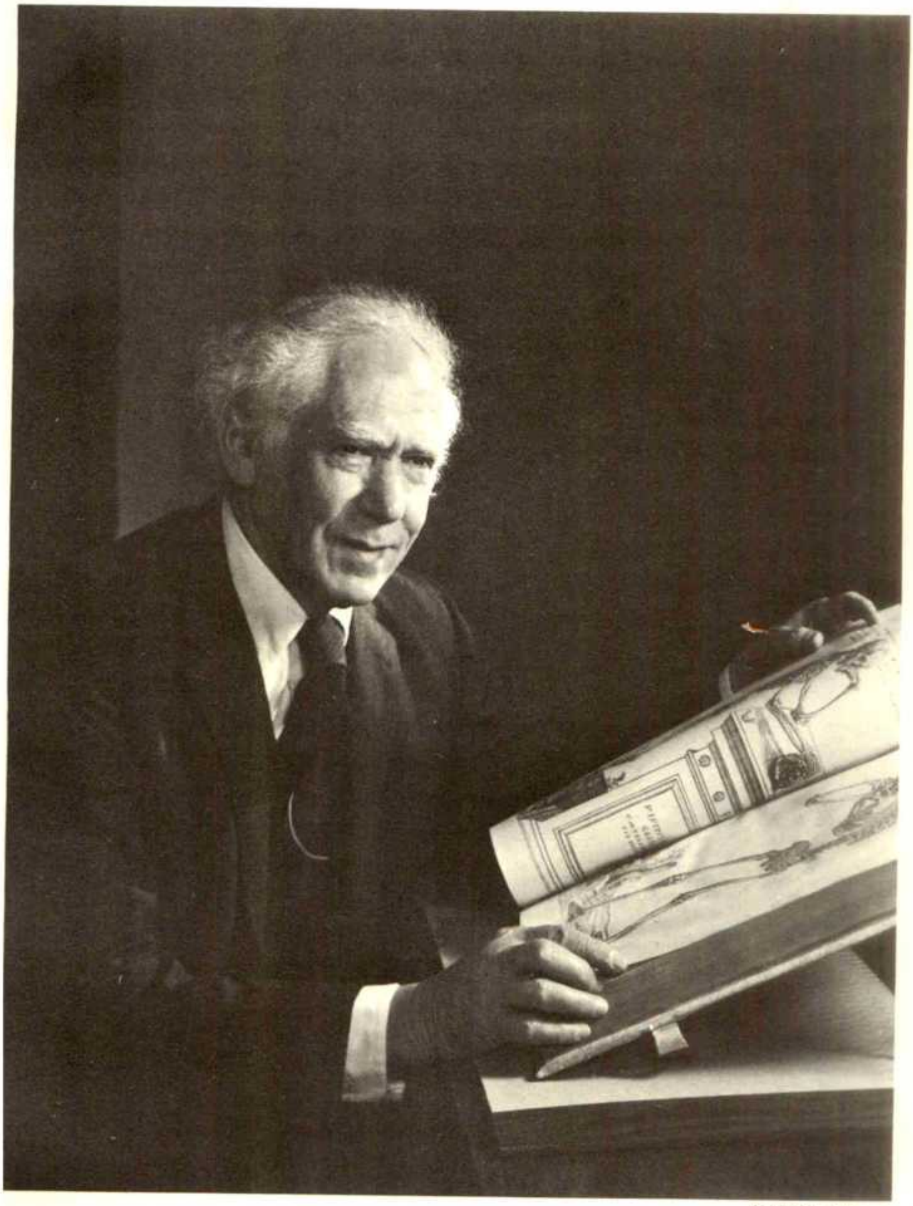
OWSEI TEMKIN AT SEVENTY-FIVE

Celebrating his seventy-fifth birthday on 6 October 1977, Professor Temkin will enjoy the hearty good wishes of scholars everywhere, and more especially those of medical historians around the world; but closer to home there is a smaller group of well-wishers who fully share the common respect for Temkin's scholarship and add to it their personal tributes of gratitude and affection. These, of course, are his immediate colleagues in the Institute of the History of Medicine, which for many years he directed and inspired, and the graduate students who took advanced degrees under his direction, students now located on East Coast and West Coast, and from Canada down to the Gulf. One and all, we acknowledge the Professor's kindness, generosity and rigor. We have found him a pretty tough customer—with a splendid sense of humor. We can testify, moreover, to his penetrating sight: he has never failed to see our faults, some of which, we like to believe, have withered a little under his eye; nor has he ever failed to see and recognize our scholarly virtues, even when to detect them was a somewhat exacting test.

Since he reached the emeritus stage, and subsequently retirement, he has published a second edition of *The Falling Sickness* and two first editions—his Messenger Lectures *(Galenism: Rise and Decline of a Medical Philosophy)* and earlier this year *The Double Face of Janus and Other Essays in the History of Medicine*. He is now pursuing, with exemplary vigor and exemplary calm, his old and far-ranging interest, irritability; we are evidentially sure that in one way or another he will overtake it, rope it and tie it. (On his seventieth birthday, one of us presented him with a Texas cattleman's hat.) But while he has shown himself well able to cope with a major, and massive, theme, he has also demonstrated that it is possible to write a cogent essay of no more than a dozen pages that will continue to point the way for decades. Needless perhaps to say, we have all fixed our eyes on some of the shorter Temkin papers, rather than on his books or his great comprehensive essays, as models for our brief contributions to this special Temkin number of the *Bulletin,* a journal he edited for more than twenty years; and needless to say, we have found these models both stimulating and dismaying.

Not without trepidation, we submit our "themes," requesting that irritability be shelved for a weekend. With no trepidation at all, and with great delight, we present a magnificent Temkin portrait from the camera of Yousuf Karsh. This portrait, we trust, will give pleasure to Owsei and Lilian Temkin and to their daughters, Ann and Judith, but in commissioning it we were also very confident that it would give pleasure to each of us—and to all who are in any way concerned with the history of medicine.

These gifts and remembrancers we offer gladly, gratefully and proudly—assured that we will be blithely joined in our good wishes by all the members of the American Association for the History of Medicine and by all the readers of the *Bulletin.*



KARSH *of Ottawa*

Professor Owsei Temkin

SYDENHAM AND THE MEDICAL MEANING OF "METHOD"

DONALD G. BATES*

At a meeting of the International Congress of the History of Science some years ago, Owsei Temkin introduced me to Professor Sambursky, the well-known historian of physics, as someone "who works with me" in the Institute at Hopkins. When I hastened to explain that I was really his student, the gentleman smiled indulgently and replied, "Everyone who works with Professor Temkin is his student."

As Temkin, who developed an early and sustaining interest in the work of Thomas Sydenham,[1] has observed, the efforts of Sydenham to revitalize ancient medical wisdom—efforts which won him the title of "the English Hippocrates"—achieve their meaning only by reference to their seventeenth-century context.[2] Nowhere is this more true than in his use of the word "method."

In his *magnum opus* of 1676,[3] one of the two things which Sydenham thought were necessary to enlarge the medical art was "a stable and perfected Practice or Method" for treating diseases.[4] At that time, and indeed from the later sixteenth century onward, this word "method" had great currency, and, unfortunately, had a great many different though often interrelated meanings. Moreover, the concepts and ideas, the disputes and discussions, which surrounded the term, bore a spectrum of significance from serious philosophical portent to faddish cant, as often typifies a word which has gained such wide popularity.

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[1] For early examples see, "Die Krankheitsauffassung von Hippokrates und Sydenham in ihren 'Epidemien,' " *Archiv für Geschichte der Medizin*, 1928, *20:* 327-352; "Thomas Sydenham und der Naturbegriff des 17. Jahrhunderts, *Neuburger Festschrift* (Vienna, 1928), pp. 287-295. Sydenham appears repeatedly in Temkin's subsequent writings.

[2] *The Double Face of Janus: And Other Essays in the History of Medicine* (Baltimore and London: The Johns Hopkins University Press, 1977), p. 14.

[3] Thomas Sydenham, *Observationes medicae circa morborum acutorum historiam et curationem* (London, 1676). Citations in this paper will be to *Thomae Sydenham, M.D., Opera Omnia*, ed. G.A. Greenhill, ed. altera (London, 1846).

[4] "Praxis seu Methodus circa eosdem stabilis ac consummata" (Greenhill, p. 10). The practice of capitalizing "Methodus" will be commented upon later. The other objective was to have "a History, or graphic and natural description of all diseases. . . ."

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Although it is the intention of this paper to focus on the use of the word "method" in a particular medical context, this cannot be done in isolation from the other meanings which the word conveyed. Since, however, these other meanings have been the subject of a recent, extensive scholarship, only a summary of their findings will be given here.[5]

The word itself comes from the ancient Greek, and so do the sources of its meaning and exposition. Plato (who, in turn, may have been influenced by Hippocrates), Aristotle, and Galen are the most prominent authorities. Their texts were subjected to the scholastic refinement of Arabic and medieval Latin commentaries, entering the centuries of the Renaissance as the basis for highly complex disputes and discussions about method.[6]

Out of the profusion and confusion of details, two lines of development in the sixteenth century have been detected. The one had to do with rhetoric and the teaching of the arts. The source of this tradition was Socratic and it concentrated on the correct way of conveying knowledge to someone else, that is to say, on education. The other is philosophical. Its roots were Aristotelian and its concern was with the proper steps to be carried out in demonstration and proof.[7]

The first of these, cultivated in particular by the Humanists, does not play a large part in our story beyond the fact that, through the innovations and enthusiasms of Peter Ramus, it enjoyed widespread use in seventeenth-century literature, including medical literature, and there may even be a trace of it in Sydenham's own work. There are two relevant aspects of this Ramian influence: the term "method" came to connote speed and efficiency in teaching; and this in turn frequently meant a resort to the spatial schematization of knowledge for its easier comprehension and assimilation. These influences gave rise to the "textbook" approach to the presentation of knowledge in printed books, books which categorized, divided, and spatially arranged their material for the convenience of readers who were trying to learn their contents.[8]

In the cases of some "textbooks" of medicine, too, it is this meaning of

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[5] For what follows, I am dependent chiefly on Walter J Ong, *Ramus, Method and the Decay of Dialogue From the Art of Discourse to the Art of Reason* (Cambridge Harvard University Press, 1958); John Herman Randall, Jr , *The School of Padua and the Emergence of Modern Science* (Padua: Antenore, 1961); and Neal W Gilbert, *Renaissance Concepts of Method* (New York Columbia University Press, 1960).

[6] Ong (n 5 above), p 153, Gilbert (n 5 above), Ch. 2, "The history of method as a philosophical term "

[7] Gilbert, p. xxiv f

[8] This development is treated at length by Ong See also his review of Gilbert's book in the *Bull Hist Med.*, 1961, *35*· 386-388.

"method" which is conveyed. Thus, in 1662, the indefatigable Culpeper brought out a translation of Felix Plater's work, *A Golden Practice of Physick . . . After a New Easie and Plain Method.* In a note to the reader about the "Order or Method" of the work, it is explained that the book is arranged by symptoms rather than by diseases. The same connotation is undoubtedly meant by George Starkey when he complained about all the treatises which he had "to digest into that method they are in. . . ."[9]

Even more of a fad was the custom of representing medical information schematically.[10] This practice is carried to its ultimate by Edward Edwards who provided a book entirely comprised of tables of schematic diagrams of fevers.[11] It is of passing interest, in this regard, that, while Sydenham was not generally given to this fashionable use of the term, there is a diagram of fevers in one of his manuscript notebooks which may owe something to this tradition.[12] And it would seem that it is this didactic meaning of the term which is to be understood by a contemporary remark that Sydenham's mentor Cox "encouraged and put him into a Method to study Physic at his return to the University."[13]

The other line of development has to do with the philosophical question of demonstration and proof. This would seem to be more promising for the study of Sydenham's thought, if there proved to be a link between that older tradition and the modern concept of "scientific method," since historians have had a longstanding interest in Sydenham's place in the development of the latter.[14] But, that there is such a connection between that form of

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[9] *Natures Explication and Helmont's Vindication .* (London, 1657), p. 252.

[10] See Karl Joseph Höltgen, "Synoptische Tabellen in der medizinischen Literatur und die Logik Agricolas und Ramus'," *Sudhoffs Arch f. Gesch d. Med. u. d Naturwiss ,* 1965, *49* 371-390. Ong (n. 5, above), p 315, says that this approach encouraged the related fad of performing intellectual "anatomies" or "dissections" of bodies of knowledge

[11] *The Cure of All Sorts of Fevers* . (London, 1638). Edwards did not, himself, use the term "method," but a testimony in the book, by Alexander Read, begins, "Methodicum istum de Febribus tractatum . . . ," (p. [55])

[12] The Vaillant Manuscript notebook of Sydenham's observations from 1669 to 1674. in the Library of the Royal College of Physicians, London, f 7^{v} For a detailed description of this manuscript, its dating, and a discussion of this diagram, see D. G Bates, "Thomas Sydenham: The Development of his Thought, 1666-1676," doctoral dissertation, The Johns Hopkins University, Baltimore, 1975, pp 308-339.

[13] Anthony Wood, *Athenae Oxonienses. To Which are Added, the* Fasti *or Annals of the Said University* . , 2nd ed. (London, 1721), vol 2, col. 840.

[14] Most students of Sydenham, in fact, have at least commented on this, sometimes at great length See in particular R. M. Yost, "Sydenham's philosophy of science," *Osiris,* 1950, *9* 84-105; David E. Wolfe, "Sydenham and Locke on the limits of anatomy," *Bull Hist. Med.,* 1961, *35* 193-220; Lester S. King, "Empiricism and rationalism in the works of Thomas Sydenham," *ibid.,* 1970, *44* 1-11; and *idem, The Road to Medical Enlightenment, 1650-1695* (London: MacDonald, 1970), Ch. 3, "Methodology."

Renaissance method and "scientific method," even in its seventeenth-century formulation, has been argued for[15] and against,[16] and this issue has probably not yet been entirely resolved.[17] It does seem, though, that Renaissance debates over method were often a mere wrangling over logical procedure, with an inescapable stamp of the scholasticism against which the seventeenth-century approach to scientific method was a reaction. Whatever their indebtedness to that older tradition, the efforts of a Galileo, and the concern of a Descartes or of a Francis Bacon, do appear to represent fresh initiatives and a break with that past.[18] Therefore, historians' discussions of scientific method, in Sydenham's time, seem justified in largely ignoring the older, scholastic, philosophical tradition.

It would seem, then, that historical interest in Sydenham which sees him as one of the "master-builders" of "mighty designs in advancing the sciences," to use John Locke's words,[19] has correctly centered on his seventeenth-century forebearers and contemporaries as the sources of his inspiration, men like Francis Bacon, Robert Boyle, and John Locke. There appears to be little justification for associating Sydenham in any very serious way with those older traditions of "method," rhetorical or philosophical.

Without wishing to deny those more contemporary influences, influences which are sometimes even explicit in Sydenham's work, I would like to suggest that there is a connection with that older literature. This arises from the fact that there was a third tradition of "method" which had its sources in much the same ancient writings, but which remained restricted to medicine. And, unlike the other two, this tradition clearly persisted in Sydenham's time and informed his own methodology.

We have already said that Galen was a prominent authority on the subject of method. Chief among his writings in this regard was the *Ars medica*, available to scholars over many centuries and still being actively used by Renaissance students of methodology. In this treatise, Galen's comments on the subject are prominent, yet ambiguous, providing fertile ground for commentary and the disputes between humanists and philosophers which are so typical of Renaissance literature.[20]

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[15] Particularly by Randall (n. 5, above), and to some extent by W P. D Wightman, "*Quid sit Methodus?* 'Method' in sixteenth century medical teaching and 'Discovery,' " *J. Hist Med*, 1964, *19* 360-376

[16] Largely by Gilbert (n 5, above)

[17] See William F Edwards, "Randall on the development of scientific method in the school of Padua—a continuing reappraisal," in *Naturalism and Historical Understanding Essays on the Philosophy of John Herman Randall Jr.*, ed. John P. Anton (Albany: State University of New York Press, 1967), pp 53-68.

[18] See Gilbert's concluding chapter (n. 5 above), esp. pp. 226-231

[19] *An Essay Concerning Human Understanding*, collated and annotated by Alexander Campbell Fraser (New York: Dover Publications, 1959), vol. 1, p. 14.

Another Galenic treatise, which also contributed to the subject, had the Latin title, *Methodus medendi*. This work was known to the Middle Ages in Latin translation, but circulated, in the Renaissance, chiefly in a translation by Thomas Linacre, published in 1519.[21] Mention of the *Methodus*, which was a work on therapeutics, reminds the historian of medicine that, within the confines of that particular subject, there is another very old tradition which involved methodology.

Admittedly, questions of both philosophy and procedure were raised within that medical tradition, but not with the sole object either of discovering new knowledge, or of education, but rather by the necessity of treating the sick. How much can we know about the human body in health and sickness? How much do we need to know to be able to treat the sick? How should we proceed to discover new remedies, to understand their action, and to determine with confidence that they are effective in various diseases? And, above all, what is the proper relationship between reason and experience in the development of a therapeutics? These are questions with which the ancients grappled and to which the various medical sects of antiquity gave different answers.[22] And it was to these very same questions, and to the philosophical views of those sects, that Galen also addressed himself, in the *Methodus*, and in other works.

It is apparent then, that, besides the philosophical and rhetorical traditions distinguished by modern students of Renaissance method, there is a third, a medical tradition of method which tried to unite theories of disease processes and drug actions to the practice of treatment, i.e. to create a therapeutics.

There are a number of reasons why this form of "methodism" has not been separated from the other two. For one thing, we are accustomed to thinking that, within medicine, Galen's influence was to eliminate those ancient disputes by reconciling the methods of the Dogmatists and Empiricists and banishing those of the Methodists.[23] But it is perfectly apparent that Galen had not, thereby, eliminated method itself from this special medical context. A commentary by Guillaume Budé, the editor of Linacre's translation of the *Methodus medendi,* puts the matter well. After observing that Galen had dismissed the views of the ancient methodists as "amethodical," Budé points out that Galen called his own approach a therapeutic method, hence the name of his book.[24] To the Renaissance, at least, the writings of Galen had not eliminated methodism from medicine but had displaced one form with another.[25]

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[20] For the Latin title and place of the *Ars medica* in Galen's writings, see Owsei Temkin, *Galenism: Rise and Decline of a Medical Philosophy* (Ithaca and London: Cornell University Press, 1973), p. 101. For its place in discussions of method, see Gilbert, pp. 16 and 19. A more technical discussion of it and commentators on it can be found in Edwards (n. 17, above), pp. 56-64.

[21] *Galeni Methodus medendi. Vel de morbis curandis Thoma Linacro Anglo interprete* (Paris, 1519). See Gilbert (n. 5 above), pp. 19-21.

[22] For Galen's most succinct discussion of these matters, see his "On the Medical Sects: For Beginners," in *Greek Medicine*, trans. and ann. Arthur J. Brock (London and Toronto: J. M. Dent and Sons, 1929), pp. 130-151. The philosophical foundations of these sects are discussed in *Ancient Medicine: Selected Papers of Ludwig Edelstein*, eds. Owsei Temkin and C. Lilian Temkin, trans. C. Lilian Temkin (Baltimore: The Johns Hopkins University Press, 1967), pp. 173-203.

And there were other things to becloud this persisting tradition of medical methodism. The Galenic branch, for example, clearly was not ancient methodism and was rarely if ever called "methodism." Moreover, even medical writers of the Renaissance helped to obscure this medical use of the term because they too used Galen's treatises, including the *Methodus medendi,* to contribute to the literature which discussed those other issues of philosophical and rhetorical method.[26] Finally, in the strictly medical literature itself, as we have seen, there was enthusiasm for using "method" in its textbook and schematic sense, as well as in its therapeutic sense. In this mosaic of method, it is not surprising that another pattern, a medical meaning and use has not been readily apparent.

The passages from sixteenth-century medical literature to which Wightman has drawn our attention,[27] are, it is true, of a more theoretical nature. They stick closely to methodology, and consist of exposition and commentary on Galen's ideas. They form part of the main trunk of Renaissance methodological literature but show the beginnings of a specialized medical branch because they retain a medical context and are aimed, in their substance and purpose, at a medical readership or audience.[28]

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[23] Just how complete a process this was, of course, is difficult to say See Charles H Talbot, *Medicine in Medieval England* (London. Oldbourne, 1967), p. 40f.

[24] Gilbert (n. 5 above), p 64, fn. 41, cites Gulielmus Budaeus, *Annotationes . in quatuor et viginti Pandectarum libros* . (Paris) I have consulted the 1532 ed., f 18.

[25] It could be argued that Galen, himself, merely replaced an inappropriate use of the term with one which better fit the classical sense of the teaching and discovery of an art, albeit the medical art Nevertheless, out of this medical context, the Renaissance developed a special, if not very precise, technical use of "Methodus" which, qualified often by "medendi" or "curandi" was synonymous with "therapeutics." John Locke, in his journals, used "M M," i.e "methodus medendi" in this way. See, for example, Kenneth Dewhurst, *John Locke (1632-1704) Physician and Philosopher* (London: The Wellcome Historical Medical Library, 1963), p 136. In the nineteenth century, when Latham was translating Sydenham into English, he frequently left "Methodus medendi" in Latin as a technical term fully familiar to his professional contemporaries Robley Dunglison's *A Dictionary of Medical Science* (Philadelphia, 1860), defines that phrase as "Method of cure, Therapeutics."

[26] Indeed, physician-philosophers were major contributors to these non-medical discussions.

[27] Wightman (n 15, above), p 375f

Leading off from this trunk, the main branch is made up of works which retain a substantial amount of commentary on method as such, but which also aim at being books of "practical," i.e., clinical medicine for the practitioner.[29] Further out, our metaphorical branch is composed of patently "practical" works which tend to be perfunctory about the study of method, itself, but use the word repeatedly as an equivalent of therapeutics—a theoretical framework into which is set actual treatment regimens.[30] Finally come the many twigs of particular methods, or routines of approach to treatment, dividing off in all directions like the sectarian branches of some religion.[31]

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[28] As Wightman has pointed out Nevertheless, if I understand him correctly, Wightman keeps his analysis within the framework of the philosophical tradition of method which he relates, tenuously, to the origins of "scientific method." In my opinion, he fails to appreciate the special context of therapeutics which, even in these writings, is apparent. For example, he dismisses Galen's *Methodus medendi* in one sentence (p. 367) and, it seems to me, misses the therapeutic context of the long passage he quotes from Manardi (p. 371)

[29] This is even true for the Manardi cited by Wightman, at least for the 1549 ed. which I have consulted. In 1558, Alfonso Bertocci wrote a book entitled *Methodus generalis, et compendiaria, ex Hippocrates, Galeni, et Avicennae placitis deprompta, ac in ordinem redacta. Ad omnes morbos recta ratione curandos summe utilis, et necessaria* . . (Lyon). By 1588, the title had become *Therapeutica, sive Methodus generalis medendi, ex Hippocratis* [etc.]. The following year this appeared in an edition with a work of Jean Riolan, père, which itself (according to Richard Durling, *A Catalogue of Sixteenth Century Printed Books in the National Library of Medicine* [Bethesda. National Library of Medicine, 1967], item 3880) had appeared in 1578 under the title *Generalis methodus medendi*, being a commentary on Fernel's Therapeutics (Riolan had, at that earlier date, incidentally, given six lectures "on classical philosophy" later printed in an edition by Gabriel Naudé (1631) entitled, *In artem parvam Galeni commentarius*, and one of those lectures was on Peter Ramus' work)

Girolamo Capivaccio, 1523-1589, wrote a number of works bearing titles such as *Nova methodus medendi* . , 1593; *Methodus, seu ars curativa*, 1594; *Practica medicina, seu Methodus cognoscendorum et curandorum omnium humani corporis affectuum*, 1594, and *Methodus practicae medicinae* . , 1597. It was this author to whom Daniel Sennert turned for authority for his definition of "the Method of healing" as being "an art wherein by Indications remedies are found out that the lost health of man may be restored" (*The Institutions or Fundamentals of the Whole Art, Both of Physick and Chirurgery . . . Also the Grounds of Chymistry* , made English by N D.B P [London, 1656], in Bk. V, Pt II "Of the Method of healing," p. 348). Incidentally, Capivaccio also wrote a book called *De methodo anatomica liber* . . (Venice, 1593), which clearly does not refer to the special technical sense of "method" employed in connection with therapeutics. The above titles represent only a small sample of those which could be cited, to say nothing of the books that used "method" in a therapeutic context within their texts but not in their titles

[30] Three examples in three languages, representing both medicine and surgery: Thierry de Héry, *La méthode curatoire de la maladie vénérienne, vulgairement appellée grosse vairolle, et de la diversité de ses symptomes* (Paris, 1552), Guillaume Rondelet, *Methodus curandorum omnium morborum corporis humani Eiusdem de dignoscendis morbis, de febribus, de morbo Italico, de* [*medicamentis*] *internis et externis* (Paris, 1576), and J. Macallo, *XCIX Canons or Rules, Learnedly Describing an Excellent Method for Practitioners in Physick* (London, 1659)

While the precise shape of these branchings could not be determined without a very close study of this literature, it is clear that they unmistakably represent a genre which involves a uniquely medical use of "method" in the sense of therapeutics. The object of such literature is neither discovery nor teaching alone or for themselves, but one or both in the service of treating the sick. Moreover, as one moves out to the more distant branches, the study of method in itself largely gives way to the championing of one method over another.

Before relating these developments to the work of Thomas Sydenham, it is necessary to say a few more words about the sorts of things which occupied the minds of those authors who wrote this type of literature.

There is, first of all, some connection between this use of "method" and the controversies of the ancient sects. Terms like "Dogmatist" and "Rationalist" are commonly encountered, although I have found the term "Empiricist" infrequently, and, when it does occur, it is in a context meant to refer to an "Empiric," which is the customary English rendering.[32] By contrast, "Methodist," in the sense of the ancient methodists, plays almost no part.[33] On the other hand that term and its cognates do appear, and appear with some frequency, as the name for Galenists. A methodist or methodical doctor is someone, trained in the schools, who follows Galen's method of treatment, in other words, his therapeutics.[34]

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[31] Again three examples Wolfgang Geuss, *Methodus curandorum morborum mathematica* i e [astrologica] . . . (Frankfurt, 1613), Simeon Partlicius, *A New Method of Physick. Or a Short View of Paracelsus and Galen's Practice* . . , transl. N Culpeper (London, 1654), and [Humphrey Blunden], *The Method of Chemical Philosophie and Physick Being a Brief Introduction to the One and a True Discovery of the Other* . . (London, 1664).

[32] For an example of the use of "Galenists" interchangeably with "Dogmatists," see George Thomson, *Galeno-pale or a Chymical Trial of the Galenists* . (London, 1665) The more formal sense of "Empiricist" appears to be intended in a lay passage which comments on someone's comparison of "a man that wanted Science, and had experience, to the Empirick Physician, and the learned man to the Methodicall . . . ," (Virgilio Malvezzi, *Considerations Upon the Lives of Alcibiades and Corialanus* . Englished by Robert Gentilis [London, 1650], p 50) The use of "Methodicall" here reflects Renaissance usage See also the discussion of indications as "the first instrument of Dogmatic Physick," and their relation to the use of experience, in Sennert (n 29 above), p. 349. Appeal to the Greek terms is made and there is an allusion to the "other Sects."

[33] This is not to say that the ancient sects are not mentioned or discussed Galen's own comments are, of course, broadcast far and wide Through them, reference to the ancient sects turns up in the vernacular of practical literature as in the case of Guy de Chauliac, *The Questyonary of Cyrurgyens* . *with the Fourth Boke of the Terapentyke, or Methode Curatyfe of Claude Galyen* . [trans from the French by Robert Coplande] (London, 1542), Sig. 2G; Prosper Alpino, in his *De medicina methodica libri tredecim in quibus medendi ars methodica vocata olim maxime celebris, quae hac aetate . desiisse visa est, denuo restituitur, atque in medicorum commodum quadantenus ad medicinam Dogmaticum conformatur* (Patavia, 1611), means the ancient sect, as the title makes clear. Partlicius (n 31, above), pp 6-8, reviews the three sects and

Obviously this Galenic methodism also has something to do with the issues raised by the ancient sects, since they were his point of departure. But in the two centuries under consideration, debates over method in medical practice varied greatly in the faithfulness and consistency with which they reflected this past.

Meanwhile, contemporary events and intellectual trends were having their own effects. For instance, the Renaissance fondness for routinizing and systematizing, along with the growth of popular medical literature, gave vent to simple, if not simplistic formulae of Galen's methodism. "The Methodick practising in Physick," as one typical formulation puts it, "hath, first, a knowledge of the Disease: next foretelleth the event of it: and last, goeth about to cure the same."[35]

More important to the contemporary scene was the clash going on between Galenists on the one hand, and Paracelsians, Helmontians, and Chymical Physicians on the other. Here methodology was frequently overshadowed by the quarrel over which method or routine was the better, given that the opposing sides had different views about the nature of disease, the best remedies, and the nature of their effects. In their attack on the Galenists, for example, opponents might ridicule *Galenical* method, but they did not ridicule *method* as such. That is to say, they insisted that they too

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adds the "Hermeticks" as a fourth. This appears to be largely a perfunctory review, as is even more clearly the intention of Samuel Sorbière and his *Avis à un jeune médecin* (Lyon, 1672). (See F L. Pleadwell, "Samuel Sorbière and his *Advice to a Young Physician*," *Bull Hist. Med* , 1950, *24* 255-287, pp. 272-275)

[34] See, for example, George Starkey (n 9, above) A later part of the title of his book reads, *Being a Necessary and Full Apology for Chymical Medicaments . Against Those Unworthy Reproaches Cast . by Galenists, Usually Called Methodists* At one point (pp 9-11), Starkey says that Galen took the art of medicine and "digested the same into a Method, whereby this Art might afterwards be communicated by verbal tradition in scholastic lectures and disputations " The Schools then bestowed the title of Doctor upon students who had "sucked out . Galen's Marrow and learned his Method, so as to be able to read a Lecture or make a Commentary upon him . ," thus illustrating how the term "method" could shift from one sense (therapeutic—used throughout most of the book) to another (didactic—as in this passage). There is here almost an overlapping or double sense which connects teacher and practitioner, in much the same way as has happened with the word and the social institution of "doctor" itself.

The *Oxford English Dictionary* gives, as its first historical definition of "methodist," a physician "of the 'methodic' school . . in the 17th c sometimes applied to the regular or orthodox medical practitioners of the day, in contradistinction to those who favoured the use of new remedies."

[35] J. Macallo (n. 30 above), p 1 For a similar formulation by a critic of Galen, see Starkey (n 9 above), p 51

practise the healing art within a theoretical framework and from an understanding of disease and treatment.[36]

What is important here is that the debate reveals as much about the social value of having a method, of treating methodically, as it says about the benefits of one method over another. In its simplest form, this social value was expressed in the opposition of orthodoxy to quackery. The Galenist, a learned doctor, possessed a theoretical framework in which to understand, explain, and defend his therapy. This distinguished him from the empiric who merely collects and prescribes remedies without benefit of this learned understanding. The orthodox physician has a *Methodus medendi,* a therapeutics. The quack does not.[37]

The story of method, as it pertained to therapeutics in the sixteenth and seventeenth centuries, is obviously more complex than the brief outline given here. I have attempted only to draw attention to three things. First, besides philosophical and rhetorical developments, there is a medical meaning of the term which deserves separate attention even though, at times, it overlaps the other two. Second, like them, this use has roots in ancient medicine and maintains definite links with that past. Like them, it also acquires new emphases and connotations by virtue of contemporary events and thoughts. And, like them, its connection with the epistemological issues of the new science may be philosophically close, at times, yet historically separate by virtue of context and purpose. And third, this medical meaning of method can illuminate our understanding of the work of Thomas Sydenham, a point which I shall now try to demonstrate at greater length.

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[36] See references cited in n. 31 above, as well as Starkey (n. 9 above), as well as Thomson (n. 32 above). Also see *idem, A Letter Sent to Mr. Henry Stubbe Wherein the Galenical Method and Medicaments . . . are Offered to be Proved Ineffectual . . .* (London, 1672). Stubbe is quoted (p. 10) as referring to "the publick Method of Physick" and Thomson is accused of "illegal Methods" (p. 13). Thomson replies, setting off "the true Methodical Chymist" from "Anomalous Methodists" (p. 22), and insists that "no fixed great Disease can be cured, but by the Method and Medicaments" of Paracelsus and van Helmont (p. 24).

[37] Thomson (n. 32 above), p. 9, complains to Galenists that "any silly fellow may read your Dispensatory, use some of the Medicines without your Method," and calls such as do, "illiterate Ideots." Elsewhere (*idem, 'Ορθο–μέθοδος ιατρο–χυμική . Or the Direct Method of Curing Chymically . . .* [London, 1675]) he calls them "plain, candid unmethodical people" (p. 119). Starkey (n. 9 above), sig. $a_3{}^{v}$f., takes pains to assure the reader of his orthodox education and that his opposition "to the usual Method of Medicine" does not arise from any unfamiliarity with it. Still "the Doctors say of me that I am a Mountebank and want method . . ." (p. 327).

It is likely that it is this social value of method which shows up in sixteenth-century French surgical literature. See Héry (n. 30 above), ff. [$a_v{}^r$-$a_{vi}{}^r$]. He suggests that empirics should more properly be called

Armed with this sense of "method," for example, we immediately recognize the significance of the title of Sydenham's first book, *Methodus curandi febres,* London, 1666. Indeed, Sydenham himself tells us what this title suggests: he was stimulated to become a medical author by virtue of his concern for the reform of therapeutics. In fact, it was this reform which motivated the major part of what he wrote over the rest of his life.[38] Whatever our interest in his concepts and descriptions of diseases, or his contributions to scientific thought in medicine, etc., he consciously and explicitly saw his work as a part of that discussion which attempted to advance the healing art beyond the *Methodus medendi* of Galen. As was mentioned near the outset, one of Sydenham's major objectives was "a stable and perfected Practice or Method" for treating diseases. In fact, Sydenham's efforts at improving bedside descriptions and "histories" of diseases, his study of epidemics, and even his species notion were taken up to serve that end.

Moreover, the direction which he wished that reform to take was away from a Galenic methodism, which required a knowledge of the hidden causes (themselves the subject of so much debate in Sydenham's time), to what Sydenham felt to be a Hippocratic methodism which eliminated this need, provided that one added certain refinements, such as Sydenham's own brand of epidemic constitutionalism, a species notion of diseases[39] adapted, most likely, from the Helmontians,[40] and a system of repeated trials of remedies which can be traced through Galen to the ancient Empiricists.[41]

Despite his reforming efforts, however, a conservative element in Sydenham's program also becomes clear. While there were, in Sydenham's time, almost as many "methods" of therapy as there were authors, the classic, Galenical form underlay most of them, and this is true for

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"Thessaliques," and thus, while advocating Galenic method, associates the name of the ancient Methodist, Thessalus, with quacks. Jacques Guillemeau (*La chirurgie françoise recueillie des antiens medecins et chirurgiens* . [Paris, 1594]) uses "method" extensively but defends "les remedes Empiriques, que detestent tant les methodiques Medecins" (p. 133). Nicolas Abraham de la Framboisière (*Les canons requis pour pratiquer methodiquement la chirurgie* [Paris, 1595], esp sig. A_4^rf) is most insistent that surgeons should have a therapeutics.

[38] Bates (n. 12 above), pp 5-8, and Ch. 8, "Sydenham's Therapeutic Reforms and the Species Notion," esp pp. 203-215.

[39] *Ibid* The various ways in which these notions served his therapeutics are dealt with throughout the dissertation For his appeal to the therapeutics of Hippocrates, see pp 212-218, for the nature of his constitutionalism, Pt. II: "The Development of His Theory of Constitutions," pp 61-104; for its role in his therapeutics, pp 224-234, for the role of species in this reform, all of Ch. 8, but esp pp 218-224.

[40] I hope to enlarge upon this point at a later date

[41] For Sydneham's views see *ibid*, pp. 242-246. The similar views of ancient Empiricism are discussed by Galen in Brock (n 22 above), p. 133 Galen's sympathy with this approach is discussed by Temkin, *Galenism* (n 20 above), p. 15

Sydenham. Such methodism involved a physiological intervention in the body's own processes for the purpose of getting out the morbid cause. Therapy was dictated by the "indications," a thoroughly scholaticized version of Galen's rendering of dogmatist doctrine, whereby the aims of treatment were divined from disease symptoms and an understanding of bodily processes.[42]

This essentially Galenic methodism also puts Sydenham's interest in specific medicines in a different light. Possibly under the influence of the Helmontian opposition to Galenic method, Sydenham also granted a place to "specifics," and gave that term the special meaning of drugs that destroy the disease cause in the body without expelling it.[43] Besides his well-known belief in the specificity of diseases, therefore, Sydenham also expressed great interest in the possible discovery of more specifics, regarded them as superior to methodical treatment, proclaimed cinchona an example of a "specific" in his special sense, and spent a good deal of time and effort searching for other specifics. But, apart from cinchona, all these efforts failed,[44] and consequently his books are limited to methodical treatment.[45]

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[42] A Galenic discussion of the Dogmatists and their "indications" is found in Brock (n. 22 above), p. 135 For the ancient Methodists' version of "indications," and how they differ from the dogmatic, see *ibid.*, p. 140 f A good example of the scholastically refined (and practically unintelligible) discussion of indications can be found in John Jonston, *The Idea of Practical Physick . . .*, Englished by Nich. Culpeper (London, 1657), Bk IV, "Of the Method of Healing in General," p. 1 f

The *Oxford English Dictionary* offers, as an historical definition of "Method," ". . that part of Physick whereby remedies are found out by the Indications. . . " This is of interest in that the work cited for this, E[dward] P[hillips], *The New World of Words: Or a Universal English Dictionary*, 5th. ed (London, 1696), is the only edition of that work containing this medical definition. Editions of 1658, 1662, 1663, 1671, 1678, and, later, of 1706, do not. Starting with the 1671 edition, however, Phillips listed those people who gave him expert advice. For the 1696 edition, and only for that edition, one of the two medical consultants listed is Thomas Sydenham!

[43] For talk of killing, as well as driving out, "the Venom," see George Thomson, 1665 (n. 32 above), p 60. A decade later, and just a year before Sydenham's *Observationes*, Thomson (n. 37 above), the Helmontian, used a number of concepts and interrelated them in ways which have striking similarities to (though also important differences from) Sydenham's thought. not only specifics which destroy the morbific cause in the body (sig A_4^r), but the likening of diseases to plants (p. 21), the uniformity and classifiability of diseases (p. 20), their "substantial being" (p. 17), etc.

[44] Greenhill (n 3 above), pp. 18-20. Sydenham's early interest in and efforts to discover specifics is expressed in a letter to Robert Boyle, April 2, 1668, quoted by Kenneth Dewhurst, *Dr Thomas Sydenham (1624-1689) His Life and Original Writings* (London. The Wellcome Historical Medical Library, 1966), p. 164. For an entirely different sense of a "specific" see Owsei Temkin, "Fernel, Joubert, and Erastus on the specificity of cathartic drugs," in *The Double Face of Janus* (n. 2 above), pp 512-517.

[45] Andrew Broun, the disciple of Sydenham's last years, wrote that, after Sydenham had tried out and evaluated "the common and received Methods of curing most diseases," he gave them up in favour of his own "more aposite and powerful ones" *(A Vindicatory Schedule Concerning the New Cure of Fevers* [Edinburgh, 1691], p. 82 f.).

Seeing Sydenham situated squarely in the mainstream of medical methodism, we also see his essential orthodoxy. However much a reformer, however much an advocate of the value of new experience over old tradition, Sydenham was a "methodical" physician concerned with the reform from within, not with an overthrow of professional medicine.[46]

Perhaps of more importance to the student of Sydenham, his orientation to therapeutics and its longstanding tradition renders it moot as to just how much emphasis we ought to give to Sydenham's references to Bacon, or his words of appreciation addressed to Boyle and Locke, or just where and when we should try to integrate his ideas with the new science. For example, an English rendering of the full title of his first work would read "Thomas Sydenham's Method of Treating Fevers Built Upon his Own Observations."[47] Despite the suggestiveness of the latter half of the title for notions related to scientific method, and despite the fact that the book is dedicated to Robert Boyle, the whole work is an exercise in the methodical treatment of fevers. There are none of the brilliant descriptions of diseases or epidemics to come later. His "own observations" refer largely to his own therapeutic experiences, especially with smallpox.[48] In the very first paragraph, as befits a methodical physician, he promised to set forth two Principles upon which his method is built, since "my therapy is not totally empirically based, but depends . . . upon the solid support of reasoning."[49]

Another example carries a significance which reaches well beyond Sydenham. In one of his rare, published comments on his friend, John Locke, Sydenham said to Mapletoft, "you know how much I have support for my Method from one who has examined it very intimately in every respect, . . . Master John Locke."[50] The infrequency of such comments provokes our eagerness to know precisely what it was that Locke supported. Others have assumed that something was being said about Sydenham's epistemology, his "scientific" method.[51] I believe that he was referring to his "*Methodus medendi,*" which is elaborated at such length in the rest of the book.[52]

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[46] This, I think, is important to the understanding of Sydenham's popularity in his own day and his posthumous success in the eighteenth century Furthermore, I believe that his methodism was exploited, in that next century, to ends quite different from those of the nosographers to whom he is usually linked. See Bates (n 12 above), Ch 10, "Summary and Conclusions," pp 292-298 I hope to expand on this theme later.

[47] *Thomae Sydenham, Methodus curandi febres Propriis observationibus superstructa* (London, 1666)

[48] Bates (n 12 above), pp 15-33

[49] Id quod eo libentius facio, ut palam fiat therapiam nostram non esse prorsus empirice institutam, sed ejusmodi quae solidis rationum fulcris innitatur, ," (Greenhill [n 3 above], p 39)

[50] Nosti . quam huic meae methodo suffragantem habeam, qui eam intimius per omnia perspexerat, . Dnum Johannem Lock (*Ibid*., p. 6) In the original 1676 ed , "methodo" is capitalized, as was commonly the practice at that time. See Gilbert (n. 5 above), p 52, fn. 22

* * *

If the above analysis of "method" has any conclusion, it is that that term, as used in the sixteenth and seventeenth centuries, had, not two, but three sides to it: teaching or persuasion in rhetoric, cultivated by the Humanists; knowing or discovery in philosophy, cultivated by the Schoolmen; and doing or practice in medicine, cultivated by the physicians. Like the others, medical method had its own literature and its own protagonists.

At the same time, it must be admitted that two or more of the meanings of method can be found fused together, as was true in Galen's own works. Medical practice, after all, had to be taught and the frontiers of knowledge of treatment had to be extended; physicians were frequently doctors, and also philosophers; and so on.[53] The historian, teasing out medical method as a separate filament, cannot help but be conscious that he is introducing artefact, that he is over-dissecting, in order to make one part of the organic whole of the Renaissance mind visible to its twentieth-century counterpart.

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[51] See, for example, François Duchesneau, *L'Empirisme de Locke* (The Hague: Martinus Nijhoff, 1973), p 4f. For the passage in question, Duchesneau has used the Latham translation of Greenhill's text, a translation which tends to obscure the technical use of the term, in this instance.

[52] The significance which one accords to this distinction depends upon two things to what extent Sydenham influenced Locke's thinking; and how much difference there is between the tradition of therapeutic methodology, to which Sydenham is responding, and Locke's epistemological interest in the new science.

The relation of Sydenham to Locke I hope to explore later, in greater depth So far, I am struck by the differences in thought between the two men, differences which might be partly accounted for by the fact that they are responding to two different traditions of methodology, taken up for two quite different purposes

As for the second point, whatever connection there may be between therapeutics as a methodology and the development of scientific method, it is doubtful that therapeutics can be considered strictly in scientific terms, or its history subsumed under the history of scientific endeavour Even the theoretical part of therapeutics is probably incapable of being regarded as a "science," in our sense, since the two conditions which it mediates, disease and health, are not susceptible of value-free definitions See Owsei Temkin, "Health and Disease," and *idem*, "Therapeutic trends in the treatment of Syphilis before 1900," in *The Double Face of Janus* (n. 2 above), pp. 419-440 and 518-524, respectively. For other difficulties, already appreciated by the Greeks, see *ibid* , "The scientific approach to disease: specificity entity and individual sickness," pp. 441-455.

[53] Even some of the definitions of medical method exhibit this fusion, if not confusion "The Method of Healing," Jonston tells us (n 42 above), "is *an Art of inventing*, by help of indications . ." the remedies suited for healing Even the Phillips definition, likely inspired by Sydenham, similarly alludes to Method in its

If linking "method" to Sydenham has any wider meaning, it is that our awareness of the medical tradition of that term situates him more clearly in the context in which he was thinking and writing. Conversely, his work demonstrates the continuing viability of this tradition in the late seventeenth century and offers the example of a man, who, while deeply preoccupied with that tradition, was also associating with men like Boyle and Locke, and being called, by the latter, a "master-builder" of the new science.

That Sydenham's thought is chiefly evolving *from* its classical and Renaissance antecedents, rather than heading *towards* its nineteenth- and twentieth-century descendents, comes, however, as no surprise to anyone who has been a student of the writings of Owsei Temkin.[54]

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philosophical sense of a way of invention or discovery, when he says it is that part of medicine "whereby remedies *are found out* by the Indications. . . ." (Italics added in both quotations.)

[54] See, for example, "A Galenic model for quantitative physiological reasoning?" and "The classical roots of Glisson's doctrine of irritation," in *The Double Face of Janus* (n. 2 above), pp. 162-166 and 290-316, respectively

THE CALIFORNIA ORIGINS OF THE JOHNS HOPKINS MEDICAL SCHOOL

GERT H. BRIEGER*

The 1870s in America are usually viewed by historians as a period of optimism and self-satisfaction. The ruinous Civil War was over, the West was “won,” and travel and communications between Atlantic and Pacific shores became much simpler and quicker with the completion of the Transcontinental Railroad in 1869. James Bryce called California in many respects the most striking state in the whole Union.[1] The West, Frederick Jackson Turner said in 1896, was a form of society rather than merely a geographical area. “It is the term applied to the region whose social conditions result from the application of older institutions and ideas to the transforming influences of free land. By this application, a new environment is suddenly entered, freedom of opportunity is opened, the cake of custom is broken, and new activities, new lines of growth, new institutions and new ideals are brought into existence.”[2]

Carey McWilliams has pointed out that California largely skipped the frontier phase of settlement of her agricultural lands and her cultural institutions generally.[3] As far as medical education was concerned, the newly established American society in California in the decade after the Civil War was far from being a backwoods, frontier outpost. One indication of California's medical sophistication at this time was evident in May 1871 when the American Medical Association held its twenty-second annual meeting in San Francisco. As the president, Alfred Stillé, put it, “That one of the largest of the learned associations of the country should hold its sessions where, a quarter of a century ago, the words science and art had never been pronounced, appears miraculous.”[4]

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[1] James Bryce, *The American Commonwealth*, 2 vols (New York, 1914), vol. 2, p 426. There are many editions of this book that first appeared in 1888.

[2] F. J Turner, “The problem of the West,” *Atlantic Monthly*, 1896, *78* 289-297, p. 289

[3] Carey McWilliams, *California. The Great Exception* (Santa Barbara. Peregrine Smith, 1976), p 39. Originally published in 1949.

[4] Alfred Stillé, “Address,” *Trans A M A* , 1871, *22*. 75-98, p. 76. Much of the address dealt with medical education. In 1872 the AMA elected Dr Thomas Logan, pioneer California public health officer and member

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One might say that Turner's cake of custom came close to being crumbled. While reform did not actually proceed at quite such a revolutionary pace, it is fair to say that ideas and practices of medical education in California were as advanced as in any state of the Union. One can further argue that much of the reform that we have come to associate with Daniel C. Gilman, William H. Welch, and their Baltimore colleagues in the 1890s, had already found clear expression in California twenty years earlier.

In 1870, a bare two decades after statehood and just one year after completion of the railroad, San Francisco was a bustling city of 149,473 people. Its first medical school had been founded by the forceful and imaginative Dr. Elias Cooper in 1858. After his death in 1862 the school soon foundered, and in 1864 several of its faculty joined with Dr. Hugh Toland in a new school he began in that year.[5] Toland, a man of no mean entrepreneurial talent but a rather mean spirit, apparently, played a leading role in the medical educational affairs of the city for the next decade. With his own funds he erected a commodious three-story building on Stockton and Chestnut Streets across from the County Hospital. Here, the Toland Medical College flourished until 1870. In that year a split between the original Cooper men of the University of the Pacific and the Toland men became irreparable. It was partly owing, no doubt, to the wish of Toland and some of his colleagues for an affiliation with the young University of California, founded by an act of the legislature just two years previously, that prompted the old Cooper men to revive their own school.[6]

Hugh Toland, whose name and actions figured very prominently in the

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of the University's medical faculty, as its president. Logan was thus the first Californian so honored. R Beverly Cole held the office in 1895-96.

[5] A fair amount has been written about the early medical education in California, none of it in any great detail. This paper is a part of a much larger project, just beginning, that will be a history of the University of California's San Francisco campus. Just thirty years ago W. F. Norwood published his "The beginnings of medical education in California," *Bull. Hist Med.*, 1947, *21:* 760-771. See also Emmett Rixford, "Early history of medical education in California," *Ann Surg.*, 1928, *88*: 321-332; William B. Thompson, "The story of the professorship of obstetrics at the University of California," *Pacific Coast Ob & Gyn. Soc Trans*, 1949, which contains a very good history of the early period. For general history of California medicine see Henry Harris, *California's Medical Story* (San Francisco: J W. Stacey, 1932), very informative but to be used with caution. My colleague, J. B deC. M. Saunders has written extensively about California medicine. See especially his Garrison Lecture, "Geography and geopolitics in California medicine," *Bull, Hist. Med* 1967, *41* 293-324.

[6] This greatly oversimplifies a complex pattern of professional tensions and rivalries that existed in San Francisco in 1870. The state also enacted legislation to establish a board of medical examiners Whether the state university or the medical societies would control its membership was one bone of contention The Cooper Medical College eventually became the medical school of Stanford University

early annals of the University of California's first medical school, was born into a large family in South Carolina. In 1828 he graduated with a medical degree from Transylvania University of Lexington, Kentucky. Returning to his native state, Toland in a short period earned enough to join the young American medical men who made the pilgrimage to Paris. Here he studied surgery on the wards of the great Parisian hospitals and in two years returned to South Carolina where he seems to have practiced medicine and from a financial point of view, at least, he was immensely successful.

It was not until 1852 that he made his way west, presumably bitten by the gold bug, though the incubation period seems to have been a bit long in his case. He quickly gave up mining as an unsuccessful venture and set himself up in the practice of medicine in the rapidly growing young city that had but recently changed its name from Yerba Buena to San Francisco. It is said that he was a man of very positive opinions, some surgical skills, and certainly of single-minded dedication to his work, the practice of medicine. He used the Wells Fargo messengers to carry his advice and medicines up to the mining country of the Sierra Nevada and ran a drug store in conjunction with his busy office on Montgomery Street. His favorite remedies, apparently kept in two barrels behind the counter, were labeled "anti-scrof" and anti-syph"; they were readily and widely dispensed. Whether it was his surgical skill or what his contemporaries called "Toland's Luck" that was involved, the facts were that he had, for that period, extremely successful results. Mortality was low and dollar income incredibly high, reaching by 1860 to $40,000 a year according to one biographer.[7]

That he desired fame as well as fortune is not surprising. According to R. Beverly Cole, one of Toland's medical colleagues with whom he feuded furiously in the late 1850s but on whom he had to rely as dean of his medical school in the early '70s, Toland was a man *sui generis:*

> I have never known anybody much like him. He was marvellously successful in his practice as physician and surgeon, certainly so considering his outlandish and unprofessional methods. By this I mean that he appeared to have no respect for the accepted rules, though practising at variance with them, he achieved often the most happy and startling results. He was supremely selfish, having no regard whatever apparently for the feelings of other professional men; this made him naturally unpopular with them. . . .

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[7] Frances Tomlinson Gardner, "The little acorn Hugh Huger Toland, 1806-1880," *Bull Hist Med* , 1950, *24*. 61-69. "Anti-scrof" was iodide of potash, "anti-syph" was mercury with a dash of lobelia, according to Gardner. An obituary of Toland pointed out that, "He was one of the few members of the medical profession who die rich." *Pacific Med. & Surg. J.*, 1880, *23*: 477-78 Another said he cut for stone-seventy times with only three failures *West Lancet*, 1880, *9* 49-53, p. 51

COLLEGE

> Toland was a man of limited intellectuality, he possessed no broad views nor great capacity. . . . He gave less time to a patient than any man I ever knew, attending to 40 or 50 daily. . . . He had great ambition to have, or to be credited with having, the largest practice on the coast, consequently, in order to swell the number of his patients he did not hesitate to treat a great many without compensation, unless perhaps he did make sure of his profits by selling them drugs.[8]

No matter how we assess the founder of the Toland Medical College, it was a successful, debt free, and moderately enlightened school. Toland served it as professor of surgery until his death in 1880.

The first formal indication of the Toland Medical faculty's desire for affiliation with the University came in a letter from Dean Thomas Bennett to the Board of Regents of 6 June 1870, discussed by the Regents at their June 21st meeting.[9] On that same day, apparently without his knowledge, they elected Daniel C. Gilman president. Gilman was pleased to learn of this honor, but pleaded unfinished business at Yale and did not accept the position until he was elected overwhelmingly for a second time in 1872.[10]

In the meantime, the Regents voted to approve the affiliation and accepted Dr. Toland's generous gift of property worth at least $75,000. On 2 August 1870, the Regents resolved to create the Medical Department of the University of California.[11] All went well with the proposed merger, apparently, until November 1870. At the Regents meeting early in December, the Minutes record a letter from Dr. H. H. Toland of 14 November 1870 in which he agreed to transfer title to the Toland Medical College property to the Regents, ". . . provided it shall be called the Toland Medical Department of the University of California, and that should the name be changed or the building used for any other purpose then the property shall revert to myself or my heirs." The Regents promptly voted to repeal their August resolution, finding Dr. Toland's demands unacceptable.[12]

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[8] Typescript, Dictation of Dr. R. Beverly Cole, 12 Sept 1889, Bancroft Library, University of California, Berkeley. The story of the original cause for a fight between Cole and Toland over the care of a prominent patient in 1856 is told by George D Lyman, "The sponge," *Ann. Med. Hist* , 1928, *10*· 460–479

[9] Regents Papers and Correspondence, Box #3, University Archives, Bancroft Library and Minutes of the Board of Regents, University of California, ms. in the office of the Secretary of the Board of Regents, vol 1

[10] Letter, Daniel C Gilman to Gov H. H. Haight, 27 July 1870, Gilman Papers, vol. 1, U. C. Archives. "I am strongly tempted to accept the unexpected and gratifying proposal that I should become identified with the great institution which California has established on so liberal a basis for the promotion of the Higher Education." But he did not wish to sever his ties with the Sheffield School at that time.

[11] Board of Regents, Minutes The Board also resolved on this day to establish a Board of Medical Examiners of the University of California who would examine all students for a medical diploma. The San Francisco Medical Society sent a letter to the Regents, duly noted in the minutes, urging that the University's Medical Department be limited only to examining candidates for degrees, and that the Medical Department not be a teaching institution as well

Toland's own faculty, moreover, seems to have been in entire agreement with the Board of Regents, for the minutes of 7 March 1871 record a letter signed by the entire medical faculty minus the professor of surgery, resigning the positions to which the Board of Regents had previously elected them: "After the acceptance of his terms by the Regents," the faculty claimed, "Dr. Toland refused to convey the property except on conditions that were not only new, but entirely inadmissable." And so things stood for the next two years. San Francisco had two medical schools, both with only a handful of students, and the University was left with a Board of Medical Examiners but no medical school.

The motives on the part of the University for such a merger are quite clear. Medicine was one of the subjects specified by the Organic Act of 1868 that created the University.[13] Since graduate education in the sciences at that time was just beginning in schools such as the Lawrence at Harvard and the Sheffield at Yale, a common preparation for a scientific career was still the medical degree. The University of California faculty was typical in this regard. Of eleven professors in the Colleges of Arts and Letters in 1870, three held degrees in medicine. The brothers John and Joseph LeConte and Ezra Carr not only played leading roles in the University in those early days, but John LeConte and Carr received special permission from the Regents to teach in the Medical Department in San Francisco during the summer term. Thus their interests in affiliation were obviously direct.[14]

The motives on the part of the Toland Medical faculty are more obscure. The prestige of affiliation with the state's university was certainly one aspect. Toland's wish for a permanent monument probably served as an equally strong incentive.[15]

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[12] Board of Regents, Minutes, Dec. 6, 1870. The vote was 11 to 1.

[13] The Organic Act is reprinted in Verne Stadtman, ed., *The Centennial Record of The University of California* (Berkeley: Univ of Calif Press, 1967, pp 378-82. For histories of the University see William C Jones, *Illustrated History of The University of California* (San Francisco. F. H Dukesmith, 1901); William W Ferrier, *Origin and Development of the University of California* (Berkeley: Sather Gate Book Shop, 1930), and most importantly, Verne A. Stadtman, *The University of California 1868-1968* (New York: McGraw-Hill, 1970). All discuss the medical school.

[14] The same may be said of The Hopkins just a short while later. H. Newell Martin and Ira Remsen had earned medical degrees. Joseph LeConte expressed the sentiment of many scientists with similar training when he recalled that, "I did not expect to practise I was independent, and had studied medicine mainly as the best preparation for science " *The Autobiography of Joseph LeConte* (New York: D. Appleton, 1903), p. 104 LeConte, a Georgian, graduated with the M D. degree from the College of Physicians and Surgeons in New York City in 1845. His brother John had preceded him there by four years

Daniel Coit Gilman, loyal son of Connecticut and of Yale, was 41 years old and a recent widower when he heeded the second call from the Regents of the University of California in 1872. Gilman had been teaching geography at Yale, and was also deeply involved in the affairs of the Sheffield Scientific School, which he served as secretary. Though still relatively young, Gilman already had a national reputation as an educator.[16] His close friend, Andrew Dickson White, had also recently assumed a college presidency, at Cornell.

Late in October 1872 Gilman arrived at the University of California, then located at the old College of California campus in Oakland. What he found was a University with a good state law, 160 acres of land in Berkeley, modest funds, and immodest expectations.[17] A mere ten days later Gilman delivered his inaugural address, "The Building of the University." California, "queen of the Pacific," so richly endowed by nature is to build a university. What shall it be? he asked. It was to be a university, not a high school, or a college, nor an industrial or technical school. A university needed to incorporate some facets of each, but it was more comprehensive. Its success, Gilman maintained, would depend primarily on its faculty, thus foreshadowing a future Hopkins motto of "men, not buildings."[18]

Gilman warned that just as those who were devoted to divinity resisted the encroachment of the humanities, now those devoted to the latter were too often ready to close the doors on the sciences: "Such barriers are no longer defensible. Science and Humanities, nature and man, are now alike recognized as the best interpreters of Divinity."[19] The purpose of the University, Gilman concluded on that inaugural day in November 1872, was to fit young men for high and noble careers, satisfactory to themselves and useful to mankind. He quoted George Berkeley, for whom the new college town had recently been named:

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[15] The Toland Medical Faculty would, with merger, have the best of both worlds. They would no longer have to pay for the repairs to their building, but they would maintain the proprietary financial arrangements as before. Student fees were divided by the faculty In the late summer of 1874, for instance, the school's resources were listed as $2,437 21. On Dec 16, 1875 each faculty member was awarded a dividend of $350. Minutes of the Faculty Medical Department of the University of California, Archives, U C S.F. These early minute books began as the record of the Toland Medical College. While they do contain evidence of the merger negotiations, the Regents Minutes and University Archives have fuller details.

[16] The two standard biographies of Gilman are Fabian Franklin, *The Life of Daniel Coit Gilman* (New York: Dodd, Mead, 1910), and the less detailed *Daniel Coit Gilman*, by Abraham Flexner (New York: Harcourt, Brace, 1946), both of which allude to the embryo of the Johns Hopkins Medical School in California For Gilman as a geographer see John K Wright, "Daniel Coit Gilman, Geographer and Historian," *Geographical Review*, 1961, *51* 381-99. Much of Gilman's work and thoughts about education are in two volumes of his addresses· *University Problems in the United States* (New York, 1898), and *The Launching of a University and Other Papers* (New York. Dodd, Mead, 1906).

[17] Stadtman, *op cit* (n 13 above), p 34

[18] Daniel C Gilman, *The Building of The University* (San Francisco, 1872). This was a 29-page pamphlet It was reprinted as "The University of California in its infancy," in *University Problems in the United States, op cit* (n 16 above), pp 153-185

> Westward the course of empire takes it way
> The four first acts already past,
> A fifth shall close the drama with the day;
> Time's noblest offspring is the last.

Although the faculty minutes of the Toland Medical College reveal that they continued to confer with the Board of Regents, affiliation did not come until the spring of 1873. Dr. R. Beverly Cole, Professor of Obstetrics and politically as wise and adept as his colleague and one time antagonist Toland was cold and awkward, left the University of the Pacific's faculty and joined the Toland group in 1871 and soon became the dominant spokesman.[20] Cole wrote to Gilman in early March 1873 telling him that the merger of the medical school with the University could now proceed with the " . . . assurance that the chief donor, Dr. H. H. Toland, has expressed to me in writing his cordial assent to this action. . . ." This time Toland would accept the professorship of clinical surgery making no further demands.[21] If his faculty had won him over, he was not terribly gracious about it all. Prior to merger, the medical faculty minutes reveal that he attended and presided over almost every meeting. After the merger of his school as the Medical Department of the University of California, he did not attend a single faculty meeting for over thirteen months, although he continued to teach.

The year 1873, one of economic hardship for many in the United States, was of singular importance to the young University of California. Barely four years old, it was already a thriving institution of which the people in the state were inordinately proud. "Our University" was the common form of reference. In 1873 the University moved from the old buildings it had inherited from the College of California in Oakland to a spacious new campus along the low coastal hills just to the north in the town called

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[19] Gilman, "Building," *op cit* , p 14.

[20] Cole was an important figure in San Francisco medical circles He played a prominent role on the Toland and then the University's faculty until the end of the century See especially the long article by Frances T. Gardner, "King Cole of California," *Ann. Med Hist* , 1940, 2 243-58, 319-47, 432-42; and Thompson, "The story of the professorship of obstetrics," *op. cit* (n. 5 above).

[21] Board of Regents, Minutes, 4 March 1873.

Berkeley. The first class of graduates, twelve in number, finished its four-year course that June, and the ceremonies were held in Berkeley before the University actually began its operations on the new site. Professional education in pharmacy and in medicine also became an integral part of the University in that eventful year.

"The Medical Department is upon us—with plans of organization, etc.," Gilman wrote to his friend Andrew White in Ithaca in March.[22] This was but one item in a long list of events and problems he described to White. "I want to see you here, to get your counsel and your help," he added plaintively. But as far as medical education was concerned, Gilman needed little help, it seems. It was, in fact, his persistence and tact that enabled the merger to become a reality. During the reorganization phase of the medical school Gilman on several occasions journeyed across the Bay by ferry boat to preside at faculty meetings. He also presided at the opening exercises and graduations until he left for Baltimore two years later.

A collection of Gilman's California addresses on medical education, as well as the various comments he left in the papers and correspondence now in the University's archive, all clearly reveal his keen interest in the subject. A longer school year, admission of women students, emphasis on the advancement of science, proper preparation for the study of medicine, and the relationship of the school of medicine to the University so as to strengthen both, all claimed his attention at this time.[23]

At the opening exercises of the Medical Department of the University of California on 3 June 1873, Gilman spoke to the large and distinguished gathering (including the Governor, City Officials, the Regents, and U.S. Supreme Court Justice Stephen J. Field) on the subject of the relation of the

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[22] Letter, D.C.G. to A.D W., 15 March 1873, Gilman papers, vol. 1, U.C. Archives.

[23] There is a four-page set of suggestions for the Medical Department dated April 1, 1873 in Gilman's handwriting in Regents Papers and Corresp., 1873, CU-1 Box 2, U C. Archives, that poses most of the questions at issue in American medical education in the last three decades of the century.

Time and again when one delves into the subject of medical education in the years after 1870 one encounters the problem of the proper preparation for the study of medicine. This is particularly the case when one reads Gilman and John Shaw Billings Gilman in 1878 noted that one wag had suggested the degree F.S.M., "fit to study medicine," might be appropriate. One could, I suppose, add F.P M., "fit to practice medicine" and by means of revamping the initials, revamp the whole process. Because I believe that the pre-medical curriculum has not been properly studied in its historical context and because medical educators, so-called, have too long neglected this phase of a physician's education, I have begun a larger study of pre-medmanship and its attendant problems

Alan Chesney, "Two documents relating to medical education at the Johns Hopkins University," *Bull Hist. Med.*, 1936, *4*: 477-504; p 488 Several of the Billings essays in Frank B. Rogers, comp., *Selected Papers of John Shaw Billings* (Medical Library Assoc., 1965), deal with medical education, as do "Two papers by John Shaw Billings on medical education, "*Bull. Hist. Med.*, 1938, *6*. 285-359

University to the community. "The University of California cannot have a rival," Gilman proclaimed. It must fulfill its expectations of perpetuating the knowledge of the past as well as cultivating the pure and applied aspects of the sciences. "A college is not chiefly a mental armory, nor an intellectual storehouse, nor an educational tool shop, nor an enjoyable dining-table with its 'feast of reason and flow of soul.' It is a place for the manufacture of men, a place where all the experience of the world should be centered for the formation of character, for the development of latent powers, for the bringing out of higher nature, for the maturity of the judgement and the acquisition of wisdom."[24] Now comes the medical department Gilman, noted. What shall we hope for its future? By implication its goals were to be similar to those of the university it was joining. The medical diploma, Gilman said to the applause of his distinguished audience, was to serve as a public guarantee that ". . . men have labored long, have studied well, have observed closely, and have fitted themselves for the difficult and delicate professional duties which are to devolve upon them."[25] The scheme of medical education was to be comprehensive and would take into consideration many of the points that Gilman would raise again for the next thirty years. But Gilman, while in California, also heard the opinions of the state's physicians as well as those of his medical faculty.

The *Western Lancet*, a journal decidedly favorable to the endeavors of the Toland Medical College, carried an editorial in 1872 urging the University of California to organize a medical department with high standards of admission: "The best institutions always attract the best students. To have a large proportion of incompetent students is a public confession of incompetent teachers."[26] This was, of course, Gilman's belief as well. In his *Statement of the Progress and Condition of the University of California*, prepared for the Regents just before he left for The Hopkins, Gilman reported that efforts of two committees to organize the medical school on a satisfactory basis had not been entirely successful. "Both left the medical department behind the other departments of the University, in its standard of requirements for admission, when it should be decidedly in advance."[27]

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[24] "Introductory exercises of the Medical Department, University of California," *Western Lancet*, 1873, *2* 374-79; p 377.

[25] *Ibid.*, p. 379. Ironically, the May issue of this medical journal had reprinted the letter of March 10, 1873, from Johns Hopkins to the President and the Trustees of what would become the Johns Hopkins Hospital.

[26] Editorial, "Medical education," *Western Lancet*, 1872, *1*. 160-170; p. 167 The editor was confident that, "If the University of the State of California had a well organized medical department, there can be little doubt that it would very quickly become the leading institution of the kind on the Pacific coast, . . . " p 169

[27] Berkeley, 1875, p. 43

The general intellectual climate among California physicians in the 1870s was probably fairly accurately reflected by the various faculty members of the two San Francisco medical schools. Their medical colleagues in the Medical Society of the State of California also spoke out on problems of medical education. Since many of these men were not actively involved with teaching, their views may be even more representative of the profession as a whole. If this is so, at least two of them may be judged to have been as enlightened as some of their leading eastern colleagues. In 1871, for instance, Dr. Edward R. Taylor of San Francisco published a long paper in the *Transactions* in which he answered the AMA presidential address given that year by David W. Yandell of Kentucky.[28] Yandell questioned the usefulness of German methods and proposed a more utilitarian approach for American medical education. It is of interest that in the Far West, the land of rugged utility, its physicians should react so strongly. If Yandell's views were accepted, Taylor wrote, it would prove fatal to the advancement of medicine as a science. "Were medical men confined to the bedside, and the bedside alone, the years to come would find every practitioner a mere mechanician and routinist, whose ideas of medicine would never range beyond the walls of a drug shop. It is research, or a study of its results, that can alone enlarge our views, and lift us to those heights whence we can take a philosophic survey of the phenomena of life."[29]

Three years later, in a short report to the State Medical Society, Dr. H. A. Dubois of San Rafael praised the recent changes in medical education instituted at both Harvard and Yale. He had special praise for Yale because it had introduced a better preparatory course for medicine. "If to Yale's preparation course we add Harvard's system of medical instruction," Dubois waxed enthusiastically, "and cap both by the practice in a large hospital . . . We believe we come nearer to a perfect education than a few years ago seemed practicable, at least, for a long time to come. . . ." "Our own university is guided by these ideas. . . ," he was very pleased to note.[30]

It is no wonder then that in the midst of the men of this relatively young medical society, Daniel C. Gilman, fresh from Yale, and now of their own state, found a congenial welcome. In 1873, the year that separated the above two addresses, Gilman himself was invited to address the state society. He did so in April 1873 at the annual meeting in Sacramento, when he spoke "On Medical Education in the State University."[31] It is of interest here because it is in harmony with the sentiments of some of the California physicians with whom he was working and because it clearly contains the principles upon which the Johns Hopkins University opened its medical school fully twenty years later. In this early formulation of his thoughts about medical education, Gilman clearly faced two of the interesting themes that were to occupy him again in future decades: the medical school-university relationship and the proper preparation for the study of medicine.

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[28] *Trans. A M.A.*, 1872, *23*· 85-103.

[29] Edward R Taylor, "Some thoughts on medical education," *Trans. Med Soc. State of California*, 1871-72, Sacramento, 1872, pp. 74-100, p. 77.

[30] H. A. Dubois, "Medical education," *ibid*, 1874-75, pp. 103-05, p. 105. For information about Yale, which is where Gilman doubtless first encountered the various problems posed by the reformation and upgrading of medical education, see Whitfield J. Bell, "The medical institution of Yale College, 1810-1885," *Yale J Biol. and Med.*, 1960-61, *33*· 169–83.

In other parts of the country, Gilman noted, schools of medicine, law, and even theology were often regarded as departments quite separate from the universities with which they were nominally affiliated. Slight guidance and even less money from the Trustees was the usual case, but not so in California. The Organic Act expressly provided for the professional schools as integral parts of the University. Now that the Toland benefaction had been made, how to best conduct medical education had become a real and lively issue.

Beginning with the proper preparation, Gilman stressed the importance of scientific research. Purely literary study, he believed, created a reverence for authority. "On the other hand, a young man trained to habits of scientific research, studies the natural object, examines its parts, its organs, or its functions, and makes up his own mind as to the truth of nature without respect to what others have said."[32] Chemistry, zoology, comparative anatomy, these should all be thoroughly learned before the student takes up medical studies. Lectures were to be accompanied by long and patient studies in the laboratory.

Despite the emphasis on science, with which probably few physicians in his audience, or for that matter in other similar groups in the country, could personally identify, Gilman by no means wished to abandon literary studies. Classical studies he seemed to take for granted as part of the intellectual baggage of the educated man. But in the spirit of Charles Eliot's "New Education," Gilman stressed knowledge of the modern languages. He urged every candidate for the study of medicine to become familiar with French and German; at least so far as to read those languages with ease, and thus, through the French and German journals of science, keep abreast of the progress of discovery in Berlin, Vienna, and Paris.[33]

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[31] Daniel C Gilman, "On medical education in the State University," *Trans. Med Soc St. Calif.*, 1872-73, pp. 194-201.

[32] *Ibid.*, pp. 195-96.

It was this double course in science and language which the University of California now offered to the young men of the state who wished to prepare themselves for the pursuit of medicine. If these requirements bear a close resemblance to the so-called demands of the Women's Fund Group led by Mary Garrett in Baltimore in the early 1890s, they surely fit well with Gilman's own desires. His Yale and California experience, as well as a long report to the Hopkins Trustees in 1878, all stressed these same points. One is led to the conclusion then, that either the Baltimore women on their own formulated a remarkable set of demands to improve pre-medical education, or that they had been well coached by Gilman, Welch, and their colleagues.[34]

For the medical course itself, Gilman in 1873 had a set of prescriptions remarkably similar to what we usually credit as the Hopkins system. In the best schools of Europe, Gilman told his California audience, lectures, recitations, demonstrations, and practical work in the laboratory, take the place of the mere lecture system. The three to four months curriculum of the ordinary school was far too short. There should be a medical course running the entire year, one that was progressive from year to year. In an interesting observation that apparently was applicable in his day as it has been a century later, Gilman pointed out that, "the young men when they first come up to study, are not the best judges of what they should study first."[35]

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[33] *Ibid.*, p. 198. For a general survey of the changing scene in higher education, one that must be understood and incorporated into a good history of medical education, see Lawrence R. Veysey, *The Emergence of the American University* (Chicago. Univ. of Chicago Press, 1965); and two studies by Hugh Hawkins *Pioneer: A History of the Johns Hopkins University, 1874-1889* (Ithaca: Cornell Univ. Press, 1960), and *Between Harvard and America,* The Educational Leadership of Charles W Eliot (New York: Oxford Univ Press, 1972).

[34] Much has been written about the stringent entrance requirements set by the Johns Hopkins Medical School in 1893. Most authors who have discussed the history of the school or the biographers of her famous teachers have given credit for the extensive prerequisites to Mary Garrett and her women associates through whose generosity the medical school was finally launched. Gilman himself, in his farewell address as President on February 22, 1902, recalled that "in accordance with the plans of the University, the generous donor made it a condition of her gift that candidates for the degree of Doctor of Medicine should be those only who had taken a baccalaureate degree based upon a prolonged study of science and the modern languages." Perhaps owing to modesty, Gilman did not point out that these thoughts were vintage Gilman of the early 1870s and likely even before that. "Resignation A farewell address," *The Launching of a University, op cit* (n. 16 above), pp. 127-141, p. 138.

[35] Gilman, "On medical education . ," *op. cit* (n. 31 above), p. 199.

It was unfortunate, he thought, that we have adopted the word "medicine" in the title of our schools. "It is something more. It is a study of life itself, and perhaps it would have been better to call it a school of biology, in order to present to the mind of the student and the public the fact that we are studying life in its fullest aspects, and all its discoverable relations."[36] Here again one can point to what Gilman must have observed in California. In the *Biennial Report of the Regents of the University of California for the Years 1873-5* one finds a rather startling description of clinical instruction used by the Medical Department:

> A patient is placed in charge of a senior student, and by him examined, a diagnosis and prognosis given, together with his views of treatment, in the presence of the class; afterwhich, all errors of investigation, conclusion, or suggestion in treatment are corrected, with such remarks upon the subject as may be pertinent to practical medicine, etc.
>
> Every student, thus detailed, is expected to keep a complete history of the case, in due form, for his own and lecturer's use.[37]

Here obviously, in embryo form, is a method of clinical instruction institutionalized by Hopkins and other schools, many features of which are easily recognizable to present-day medical students. The extent of this practice or how many patients and what variety of disease conditions or emergencies each student had to face, I have as yet been unable to determine. One can probably not argue that Toland, Cole, and their colleagues should be viewed as having instituted in the West twenty years prior to what many view as the real Hopkins contribution. And yet the seeds were surely there, and if we look closely, they were probably also being planted elsewhere.[38] It was from this medical milieu, then, that Gilman journeyed east in 1875 to take up the great challenge in Baltimore.

All this is not to say that Gilman and the University of California brought about reform in medical education two decades before Hopkins. Far from it, the medical school with which Gilman was associated in his brief tenure as President of the University of California was not to approach the standards we associate with the 1890s until that time actually arrived. What is worth noting, though, is that Western physicians themselves had similar ideas and

[36] *Ibid.*

[37] P. 94.

[38] This has been pointed out by W. F. Norwood, *Medical Education in the United States Before the Civil War* (Philadelphia: Univ. of Pennsylvania Press, 1944), and John Duffy, ed., the *Rudolph Matas History of Medicine in Louisiana*, vol. 2 (Baton Rouge. Louisiana State Univ Press, 1962

that they received Gilman warmly into their midst. His ideas, while not fully to take hold in the 1870s, were in the air, so to say. Just how much he learned from medical men in California is difficult to say. Influence, as any graduate of a Temkin seminar knows, is not a word to be used loosely. One can only try to show the pattern and extent of Gilman's early involvement in medical education while he was in California. How much of his views and ideas he carried with him from New Haven to Berkeley and how much more he refined these while there is, of course, also difficult to assess. What does seem obvious is that much that historians have firmly associated with the Johns Hopkins Medical School was clearly evident in the writings of Gilman and some of his medical contemporaries in California in the early 1870s. While Billings, Welch, Osler, and others wrote about reforms and helped to put them into practice, Daniel Coit Gilman clearly qualifies as an early and important figure in the history of late nineteenth-century medical education.

RICHARD CLARKE CABOT (1868-1939) AND REFORMATION IN AMERICAN MEDICAL ETHICS

CHESTER R. BURNS

In 1892, the year in which Richard Cabot received his medical degree, American physicians were still debating, fervently, the merits of the code of ethics adopted by the American Medical Association 45 years earlier. After a decade of angry disagreement in New York, resulting in two medical societies for that state, the A.M.A. leaders urged settlement of the still raging conflicts. The liberal New Yorkers responded by repealing their code of ethics, originally prepared as an alternative to the A.M.A. code.[1] That it was possible for any group of American physicians to feel secure at that time without a written code of professional ethics heralded a new day for the American medical conscience. Cabot was to be a singular figure in that new day. His contributions to the reshaping of professional ethics in medicine were extensive. None were more basic than those pertaining to diagnosis, therapeutics, and professional relationships in hospitals.

Truth in Diagnosis. Arranging certain phenomena as characteristics of a specific disease is a pattern of human response which goes a long way back in medical history. Names could be assigned to these clusters of phenomena. Distinguishing diseases by such names assumed greater significance after Thomas Sydenham, a 17th-century London practitioner, declared that every disease has its own natural history, its own sequence of phenomena. Physicians of the 18th century, following Sydenham's cue, fashioned elaborate catalogues of names for many species of diseases.

This act of naming a disease appeared, for many, to be the principal part of diagnosis. Names were sufficient, as indicated by the account books of private practitioners. Each entry typically included only the patient's name, one or more disease labels, remedies used, and the fee. More detailed clinical histories were kept for unusual diseases or extraordinary responses to therapy. Many were published in the medical journals of the day. They attracted attention because of their marvelous or even mysterious

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[1] Donald E. Konold, *A History of American Medical Ethics 1847-1912* (Madison the State Historical Society of Wisconsin, 1962), p 40

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circumstances, not because they illustrated a generally accepted pattern of clinical diagnosis.[2] Moreover, precise diagnosis was not a prerequisite for treatment, or autopsy.

Morgagni's autopsies, justly famous for their introduction of anatomical thinking, were not done to prove or disprove a particular diagnosis or to evaluate the nature of treatment, although the Paduan professor sometimes described treatment in general terms. His clinical histories, many very brief, were surely abstracts of more complete stories. He selected certain symptoms or signs: vomiting, cough, suffocation, chest pain. He located the deranged organ which he believed to be connected with the symptom or sign. A fifty-year-old butcher experienced rightsided and substernal chest pain.[3] He had persistent spasms of dry coughing and could not breathe well except with his neck upright. He died on the seventh day. Morgagni examined the lung and discovered a very hard right upper lobe and a left lower lobe whose posterior portion was colored black. He made a few observations about the heart and pericardium. No more. He did not attempt to confirm a particular diagnosis nor demonstrate errors of clinical judgment. Data in these ''case histories'' were highly circumscribed, used selectively by the anatomist, Morgagni.

During the century after Morgagni's volumes were published, the art of making a clinical diagnosis was broadened and systematized.[4] By the middle of the 19th century, numerous physicians, like Austin Flint, considered diagnosis an ''essential prerequisite'' for treatment.[5] Unless the nature of the disease was known, appropriate principles of treatment could not be employed and conclusions about the effects of certain remedies on specific diseases would be erroneous. Skill in differentiating diseases required thorough attention to the evolving patterns of symptoms and signs. A physician could not interpret these clinical histories accurately unless he carefully recorded all phenomena associated with every case. Analysis of each case involved special attention to the relative frequency of symptoms and signs, the order of their appearance, and their duration and termination.

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[2] To sample a large group of these cases, see Paul F. Eve, *A Collection of Remarkable Cases in Surgery* (Philadelphia. Lippincott, 1857).

[3] John Baptist Morgagni, *The Seats and Causes of Diseases* . . . , Trans by Benjamin Alexander from the London, 1769 edition (New York Hafner, 1960), vol. I, p. 553.

[4] General information about the history of diagnosis may be found in Kenneth D. Keele, *The Evolution of Clinical Methods in Medicine* (London: Pitman, 1963), and Mahlon H Delp and Robert T. Manning (eds), *Major's Physical Diagnosis* (Philadelphia Saunders, 1968), pp 1-12.

[5] Austin Flint, *A Treatise on the Principles and Practice of Medicine.* Third edition (Philadelphia· Lea, 1868), p. 21

Flint, like others, believed that many diseases had not received adequate clinical studies of this kind.

Toward the end of the 19th century, William Osler's approach to specific diseases was very similar to Flint's.[6] Osler's conceptual categories for each disease were definition, etiology, morbid anatomy, symptoms, diagnosis, prognosis, and treatment, usually in that order. Osler's text, published in the year of Cabot's graduation from medical school, was an elaborate catalogue of specific diseases arranged for the benefit of those practitioners committed to the rightness of differential diagnosis and to this conceptual procedure as antecedent to methods of treatment.[7]

Although Flint and Osler both referred to "cases" of this disease or that disease, the clinical histories of given individuals were absent from their texts. The focus was on diseases, specific entities, with their own unique patterns of beginnings, stages, and endings.

Differential diagnosis required each clinician to perform essential steps, sequentially and thoroughly, and the information obtained by each step had to be recorded in an orderly fashion. Not a few clinicians of the 19th century, especially during the last quarter, delineated the features of this methodical approach to clinical evaluation. One British doctor called it "taking a case" and suggested an elaborate order that included general observations, anatomical or physical pecularities, intellectual and moral pecularities, previous history, previous general health, present illness, present condition, conditions of the various organ systems, causes of the disease, diagnosis, prognosis, and treatment.[8]

Physicians who knew how to "take a case" were the only ones who exhibited a truly scientific approach to patient care. Professional "goodness," with reference to science, had little to do with oaths, codes, and laws. It had much to do with the clinician's knowledge of diseases, his understanding of pathological anatomy and physiology, his skills as biographer and physical examiner for each patient, and his capacity to integrate all of this data into a pattern of proper clinical judgment of which there were three fundamental parts: diagnosis, prognosis, and treatment.[9]

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[6] Almost invariably, Flint considered each disease from the following viewpoints pathological anatomy, symptoms and signs, causation, diagnosis, prognosis, and treatment

[7] I have used the second edition of William Osler's *The Principles and Practice of Medicine* (New York· Appleton, 1896)

[8] Thomas Hawkes Tanner, *A Manual of Clinical Medicine and Physical Diagnosis*. Third American from second English edition, revised by Tilbury Fox (Philadelphia Lea, 1876), pp 27-30.

[9] By Cabot's time, the skillful use of laboratory tests had become an important part of clinical approaches In the early part of his career, Cabot devoted considerable attention to laboratory tests For some examples,

This commitment required far more rigorous thinking than that implied by 18th-century account books. More extensive records became essential for physicians who were expected to see a variety of sick strangers day after day in the growing number of hospitals. Case records could be used for historical, scientific, and legal purposes. Above all, they encouraged systematic, comprehensive, and precise thinking.

That which distinguished a professionally ethical clinician was not whether he recited an oath or abided by a particular clause in a code, but whether he was willing to approach the care of each individual patient with specific clinical methods designed to facilitate adequate diagnosis and appropriate treatment. It involved a willingness to engage in the systematic, comprehensive, and precise thinking signified by the case record.

Only three years after graduation, Richard Cabot began using case histories in his teaching.[10] These case histories were similar to those developed by Harvard's F. C. Shattuck for student examinations in clinical medicine. Cabot first employed them in private quiz exercises, and then adapted them to teaching sessions with junior medical students at Harvard.[11] In 1906, he published 78 of these cases.[12]

As an example, consider case 26.

> A boy, 14 years old, of gouty family history, complains for a year of frontal headache, not very severe, but persistent and wearing. Appetite excellent, but digestion not as good as it has been. Has grown suddenly very irritable, having been previously sweet-tempered. He has lost flesh during the year and seems listless and weak. Sleeps well. Bowels somewhat costive. Getting pale. Heart, lungs, and abdomen negative. Knee-jerks not easily obtained, but gait shows only weakness. Urine normal color, acid, 1028, no albumen. Sediment negative. Temperature 98°, pulse 96°. No oedema. blood negative.[13]

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see the following articles by Cabot: "Remarks on Widal's 'clump reaction' in typhoid fever," *Bost. Med. & Surg J*, 1897, *136* 107-108; "The use of methylene blue in the diagnosis of renal disease," *St Paul Med J.*, 1899, *1*.88-99, and "Justus's test for the diagnosis of syphilis," *Bost Med. & Surg. J*, 1899, *140*·323-324 Cabot did object to a rigid distinction between laboratory and clinical diagnosis See his essay, "The historical development and relative value of laboratory and clinical methods of diagnosis Physical diagnosis," *Bost Med & Surg J.*, 1907, *157*.150-153.

[10] Richard Cabot, "The clinical-pathological conference," Typescript sent on February 2, 1938, to Dr F A Washburn This is located in the Cabot papers at the Harvard University Archives in Cambridge, Massachusetts. Special thanks are due as follows· to the American Philosophical Society for a travel grant from the Penrose Fund which enabled me to visit these Archives; to the trustees of the Cabot papers for permission to examine and use the papers, and to the staff of the Archives for their generous assistance.

[11] For an example of a teaching session, see Richard Cabot, "A case-history clinic," *Detroit Med J*, 1905, *5*.6-13

[12] Richard Cabot, *Case Teaching in Medicine* (Boston: Heath, 1906)

[13] *Ibid*, p. 64

Cabot inquired about the causes of the lad's frontal headache, his irritability, his pallor. The scheme of questions varied from case to case, but always ended with the big three: Diagnosis? Prognosis? Treatment? With a focus on each individual patient, student physicians were expected to appreciate the importance of a thorough family history, an adequate review of signs and symptoms, a careful physical examination, and appropriate laboratory studies. Unlike other school books of the day, answers were not provided in the early editions of Cabot's collected cases.[14]

A subsequent teachers' edition did contain answers and discussion, as did the final edition of 1928, in which case 91 is the same as the one presented above. It is now listed as "diabetes mellitus" in the chapter dealing with "diseases of lymphatic and ductless glands." Cabot's comments about diagnosis are pointed.

> The careful student of this case will notice first of all the *loss of flesh despite good appetite*. Emaciation with persistent headache and diminished knee-jerks, are the obvious physical signs. Eyestrain, adenoids, malaria, masturbation were easily excluded by examination and watching. Further questions revealed the fact that micturition was frequent and copious. This, with the loss of flesh despite good appetite, suggested diabetes, and the urine was found on examination to contain sugar—a point omitted in the examination of the attending physician and therefore omitted in the above description of the case. As the glycosuria proved persistent, the diagnosis of diabetes mellitus was made.[15]

The practitioner could not make an adequate diagnosis without careful observation of signs, their use in excluding certain provisional diagnoses, further questioning, and additional examination of the urine. Cabot emphasized the error of omission—failure to examine the urine for sugar—because of its crucial importance in the final diagnosis.

Throughout these case histories, his editions of *Physical Diagnosis*,[16] and his frequently revised volumes of *Differential Diagnosis*,[17] Cabot was concerned with errors of omission and commission in clinical diagnosis. Whether or not the practitioner went to church on Sunday, knew the "Star Spangled Banner," swore the Hippocratic Oath, or adhered to precepts about consultation in the A M. A. code of ethics were not the important criteria for judging professional propriety. What counted was whether a practitioner understood specific diseases—their causes, signs, symptoms, courses, prognoses, treatments—and whether each practitioner applied this understanding in the assessment and management of each individual patient. These were the characteristics of a physician practicing truth in diagnosis, rightness in clinical care. This unswerving devotion to truth led Cabot to create a new method for instilling his professional ideals, the "clinical-pathological conference."

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[14] The first group of 43 cases had been published in 1902, titled *Exercises in Differential Diagnosis*

[15] Richard Cabot, *Case Histories in Medicine* Third edition (Boston: Leonard, 1928), p. 246

[16] Beginning in 1901, twelve editions of *Physical Diagnosis* were published before Cabot's death

[17] There are several editions; I consulted the third published in 1915

In 1908, Cabot discovered that a clinical diagnosis of neurasthenia had not been changed in the record of a patient who had actually died of cancer of the pleura.[18] Although the pathologist had documented the final "truth" about the patient, the clinical record had never been changed. Cabot wondered about the extent of these circumstances.

He analyzed 1,000 consecutive autopsies at the Massachusetts General Hospital and compared the pathological findings with the clinical judgments rendered in each case.[19] An error of omission meant that the clinician had failed to find a lesion which contributed to the patient's death. An error of comission meant that the clinician had diagnosed a lesion not actually found at autopsy. For example, he discovered that mitral stenosis had been correctly diagnosed in 69 percent of the cases, that it had been omitted from diagnoses in 22 percent, and that it had been incorrectly diagnosed in 9 percent. Bronchopneumonia had been correctly identified in 33 percent of the cases and had been assumed present in 3 percent of the cases when the lesion was not there. In 60 of 94 patients (64%) the diagnosis of bronchopneumonia had not been made, when it was actually present. Practitioners had missed the diagnosis of peptic ulcer in 56% of the patients evaluated, and this rather high rate of error was true for other diseases. Cabot published these findings in 1910 and its implications for American clinicians were as shocking and poignant as were the revelations of Abraham Flexner about America's medical schools, revelations made in the same year.[20]

Two years later, at the 63rd annual session of the American Medical Association, Cabot reported on a study of 3,000 autopsies.[21] The clinico-pathological data from these studies verified his belief that many clinical diagnoses were wrong. The public response was not surprising. An article in the *Chicago Tribune* for December 28, 1912, was titled "Wrong Diagnosis Common, asserts Dr. Richard Cabot." The professional response was not surprising either. A Chicago practitioner assailed Cabot in a letter to the *Journal of the American Medical Association* published on January 11, 1913. Accusing Cabot of "diagnostic pessimism," this practitioner believed that conscientious physicians should not reveal and criticize the improper diagnoses of other doctors. Cabot's reply was terse:

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[18] Typescript cited in note 10 above.

[19] Richard Cabot, "A study of mistaken diagnoses," *J A.M A* , 1910, *55* 1343-1350

[20] Earlier in that same year, Cabot had begun weekly CPC's at the Massachusetts General Hospital Typescript (n. 10 above).

[21] Richard Cabot, "Diagnostic pitfalls identified during a study of three thousand autopsies," *J A M A* , 1912, *59*.2295-2298. For Cabot's views on the limitations of clinical accuracy, see his essay, "The ideal of accuracy in clinical work· its importance, its limitations," *Bost Med. & Surg. J.*, 1904. *151*:557-560

> I hoped to read such objections as Dr. Croftan's. They helped to bring out the truth. Before I knew the facts, I used to feel just as he does. When he has had 3,000 clinical diagnoses criticized at autopsy by an independent and unprejudiced pathologist who makes full bacteriologic and histologic examinations of every case, he will find, I believe, that the facts are not less unpleasant than I have stated them to be.[22]

Cabot's scrupulous honesty was so frightening that the Massachusetts Medical Society, in 1916, considered expelling him for "publicly advertising the faults of the general practitioner."[23] By that time, the recitation of codified precepts of professional ethics could no longer substitute for proper diagnostic skills and the willingness to have those skills tested by the facts of an autopsy. Even ultraconservative Southerners acknowledged that incomplete physical examinations and inadequate diagnoses were frequent moral errors in the practice of medicine.[24]

Another facet of Cabot's case histories merits attention. They were, from the beginning, histories of sick individuals. Each was a suffering person, having lived a specified number of years, the heir of a particular family history, characterized by a specific occupation or life predicament, and often bearing a unique cultural legacy. All of this was communicated by Cabot in the first sentence or two of each case: "A boy, fourteen years old, of gouty family history. . . . A shoemaker of twenty-four. . . . A businessman, fifty-eight, with good family history and habits. . . . Mrs. A., a Jewess of thirty-six. . . . A Portuguese lumberman of forty. . . . An Italian housewife of thirty-five. . . . A colored man of sixty-four. . . . A boarding-school boy of sixteen. . . . An expressman, thirty years old of good family history and good habits. . . ."[25] These notations for individual personhood were absent in the textbook approaches of Flint and Osler.

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[22] *J.A.M.A.*, 1913, *60* 145

[23] "Richard Clarke Cabot," in *Dictionary of American Biography* edited by Robert Schuyler (New York: Scribner's 1958, Supplement two), p 85. For some additional details about Cabot's life, see Thomas Franklin Williams, "Cabot, Peabody, and the care of the patient," *Bull Hist Med.*, 1950, *24*.462-474 This is reprinted in *Legacies in Ethics and Medicine* edited by Chester R Burns (New York Science History Publications, 1977), pp 307-326.

[24] Chester R. Burns, "The quest for professional ethics in Texas medicine, 1900-1920," *Bulletin of the Friends of the P. I. Nixon Medical Historical Library*, October, 1975, p 5

Although Austin Flint understood the need for systematic studies of case histories as the basis for more accurate differential diagnosis, his notion of a clinical history was still that of the spontaneously occurring events associated with a particular disease (the natural history) and, moreover, that sequence of events undisturbed by any clinical intervention.[26] After defining diabetes mellitus, for example, Flint examined the disease from the viewpoint of pathological changes, evolution of signs and symptoms, etiology, diagnosis, prognosis, and treatment. Osler used the same aforementioned categories of definition, etiology, morbid anatomy, symptoms, diagnosis, prognosis, and treatment in approaching diabetes mellitus. Both Flint and Osler wished to differentiate diabetes mellitus from other diseases.

Cabot wished to do the same. But he also wanted students and fellow physicians to always regard each patient as person. The boy of fourteen and the Russian housemaid of twenty-four were sick with diabetes mellitus.[27] Diagnosis, prognosis, and treatment involved not only the disease, but the person afflicted with the disease. There was usually a sense of temporal change in each person's case history. Even in those volumes containing the most elaborate case histories, the editions of *Differential Diagnosis,* Cabot invited students and practitioners to use the disease categories as constructs for understanding and evaluating the changes perceived by patients themselves: pain, headache, weakness, cough, vomiting.

Cabot's case histories represented a fusion of the art of caring for sick persons and the science of analyzing symptoms and signs. It was a method for integrating the knowledge of particular diseases with the needs of individual persons, a way of keeping the "objective" science of medicine focused on the "subjective" desires of a suffering patient.

Cabot's case histories were antidotes to the elusive generalizations of organizational codes of ethics, such as the Principles of Ethics adopted by the American Medical Association in 1903 and revised numerous times since then. As counterpoint to the medical nosologies of the 18th and 19th centuries, Cabot's case histories also added the all important flesh and blood, albeit suffering, to the often vacuous labels of classificatory schemes. Cabot's case histories represented the realities of clinical situations, even more so than the categorical abstractions of Austin Flint and William Osler. Cabot's brand of medical casuistry had not appeared before in the history of clinical medicine.[28]

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[25] *Case Histories, op. cit* (n. 15 above), pp 139, 165, 207, *Differential Diagnosis* (n 14 above), pp 302, 303, 353, 415, 535

[26] Flint, *op cit* (n 5 above), p 113

[27] The Russian housemaid's plight is described as case 378 in *Differential Diagnosis* (n. 16 above), pp 731-733.

Therapeutics and Patient Care. Throughout his diligent efforts to improve clinical diagnosis, Richard Cabot never forgot that the treatment of a sick person—therapeutics—was the central concern in medical ethics, the preeminent obligation of physicians. Yet, he practiced during a period of extremely profound transition.

The empirical and dogmatic traditions of medical therapeutics—the bleeding, cupping, blistering, and the "opathies"—had been repudiated, but not much remained that was effective. Cabot knew that the proponents of Claude Bernard's experimental medicine were laying the foundation for a pharmacological therapeutics that would replace the archaic materia medica. Yet, he could not wait for the days of miracle cures; his patients expected treatment and his approach to that treatment was strikingly comprehensive.

Even though many drugs had been discredited, Cabot was not a nihilist. He was suspicious of most claims for specific drugs, believing, at one time, that only malaria, diphtheria, myxedema, and diseases caused by intestinal parasites were cured by specific drugs. During the early days of their use, Cabot encouraged the evaluation of hormonal preparations (thyroid, adrenal, and pituitary) in certain clinical situations.[29] He critically evaluated 102 drugs used in 50,000 prescriptions given by the internists, including himself, at the Massachusetts General Hospital over a four year period.[30] He was ecstatic about the discovery of salvarsan, believing it to be just the beginning of a new era in drug therapy.[31]

Nevertheless, he believed that doctors prescribed too many useless drugs, especially placebos and analgesics. He viewed placebos as lies, but he used them.[32] He regarded analgesics as important labor saving devices for some physicians who were unwilling to do the hard work of differential diagnosis.[33] He was convinced that physicians prescribed drugs indiscriminately and that they were responsible for teaching the American public that drugs would solve all of their problems.[34] He argued repeatedly against a philosophy of clinical care that made therapeutics synonymous with the selection of drugs.

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[28] Awaiting the efforts of an eager graduate student is a comparison of casuistry in theology, law, and medicine In developing the C P C , Cabot acknowledged the influence of case teaching at Harvard's Law School For a description of the latter, see Arthur E Sutherland, *The Law at Harvard A History of Ideas and Men, 1817-1967* (Cambridge: Harvard University Press, 1967), pp. 174-180.

[29] Richard Cabot, "The clinical uses of the preparations from the thyroid gland, pituitary body, suprarenal capsules, and bone marrow " *Med News*, 1896, *69*·281-289

[30] Cabot, *Case Histories, op cit.* (n. 15 above), pp. 278-282.

[31] *Ibid* , pp 274-275

[32] Richard Cabot, "The use of truth and falsehood in medicine an experimental study," *Amer Med* , 1903, *5*:344-349.

Cabot acknowledged the importance of treatment for symptoms and treatment for diseases whose causes were known.[35] Yet he thought that both approaches were too narrow. His therapeutics would be rooted in "pathological physiology," a full understanding of nature's responses to disease conditions. Such treatment would be effective, if and only if a physician comprehended the physical and psychological pecularities of each patient and that individual's relationships to family, work, education, place of residence, and other aspects of the environment. Treatment involved the entire person, not just the liver or kidneys, not simply hookworms or headaches.

Cabot advocated the use of all forms of physical therapeutics including fresh air, water, food, massage, heat, and cold. He also devoted considerable attention to various forms of psychotherapy, or as he called it, "psychic treatment" or "scientific mind cure." He was extremely pleased when a study of therapeutics at the Massachusetts General Hospital between 1898 and 1909 demonstrated a marked decline in the use of drugs, especially alcohol, and a significant increase in the use of baths, massages, mechanical treatments and various forms of psychotherapy.[36]

Physicians alone could not be responsible for all aspects of Cabot's comprehensive therapeutics. Underlying his commitment to paraprofessionals was a belief that women, not men, had the necessary aptitude for attending to the details of therapeutics.[37] Because he believed proper nursing to be an essential ingredient of effective hospital cure, Cabot devoted a great deal of attention to the new profession of nursing.

He suggested improvements for the training school for nurses at the Massachusetts General Hospital. He gave a number of graduation orations for nursing schools, and he published several articles in nursing journals.[38] He thought that too many nursing schools were designed to conscript low-paid labor for hospital services. He believed that nurses should pay for their education and be taught by salaried instructors; that nursing should be taught by nurses and medicine by physicians; and that the education of nurses should include such liberal studies as sociology, history, and literature. He believed that nurses would perform tasks relinquished by physicians and that nurses had to become involved in original scientific investigation. He was pleased to see the specialized ventures of these nurses who were developing the allied professions of physical therapy and occupational therapy.[39] Some nurses were also becoming hospital social workers, another consequence of Cabot's vision of medical therapeutics.

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[33] Richard Cabot, "Pain allaying drugs as used and abused," *Good Housekeeping Mag.*, 1911, *53*.57-60.

[34] Richard Cabot, "The physician's responsibility for the nostrum evil," *J.A.M.A* , 1906, *47* 982-984

[35] Richard Cabot, "Therapeutics based on pathological physiology," *Bost Med & Surg J.*, 1906, *155*·57-59

[36] Richard Cabot, "The consumption of alcohol and of other medicines at the Massachusetts General Hospital," *Bost. Med. & Surg J.* 1909, *160* 480-481.

[37] Richard Cabot, "The renaissance of therapeutics," *J.A.M.A* , 1906, *46*:1665.

Cabot believed that the care of the body could not be divorced from the patient's social circumstances. He became disillusioned when he realized that the life situation for many of his patients precluded a proper response to his therapeutic recommendations; when he knew that absolute rest would cure symptoms and yet a vacation or absolute rest meant starvation; when he could write a prescription for a tonic that would improve a patient's appetite, yet the patient had no money to buy food.[40]

In October of 1905, Cabot hired an individual to handle the social problems of his patients at the Massachusetts General Hospital. Ida Cannon, sister of Harvard's outstanding physiologist, Walter Cannon, joined Cabot in the following year. Together they created the first significant social service department in any American hospital. They regarded social work as an essential part of medical care. They believed that a social service department should be a fundamental unit of every hospital and that social workers should be reimbursed by the hospital and responsible to its administrators.

Cabot told many stories about improvements in patient care made possible by social workers. One involved the O'Flaherty's—father, mother, and nine children. At varying times over a period of eleven months, different members of this family appeared in the outpatient department of the Massachusetts General Hospital for the treatment of scabies. Said Cabot,

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[38] Some of Cabot's essays about nursing are "Suggestions for the improvement of training schools for nurses," *Bost. Med. & Surg. J.* 1901, *145* 567-569; "The importance of original work by nurses," *The Trained Nurse and Hosp. Rev* , 1902, *28* 339-340; "A statistical study of the educational opportunities offered in the Massachusetts Training School for Nurses," *Amer J Nursing*, 1906, *6*.438-448

[39] See Cabot's essay entitled "The importance of occupations for the sick," *Bull Mass Assn for Occupational Therapy*, Sept., 1927, vol 1, no. 6.

[40] Richard Cabot, "Social service in hospitals," *J. Maine Med. Assn* , 1919, *10* 91-107. Also see these essays by Cabot: "Some functions of social work in hospitals," *The Modern Hosp.*, 1915, *4*·188-191; "Social work in hospitals," *Nosokomeion*, 1930, *1* 36-42; "Report on the principles of hospital social service," *Nosokomeion*, 1933, *4*.388-396.

> We sent a social worker out in this case and she rounded up the whole family on a Sunday, and, with the assistance of some aunts of the family, treated the whole eleven simultaneously. As they were treated simultaneously, they did not reinfect one another, and, as they did not reinfect one another, they stayed cured, and we watched that family a year to see that they stayed cured, and they did.[41]

Not every sick patient required this kind of care.

Consider a patient with diabetes mellitus. When Austin Flint discussed the treatment of diabetes mellitus, he noted that general and local bloodletting, mercurial preparations, emetics, and active purgatives were harmful. He thought that alkalies, opium, and strychnine might be useful and mentioned other remedies. Dietary regulation was extremely important and he recommended long sea voyages, exercise in the open air, and occasions for mental relaxation and recreation.[42] William Osler discussed exercise and massage in conjunction with the personal hygiene of diabetic patients. He discussed diet extensively and reviewed numerous drugs, concluding that none had curative powers.[43] Cabot discussed dieting in great detail and then recommended exercise in moderation, sleeping in the open air, and careful attention to cleanliness together with the "use of such a regime as will keep up the patient's courage."[44] Not only did he think drugs useless in this disease, but he acknowledged the importance of measures designed to "keep up the patient's courage."

By the early decades of the 20th century, the physician's burdens of diagnosis and treatment had become very great. Differential diagnosis required extensive knowledge of the causes and forms of disease and the willingness to engage in rigorous logic and careful observations. Selection of appropriate drugs and other therapeutic measures was difficult at best. All of this required enormous time and energy. How could a physician also be responsible for "the patient's courage"? Only by sharing that responsibility with others.

With the site of medical care shifting from the patient's home or the doctor's office to the modern hospital, responsibility for the patient's care and "courage" began to be distributed among representatives of allied professions. The physician was still the leader of the group, but there was a group.[45] This group of physicians and para-professionals could become responsible for the comprehensive approach to therapeutics envisioned by Richard Cabot. For Cabot, any approach to clinical care that was less than comprehensive, or that failed to consider "the patient's courage" was morally wrong.

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[41] Cabot, "Social service in hospitals," *op cit.*, p. 99.

[42] Flint, *op cit* (n 5 above), pp 792-795

[43] Osler, *op cit* (n. 7 above), pp 327-330.

[44] Cabot, *Case Histories, op. cit.* (n 15 above), p 250

Hospital Ethics. If one-to-one physician-patient relationships and one-to-one physician-physician relationships were ever the exclusive relationships in professional ethics throughout the history of medicine (and I doubt it), this was certainly no longer true for the modern hospital of Cabot's day. The hospital physician was caught in an intricate web of interpersonal relationships, and the incumbent duties and rights were complex. Gone was the simple obligation, enshrined in the A.M.A. code of 1847, that physicians care for the hospitalized sick poor as part of their duties to communities. By the early decades of the 20th century, much more was at stake.

There was an incredible increase in the number of hospitals in the United States, long before the antibiotic era. The American public believed that the new breed of scientific physician had something to offer and they wanted it. Much value was attributed to differential diagnosis and comprehensive care. The public understood that the doctor's black bag could not hold all of the equipment necessary for adequate diagnosis, nor could solo practitioners purchase or house all of the needed machines. They realized that equipment needed operators; technology required technicians. The public also supported the emergence of a host of professionals involved in therapeutics: physical, psychological, and social. With this public approval of the new medicine, physicians began to reassess their professional obligations.

Doctors could not rest secure after experiencing the rigors of differential diagnosis or the perturbations of selecting effective remedies. That was not enough. Physicians also had to consider their obligations to medical science, their relationships to other physicians including students, their duties to the newly emerging allied professionals, and their responsibilities to the general public. Cabot not only recognized this new predicament for physicians, but he also analyzed the attendant obligations in an explicit fashion.[46] Following are summaries of some of his "ought" claims.

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[45] Cabot called these groups "Therapeutic teams " Folder 14, Box 74, Cabot papers, Harvard Archives

[46] Believe it or not, this analysis is found in one rather short essay by Cabot entitled "Medical ethics in the hospital," *Nosokomeion*, 1931, *2*.151-161 Also see Cabot's essay, "The hospital as a social institution," *Nosokomeion*, 1932, *3*.218-222.

Since so many professionals, paraprofessionals, and technicians were involved in hospital care, nothing less than extensive cooperation would be needed for gathering and sorting all of the information needed for adequate diagnosis and effective therapeutics. An adequate system of record keeping was needed.

Physicians' diagnoses should always appear in writing and, in fatal cases, these diagnoses should always be written in the record before an autopsy is begun. Practitioners should attend the autopsies of their patients and all fatal cases should be reviewed at weekly staff meetings.

The number of patients assigned to a hospital physician must be limited so that adequate attention can be given to each individual. Inappropriate reexaminations, unnecessary procedures and tests, and failure to seek appropriate consultations are errors to be avoided. Physicians should be courteous and considerate in their relationships to all patients.

Doctors should explain diagnosis, prognosis and treatment to each patient, but they should never allow the patient to hear interprofessional conversation about pathological findings, or possible complications and dangers. This did not mean that patients should be treated as objects. Cabot believed that too many hospital practitioners treated their patients like "children or feeble-minded persons." Since the patient is more interested in his disease than anyone else, enough time must be taken to explain the nature of the disease and its treatment so that the patient can assist intelligently in his own care. If they have been properly trained, assistants to the physicians can provide this kind of instruction. The modesty of women patients must always be protected.

Furthermore, patients must not be exploited for teaching purposes. The patient's consent must always be obtained and the activities of the student should not lead to additional suffering or fatigue. The physician was responsible for supervising all operations and verifying all examinations done by students and house staff.[47]

The hospital was also the setting in which various physician specialists were engaged in patient care, and interns and residents were climbing the ladders of professional respectability. Hospital physicians should not abuse junior colleagues by expecting them to do the work of the senior physician, nor should the senior doctors take credit for the scientific contributions of their younger colleagues. Conflicts about surgical versus medical care should be resolved by committees which included both internists and surgeons. In contrast to past traditions of private practice, Cabot believed that age or seniority should never be the basis for promotion or authority in a hospital setting.

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[47] Cabot was remiss in not discussing obligations toward "students" in a more explicit fashion His discussion of duties in relation to medical students and house staff is based primarily on the need to protect the patient and the need to protect an individual practitioner from abuse by another

As mentioned earlier, Cabot did not believe that adequate patient care could occur without consideration of the patient's social circumstances. Since the physician himself was unable to investigate or handle all those circumstances, he must have the assistance of appropriately trained social workers. Physicians must learn how to share responsibilities for patient care with social workers, clergymen, nurses, occupational assistants, and other hospital employees. Physicians should do everything they can to foster effective contributions by these allied professionals and they should treat them with respect and consideration.

Hospital physicians have duties to the general public. They should educate citizens about all matters of public health and preventive medicine. They should make certain that the public understands the nature of the responsibilities of hospital physicians, including what not to expect from hospital care. The hospital should publish annual reports and its physicians should, in those reports, admit failures and mistakes. The managers of hospitals should not divert public monies into teaching or scientific research unless the public desired this.[48]

Hospital physicians do have obligations to the world of science, though. Every hospital physician should either conduct scientific research himself or should afford an opportunity for other physicians to do so. In all circumstances involving experimental procedures, the consent of patients should be required.

Cabot emphasized that every hospital physician had three main obligations: those in relation to the care of patients, those involving teaching, and those pertaining to scientific research. Each physician was expected to attend, in some fashion, to all three of these areas and he was expected to maintain a proper balance among them, that is, not neglecting any one of them by placing too much emphasis on one of the others.

Until today, no other physician in the history of American medicine has examined the contours of ethical obligations in a hospital setting as succinctly and comprehensively as did Richard Cabot.[49]

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[48] Cabot perceived hospital administrators as representatives of the public, but he did not discuss explicitly the duties of hospital physicians toward these administrators.

[49] I invite readers to challenge this interpretation. Although I continue to be amazed, I have not yet discovered any analysis of hospital ethics comparable to the one given by Cabot in the *Nosokomeion* article of 1931.

By the fourth decade of the 20th century, the American medical conscience had been reshaped. Oaths and codes were still being written, but their meaning was more symbolic than real. The all-purpose general practitioner of the 19th century—a Christian gentleman of intrinsic goodness, law-abiding, and adhesively loyal to the codified rules of one professional society—had been displaced by a new group of specialist practitioners. With varying religious and cultural commitments, these specialists developed a professional righteousness rooted in experimental science and expressed in elaborate methods of clinical evaluation and patterns of comprehensive care. These ideals were forged in the modern hospital by physicians, like Cabot, who believed as absolutely in the triad of patient care, research, and teaching as had the 19th-century general practitioners believed in the Bible, the American flag, and the A.M.A. Code of Ethics. As much as any other physician of his day, Richard Cabot demonstrated the validity of the new bases for professional goodness and exemplified the reformed conscience of American medicine.[50]

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[50] I have discussed only a few aspects of Cabot's involvement in the story of American medical ethics Much remains to be said

NUTRITION, QUANTIFICATION AND CIRCULATION

JEROME J. BYLEBYL

In his recent book on Galenism, Owsei Temkin has pointed to a fundamental difference between Galen and Harvey with regard to the role of specialized scientific investigation in the formation of physiological theory.[1] In developing his concepts of internal function Galen did take account of extensive anatomical and vivisectional data, but his major aim was to elucidate the underlying bases of dietetic medicine, which "gave its attention to man's food and drink and the air surrounding him as necessities for the maintenance of physical and mental life, as causes of disease, and as factors in preserving and restoring health."[2] Accordingly, his physiological theories are for the most part limited to providing the internal counterparts of such manifest activities as nutrition, respiration, and locomotion. By contrast, Harvey's intensive investigations within the bodies of live animals and dead men resulted in the discovery of a phenomenon that was quite novel in more than a strictly factual sense: first, the circulation could not have been predicted from, nor was it reducible to, the externally apparent activities of whole organisms; and second, the idea was antithetical to, rather than supportive of, the prevailing medical theories and practices. Thus with Harvey's work the study of internal bodily function began to acquire a certain measure of autonomy, instead of being simply "the natural part of medicine" (though it has of course continued to serve the latter function as well).

In these pages I shall examine certain aspects of this transition having to do with nutrition and quantitative reasoning, themes which Temkin has also discussed in two articles published in 1960 and 1961.[3] In the first of these articles, he quotes the following summary of the nutritive process from Galen:

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[1] *Galenism, Rise and Decline of a Medical Philosophy* (Ithaca and London, Cornell University Press, 1973), pp 152-159.

[2] *Ibid*, p. 154.

[3] "Nutrition from classical antiquity to the baroque," in *Human Nutrition, Historic and Scientific*, ed. Iago Galdston (New York, International Universities Press, 1960), pp. 78-97, and "A Galenic model for quantitative reasoning?" *Bull. Hist. Med.*, 1961, *35* 470-475.

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> The quantity of food must be proportionate to the quantity of transpiration [i.e., invisible loss]. If nothing in us flowed away, we would not have come to need food, since the original [mass] of body substance would have been preserved for ever. However, the animal bodies are dissipated by the transpiration that takes place through the pores invisible to the sense. Therefore we need food. For nutrition is the restitution of what has been emptied.[4]

On these simple assumptions rested a good deal of Galen's medical system, as indeed of ancient medicine in general. Thus disruptions at various stages of this process of loss and replacement were assumed to be among the major causes of disease, while much of therapy was aimed at compensating for the effects of improper nutrition. And most important for our purposes, one of the main goals of Galenic physiology was to provide a detailed account of what happens to the food in the interval between ingestion and dissipation.

Of central importance in Galen's physiology of nutrition was the assumption that the blood is essentially an intermediary substance in the whole process, i.e., the form taken by the food following digestion and preceding final assimilation.[5] Galen's predecessors had differed widely over the specific anatomical pathway taken by the food in reaching the body at large, as well as over the nature of the qualitative changes involved and of the forces causing these changes and the accompanying movements. But from at least the time of Plato onward they were nearly unanimous in their assumption that blood is food-becoming-flesh, and that *the movement of the blood is little more than the internal counterpart of the overall nutritive turnover.*[6] The ancients did, of course, ascribe other functions, and consequently other movements, to the blood,[7] but such alternative conceptions were quite subordinate to the basic view that food is transformed into blood in some central organ, and that the blood then moves slowly outward to be assimilated and ultimately lost through invisible emanation.

Galen gave his most comprehensive account of this process in his influential treatise *On the natural faculties*. Regarding the various controverted issues mentioned above, he maintained (1) that food passes from the gut, through the portal veins, to the liver, where it is transformed into blood, and that from the liver the blood is dispensed to all the parts of the body through the systemic veins; (2) that the changes from food to blood, and from blood to tissue, are real, substantial transformations, based upon alterations in the blend of the four primary qualities; and (3) that the main factors responsible for moving the food/blood along this pathway are active powers of attraction, retention, and expulsion resident in all parts of the body. Thus for Galen there was really no theory of the movement of the blood as such, but only a theory of nutrition, in which every part of the body is assumed to go about the business of attracting, retaining, and assimilating food, and expelling its superfluities.[8] Moreover, Galen explicitly stipulated that under normal circumstances every part will exercise its powers to move the blood only to the degree necessary to satisfy its existing needs, so that there is a direct teleological link between the rate of blood flow and the rate of nutritive turnover.[9]

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[4] "Nutrition," p. 86, citing Galen, *Opera omnia*, ed Kühn, vol 15, p. 240

[5] See, e.g , Galen, *On the Natural Faculties*, I, 10, tr Brock (Loeb Library), p. 35: "How, then, could blood ever turn into bone, without having first become, as far as possible, thickened and white? And how could bread turn into blood without having gradually parted with its whiteness and gradually acquired redness?"

[6] Plato, *Timaeus*, 80-81, Aristotle, *Parts of Animals*, 650a, 33-650b, 14

[7] E.g , Plato, *Timaeus*, 70 A and B; Aristotle, *History of Animals*, III, 19; 521a, 6-21, Galen, *On the Natural Faculties*, III, 14

This assumed link between the movement of the blood and the nutritive process was one of the greatest obstacles to any conception of the circulation, in which the blood must endlessly retrace the same pathway rather than being simply distributed on a one-time basis. This point is clearly illustrated by the case of Cesalpino, who came closer than anyone else to anticipating Harvey's discovery.[10] In contrast to most contemporary physicians who accepted the Galenic view that nutritive blood is formed in the liver and distributed through the veins, Cesalpino was a staunch defender of Aristotle's view that the heart is actually the body's main source of nutriment.[11] Accordingly, he shifted emphasis from the venous to the arterial blood as the principal nutriment of the body: the veins derive their contents from the gut and liver, but

> In animals we see that aliment is brought through the veins to the heart as to the workshop of the innate heat, and having received its final perfection there, it is distributed to the entire body through the arteries.[12]

As evidence for the heartward flow of the venous contents Cesalpino pointed to the distal swelling of ligated veins, and he even maintained that under certain circumstances the same blood that had flowed out from the heart through the arteries would pass over to the veins at the periphery and so return to the heart.[13] Yet, with his gaze fixed primarily on nutrition, he failed to generalize the latter notion or to see its fundamental importance—there was simply no reason why aliment which had once received its "final perfection" should repeatedly return to the heart on a constant basis.

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[8] See esp *Nat Facs*. III. 13; tr. Brock, pp. 295, 313

[9] *Ibid*., II, 3 and 6, pp. 131-133, 161-163.

[10] Walter Pagel, *William Harvey's Biological Ideas* (New York, Hafner, 1967), pp 169-209

[11] See esp his *Peripateticorum quaestionum libri quinque* (Venice, 1571), Lib. V, Q III, pp. 102r-107v

[12] *De plantis libri XVI* (Florence, 1583), p 3.

Harvey too was long influenced by the idea that the movement of the blood is essentially a matter of the one-time distribution of food. In his anatomical lecture notes of 1616, written before the discovery of the circulation, he sketched the overall pathway of nutriment through the body: "through the liver passes all incoming aliment, through the lungs pass incessantly all the aliment and the whole mass of the blood."[14] Most of the anatomical details of the pathway have been left out, but from other passages it is clear that they would have been the same as those envisioned by Cesalpino: from gut, through portal veins, to liver; from liver to systemic veins; from systemic veins, through heart and lungs, to arteries.[15] In contrast to Cesalpino, however, Harvey ascribed to the lungs the primary role in concocting venous into arterial blood, while the heart had emerged chiefly as a motor organ, i.e., as the efficient cause of the "incessant" passage of blood through the lungs, to the arteries.[16] Indeed, by far the most original feature of the lecture notes is precisely the lengthy analysis of the events comprising the cardiac cycle, based on Harvey's own unprecedentedly detailed studies *in vivo*.[17]

The movement of the heart was also one of the two major topics in Harvey's *De motu cordis et sanguinis* (1628), and I have tried to show in a recent article that the relevant portions of the treatise were probably written before the discovery of the circulation, for what was to have been a separate monograph on the heartbeat alone.[18] Most of these early chapters were devoted to a more definitive presentation of the conclusions that had been set forth in the lectures of 1616, but in one of them (chapter seventeen) Harvey gave a lengthy teleological analysis of his anatomical and vivisectional findings. Here he developed the idea that the heart is essentially the "impulsor of aliment," his reasoning being that in all but the smallest and simplest animals, "some driving force is needed to distribute the aliment, either because of the organic variety of the parts or because of the density of the body's substance."[19] Thus the heart basically exists to propel an adequate supply of nutriment to all the parts of the body, but beyond this the hearts of different kinds of animals are nicely suited to their particular needs, so that the larger and denser is the body of a species, the larger and more powerful is the heart. Thus for Harvey, the heart as a specialized propulsive organ had largely supplanted Galen's diffuse faculties of attraction as the underlying cause of the distribution of blood, and beyond this the heart had also taken on the more pointedly teleological role of ensuring that supply is carefully matched to need.

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[13] Cesalpino, *Quaestionum medicarum Libri II* (Venice, 1593), p 234.

[14] *The Anatomical Lectures of William Harvey*, ed G. Whitteridge (Edinburgh and London, E & S. Livingstone, 1964), p. 296.

[15] *Ibid.*, pp. 80, 90, 124, 140, 252, 270, 284

[16] *Ibid* , pp. 248-250, 270, 284-296

[17] *Ibid.*, pp. 264-272

[18] J J. Bylebyl, "The growth of Harvey's *De motu cordis*," *Bull. Hist. Med.*, 1973, *47* 427-470

Nevertheless, having actually studied the heartbeat in painstaking detail, and then having clearly separated the efficient cause of blood distribution from the individual parts that would consume the blood, Harvey was in a position to eventually challenge his own assumption that the heartbeat serves only to provide the body with a proper supply of food. As he tells us in chapter eight of *De motu cordis,* the breakthrough stemmed primarily from an intense consideration of the actual abundance of blood transmitted by the heart, as a result of which he came to see that

> the juice of the ingested aliment could not supply [this amount], but that we would have the veins emptied, completely drained, and the arteries, on the other hand, disrupted by the excessive intrusion of blood, unless the blood somehow permeates from the arteries back into the veins, and so returns to the right ventricle of the heart.[20]

Moreover, quantitative reasoning not only provided Harvey's initial insight into the necessity of the circulation, it also served as the main pillar of his mature defense of the idea. Thus, first place was accorded to his famous method of hypothetical calculation: suppose, for example, that the heart transmits only a single dram of blood at each beat, and further that it makes only a thousand beats in half an hour, still the total amount transmitted in this period would exceed the sum total of blood contained in the whole body, so that the same blood must continually return for retransmission.[21] But besides showing the inexorable necessity of the circulation, the quantitative argument served equally to refute the age-old assumption that blood flow is rigidly tied to nutrition, a point that is epitomized by Harvey's repeated assertion that the amount of blood transmitted by the heart "cannot be supplied by the freshly ingested food, and is far greater than would be suitable for the nutrition of the parts."[22]

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[19] Harvey, *Exercitatio anatomica de motu cordis et sanguinis in animalibus* (Frankfurt, 1628), pp 64-65

[20] *Ibid* , p. 41.

[21] *Ibid.*, pp. 43-44.

Thus told in summary fashion the story seems to be that of an ancient theory based on common sense yielding to the modern scientific methods of specialized observation and quantitative reasoning, though there is of course much more to it than that. For one thing, the very process of studying the heartbeat *in vivo* extended back to antiquity, and Harvey's early investigations were a direct outgrowth of this long-standing problematic and methodological tradition.[23] Moreover, long before Harvey, individuals concerned with nutrition and related issues had gone beyond the general supposition of a balance between the body's intake and loss to develop more pointed forms of quantitative physiological reasoning, some of which closely parallel those employed by Harvey in the discovery and proof of the circulation. Indeed, as Temkin has pointed out in the second of the articles referred to above, Galen's own treatise *On the natural faculties* contains a prime example of such reasoning.

In this passage, Galen was attempting to refute the view that the kidneys produce urine simply as a residue of their own nutrition, rather than as a result of a special power to attract the watery superfluities of the entire body.[24] Basic to his argument was the fact that "the amount of urine passed every day shows clearly that it is the whole of the fluid drunk which becomes urine, except for that which comes away with the dejections or passes off as sweat or insensible perspiration."[25] Depending upon how much he has drunk, someone might well pass four congii (about fourteen quarts) or more of urine in a short time, but on the assumption that urine is a residue of nutrition, all other parts should produce similarly large amounts of residual fluid, propertional to their size. The lungs, for example, being many times larger than the kidneys should produce many times more watery fluid, and the total residue for all organs would fill a large barrel. However, this would lead to two impossibilities: since the lungs, unlike the kidneys, have no way of eliminating this fluid, it would simply collect in the thorax and lead to suffocation; and since the kidneys alone rapidly eliminate an amount of fluid nearly equal to that consumed, there obviously is no surplus to supply the much larger amounts that ought to be eliminated by the other parts.

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[22] *Ibid* , p 54, see also pp. 43, 48, 58.

[23] Bylebyl, "Growth of *De motu cordis*," pp. 434-438 See also Temkin, *Galenism*, pp 153-154.

[24] Temkin, "A Galenic model," referring to Galen, *On the Natural Faculties*, I, 17.

[25] *Ibid* , tr Brock, p. 109

Thus we see Galen attempting to quantify an internal physiological process—the production of urine by the kidneys—but with reference to two externally manifest activities of the whole organism—drinking and urination—for which the internal process is assumed (or, in this case, experimentally known) to provide an intermediary link. The two external processes are not identical with the overall nutritive turnover, though they are closely related to it—drinking is, of course, part of the intake of food, and Galen is careful to discount for invisible emanation as an alternative way of eliminating fluid. Moreover, the overall intake and output of fluid not only serves as a measure of the activity of the kidneys, it also provides a criterion for testing the alternative view, which is rejected in part because it would entail impossibly large aggregates of material.

Similar characteristics are revealed by a more ambitious effort at quantification undertaken in the sixteenth century by Leonardo Botallo, who set out to determine how much blood is generated in the human body every day.[26] The inquiry was prompted by practical medical considerations, since Botallo advocated the far more liberal use of bloodletting than was customarily done, and had thereby aroused sharp criticism. In 1577 he published a defense of his methods which was based on the idea that the human body is rather like a syphon, continually losing substance through invisible emanation, and just as steadily taking up fresh material from the veins.[27] Botallo's aim was to provide a rough quantitative estimate of the internal process underlying this one, namely the liver's daily output of fresh blood. He emphasized that no single figure could be given, since the rate will vary enormously from one individual to another, and within the same individual from one day to another, depending upon his activities, the amount of food consumed, and his state of health. But, he proposed, in a healthy, well-nourished body of moderate size, the amount of fresh blood generated per day would be something on the order of eight to ten ounces, an estimate which he increased to one pound in a second edition of his treatise published in 1580.[28]

To justify so large an estimate, Botallo drew heavily on medical experience.[29] Thus he cited individuals who were accustomed to being bled from eight to twelve ounces twice per month over the course of many years, without suffering an apparent deficiency of blood. Yet, since the body contains only about twelve to fifteen pounds of blood, they should be bled dry within one year if blood is replaced only very slowly. Again, as a military surgeon he had often seen patients who had lost blood almost to the point of death, but whose veins seemed to be adequately, if not completely, replenished with good blood within three to four days. Furthermore, Botallo knew of many patients who were afflicted by chronic daily blood loss through haemorrhoids or other forms of bleeding, losses which would add up to fourteen or more pounds per month. "Whence does so large an amount come from?" Botallo asked.[30] Clearly, he asserted, the body must be capable of generating at least eight to ten ounces of blood per day in order to sustain such chronic losses. Another index of the quantity of blood production in the healthy body was provided by the process of lactation, since the mammary glands were thought to produce milk by the further elaboration of blood. Botallo asked,

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[26] Botallo, *De curatione per sanguinis missionem* (Lyons, 1577), pp. 159-169 The second edition (1580) contained an augmented discussion, which I have read in Botallo, *Opera omnia* (Leyden, 1660), pp 271-305.

[27] *De curatione*, pp. 9-12, esp p. 11.

[28] *Ibid.*, p. 163; *Opera*, p 296

> But what infant is so small that it does not, in one day, suck [at least] half a pound of milk? But isn't this milk made from blood? Besides, isn't the mother herself nourished by her blood? Who therefore is so ignorant that he does not learn from this that in one nursing woman two or even more pounds of blood can be generated daily, and from this surmise how much can be generated in a healthy body, and how much in an infirm one?[31]

However, Botallo recognized that the nature of his evidence posed certain problems, for granted that a body which gave up considerable amounts of blood or milk might generate sufficient fresh blood to make up the loss, it might seem incredible that the same could happen in a body not subject to such unusual loss. As he put it, "But some will cry out, 'Whew! How great an amount [will be generated] in a year! Where will there be a body, or rather an urn, capable of holding [all this blood]?' "[32] To meet this potential objection, Botallo looked to the process of insensible emanation. He had previously established that this is the ultimate controlling factor in the rate of nutrition, and hence of blood production, and now by insisting on the steadiness and copiousness of such loss, he sought to show that the body might easily dispose of fairly large amounts of blood in the course of normal, daily

[29] *De curatione*, pp. 159-164

[30] *Ibid*, p 162

[31] *Ibid*, pp 168-169. Cf Harvey, *De motu cordis*, ch 10, p. 47

[32] *Opera*, p 296. Cf. also *De curatione*, p. 163

nutrition.[33] He did not actually try to measure the daily loss by weighing the body itself, but he clearly saw the possibility of such a procedure. Thus to form some estimate of how much material might be lost, he said, one should carry out an experiment, either in thought or in deed: take a lump of moist clay, weigh it, put it aside, and weigh it again the next day to see how much moisture it has lost. Then, given the great size of the human body, and the fact that it is subject to internal as well as external heat, it would not be at all unreasonable to assume that it can dispose of eight to ten ounces of blood per day even under healthy conditions.[34]

Botallo's emphasis on insensible transpiration, as well as his suggestion of a possible weighing experiment, reminds us of the work of the Venetian physician Santorio Santorio, who began his life-long effort at actually measuring the insensible loss of the human body just a few years after Botallo's discussion appeared.[35] Like Botallo, Santorio took for granted the Galenic physiology of nutrition as the underlying basis for what he was attempting to measure, and he too assumed that in keeping careful track of the body's weight, one is indirectly measuring the increase or diminution of blood, the intermediary form of the nutriment.[36] However, in contrast to Botallo, it was transpiration *per se* that was of primary interest to Santorio. He was convinced that it is the most critical determinant of health and disease, and that it is subject to direct dietetic regulation as an alternative to such traditional remedies as bloodletting and purging. Meticulous attention to bodily weight enables one to keep track of this hitherto elusive factor of invisible loss, as well as to ascertain what things promote or inhibit its proper occurrence.[37]

Santorio's *Medical Statics*, published in 1614, was based on more than thirty years of such routine weighing procedures. It is decidedly a practical medical work rather than a physiological treatise—in fact, it is not really a treatise at all, but a collection of more than five hundred short aphorisms, on the Hippocratic model. These are divided into seven sections, one introductory and the others corresponding to the non-naturals, the main determinants of health and disease in traditional dietetic medicine. The following examples from section one will illustrate the general range of Santorio's interest:

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[33] *Ibid* , pp 164-167

[34] *Ibid.*, p 165 Temkin, "Nutrition," p 87, calls attention to the famous weighing experiment of Erasistratus, although this does not appear to have been known during the sixteenth century

[35] See M. D Grmek, "Santorio Santorio," in *Dictionary of Scientific Biography*, vol XII (New York, Scribner's, 1975), pp 101-104, esp. p 101.

[36] Santorio, *Commentaria in primam fen primi libri canonis Avicennae* (Venice, 1626), cols 568-577, 556-557. See also *De statica medicina aphorismorum* (London, 1701), Section 3, Aphs. 17, 20, 84, 92, 103

[37] Temkin, "Nutrition," pp 88-89; Grmek, "Santorio," p 102

No. 4. Insensible transpiration alone is usually more plentiful than all the sensible [forms of evacuation] put together.
No. 6 If the food and drink in one day are eight pounds in weight, insensible transpiration usually rises to above five pounds.
No. 13. If someone sensibly evacuates more than is proper, he will perspire less than is proper.
No. 59. In the space of one night there are usually evacuated sixteen ounces of urine, more or less, four of concocted excrement through the rectum, and forty or more through hidden transpiration.

Thus Santorio was primarily concerned with the overall intake and output of the body in a given period of time, rather than with the internal physiological processes that were supposed to link the two.

Nevertheless, Santorio's studies did yield an important physiological conclusion, namely that the body's normal rate of turnover of substance is appreciably greater than was previously thought—the five pounds mentioned in aphorism six is several times greater than Botallo's estimate that the body disposes of a pound or two of blood daily, even though Botallo himself had regarded the latter estimate as daringly large. Furthermore, the work of the two men taken together represents an effort to quantify the very parameters with which Harvey would compare the amount of blood transmitted by the heart, namely food consumed, blood produced, and food/blood absorbed. For Botallo and Santorio themselves it would not have been relevant to make these comparisons since they assumed that the nutritive blood is simply dispersed through the veins, but once Harvey had come to regard the heartbeat as crucial for the propulsion of nutriment, the comparison became quite reasonable, if not entirely obvious.

Moreover, it was probably no coincidence that even before Harvey, a colleague of Santorio's in the Venetian College of Physicians went on to make fairly regular use of quantitative argumentation in a primarily physiological, rather than practical medical, context. This was Emelio Parigiano, who in 1621 published *Twelve books of noble exercises on subtlety*, in which he defended a variety of avowedly heterodox—and generally speculative—physiological theories.[38] Chief among these was a pangenetic theory of generation, according to which sexual reproduction involves a temporary diversion of the nutritive process, to generate a new organism rather than simply to regenerate the old. At any given moment, every least part of the body will have adhering to it a minute amount of material that has been fully assimilated to the point of being alive, but is just short of the final stage of irreversible incorporation. During orgasm, every part is forced to give up its minute portion of living and all-but-incorporated aliment, all of which come together in the genital organs to form a minute, living replica of the parental body.[39] Each parent emits such a small (subvisible) spermatic animal into the uterus, where they undergo a perfect, part-for-part fusion to form the new offspring.[40]

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[38] *Nobilium exercitationum libri duodecim de subtilitate* was published at Venice in 1621, but it appears that most of the printing was reissued with a new title page in 1623, together with an appendix, *Par et sanius judicium*, in which Parigiano replied to the criticism of Mundinius Mundinius. Cf. H B. Adelmann, *Marcello Malpighi and the Evolution of Embryology*, 5 vols. (Ithaca, New York, Cornell University Press, 1966), V, 2311 I have used a copy having the 1623 title page.

One potential difficulty in this theory was that it might not be possible to have a complete representation of every part of the parental body within an extremely small aggregate of material. In responding to such an objection, Parigiano offered the following hypothetical reckoning:

> Let us suppose that the whole body consumes an ounce of blood every single hour. Suppose further that the aliment is divided equally for nourishing each part, so that each of the parts has its fair share. And let the weight of the spermatic parts be one hundred or two hundred pounds. Thus the ounce of blood will be divided into one hundred or two hundred portions. Then let each pound of the body be further divided into ounces, obles, and grains, if you please; and let the grain be the least part that has been conceived mentally. How much blood [of the one ounce] do you think each of these parts will have coming to it? Indeed, if the reckoning be done properly it will not be even half of a grain of sand, nor perhaps even half of that.[41]

From these and other considerations Parigiano concluded that it would indeed be possible to withdraw a few minutes' worth of ultimate nutriment from every part of the body, and still have an aggregate that was quite small.

In Parigiano's view, one of the main strengths of his theory was its ability to account for the temporary fatigue of the whole body following orgasm, as well as for the more permanent emaciation that might result from the too-frequent use of sex.[42] By contrast, supporters of the orthodox Galenic view that semen is simply concocted from blood by the genital organs were hard pressed to explain why orgasm should have these short and long-term effects on the whole body. To do so, they advanced the idea that it takes a large amount of blood to generate a small amount of semen, citing the suggestion of Avicenna that the ratio may be as high as forty to one.[43] On this "moorish fable" Parigiano unleashed his rhetorical scorn.[44] Suppose, he said, that someone emitted half a pound of semen during the space of one night, such as might occur if he were to have intercourse ten or more times. Then, on the Avicennan ratio, the generation of this amount of semen would require twenty or more pounds of blood, "so that all the blood in the body would rush to the genital organs," resulting not only in emaciation, but possibly even in death. Further, if it takes forty ounces of blood to generate one of semen, then what, Parigiano demanded, becomes of the other thirty-nine ounces? Where do they go? How do they disappear? Do the testes perhaps consume them, so that they are, as it were, gluttons of blood? He continued:

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[39] *De subtilitate*, pp 64-120

[40] *Ibid.*, p. 81

[41] *Ibid.*, pp 126-7

[42] *Ibid*, pp 40-42, 72

> For I would certainly believe that [the testes] can generate at least two ounces of semen in the space of one night, so that during the night they would consume at least eighty ounces of blood. Why! for generating eighty ounces of blood during the evening, which would in turn serve for generating two ounces of semen to be emitted at night, as well as for nourishing the rest of the body—not even a whole calf would be sufficient. And for generating a pound of semen for one who has intercourse ten times—not even a cow would suffice.[45]

Parigiano used similar demonstrations of impossibly large aggregates to refute a number of other theories,[46] but in one remarkable instance he turned the method to the defense of another of his own idiosyncratic views, namely that there is "an instantaneous flux and reflux [of blood] through the arteries, from the heart to the whole body and back again."[47] To some extent, even the orthodox Galenists would have accepted the notion of flux and reflux as characteristic of the movement of the blood within the arteries, as distinct from that within the veins, but Parigiano went so far as to maintain that the aortic valve is incompetent, so that every time the heart contracts it expels blood into the arteries, and when it dilates it receives the same blood back into the left ventricle. He offered a variety of arguments to support this view, among them the following:

> But if the valves in the aorta were an impediment to all return, then, since the heart in systole expels the greater part of its blood into the aorta, and that in scarcely a single moment of time, the aorta would be so continually filled [with blood] that it could

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[43] Parigiano, *Par et sanius judicium*, pp. 573-4, with reference to the views of Mundinus

[44] *Ibid.*, pp. 582-583

[45] *Ibid.*, p. 583

[46] E.g., *De subtilitate*, p. 125, *Par et sanius judicium*, pp. 610-611

[47] *De subtilitate*, p. 558. The theory is expounded in detail in Book six, pp. 267-303

> receive no more, while the heart as well as the superior arteries would be emptied within a few beats, though experience shows that the upper arteries as well as the lower ones are filled with blood. Thus the arterial blood must necessarily undergo a flux and reflux from the aorta.[48]

It is not clear what Parigiano intended by his references to the upper and lower arteries, but the general thrust of his argument is clear enough: if the transmission of blood from heart to arteries were irreversible, it would soon lead to an excess of blood in the latter and a defect in the former.

For Parigiano, as well as for the majority of medical physiologists whose views fell broadly within the Galenic camp, it was appropriate to think of the arterial blood as undergoing a ceaseless flux and reflux since they conceived of it primarily as a vivifying agent, as a vehicle of heat and spirits, in contrast to the venous blood, which serves as nutriment. As we have seen, however, contemporary Aristotelians did not agree with this exclusion of the arterial blood from nutrition, and one of them, namely Cesare Cremonini, developed a quantitative argument to support this very position. In his lectures on the primacy of the heart, delivered at the University of Padua and subsequently published in 1627, Cremonini argued:

> Galen wants the venous blood to be, of its own substance, suitable for nutrition, so that from it all the members are nourished. I wish that Galen would tell me what becomes of the arterial blood? For it is continually generated, and once generated is diffused in great quantity to the entire body through the arteries. What then becomes of it, if it is always generated but is not consumed as nutriment? It would grow to infinity, because he says that an immense amount is always generated, but none of it is consumed.[49]

Thus like Parigiano (whose ideas were probably known to him)[50] Cremonini points to the threat of an impossibly large aggregation of blood in the arteries, though his way of alleviating the problem was simply by having the blood consumed as a major source of nutriment for the body.

In the same year that Cremonini's lectures appeared in print, his former student and friend Caspar Hofmann pointed out that the rate of cardiac transmission is also of crucial importance for maintaining a proper balance within the arterial system. Hofmann was an avowed disciple of Cesalpinian physiological ideas, and accordingly a firm advocate of the idea that "by far the largest part" of the blood produced by the liver is taken up by the heart and transmitted to the arteries, to nourish the body.[51] Like his two mentors he saw the primary function of the heart as the concoction of venous into arterial blood, but in contrast to them he also had a clear comprehension of the heart's role in transmitting blood to the arteries through its forceful systole.[52] Indeed, he maintained that the arterial pulse is the simple mechanical consequence of the heart's impulsion of blood.[53]

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[48] *Ibid*, p. 297.

[49] Cremonini, *Apologia dictorum Aristotelis de origine, et principatu membrorum adversus Galenum* (Venice, 1627), p. 338.

[50] Both were part of the same Venetian-Paduan intellectual community. Parigiano had criticized Cremonini by name in *De subtilitate*, p. 506.

It was in defending the latter idea that Hofmann touched on the issue of balance between the input and output of the arterial system. He was responding to someone who might object to his mechanical pulse theory on the standard grounds that it would take some period of time for blood to flow from the heart to the most extreme arteries, so that there ought to be some clearly perceptible lag in the transmission of the pulse. To this Hofmann replied:

> You are deceived, my good man, and the deception results from this, that you think that the arteries are emptied at each beat, which is not even imaginable. For the blood stagnates within the arteries, and is concussed by the effervescent innate heat, so to speak, just as is water in the seas. And just as waves in the sea successively strike the shore, so does the blood in the artery. But only as much [blood as leaves the artery], partly by flowing into the accompanying vein, . . . partly by being absorbed by the parts: so much is then replaced by the heart.[54]

Thus Hofmann makes a sharp distinction between the rate at which blood enters and leaves the arterial system, which is by implication very slow, and the nearly instantaneous transmission of a wave-like impulse through the "stagnant" arterial blood. With regard to the former issue, he simply takes it for granted that the transmission of fresh blood by the heart must match the rate at which nutriment is absorbed by the body. Remarkably, he also points to a flow of blood from arteries into veins as an additional factor in maintaining this balance, although in his view this blood would also be consumed by the body, after being mixed with the somewhat cruder venous blood.[55]

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[51] Hofmann, *Commentarii in Galeni de usu partium corporis humani* (Frankfurt, 1625), p. 95, *De thorace ejusque partibus* (Frankfurt, 1627), p 56 On Hofmann and Cesalpino, see Pagel, *Harvey's Biological Ideas*, pp 198-199, On his relations with Cremonini, see Hofmann, *De generatione hominis libri quatuor* (Frankfurt, 1629), dedication.

[52] On sanguification, see *Commentarii*, pp. 94-96, 121, *De thorace*, p 96. On systole, *Commentarii*, p 129. " hoc discere est ex anatomiis vivorum animalium. In his enim apparet, contracto corde, & expellente sanguinem, dilatari arterias;" *De thorace*, p. 40. "Vulgaris est sententia, quamque confirmat structura valvularum in Aortae initio, contracto corde ebullientem sanguinem expelli in Aortam "

[53] Hofmann, *Commentarii*, p. 129, *De thorace*, pp. 39-44

[54] *Ibid.*, p 41

[55] *Commentarii*, pp 95, 109, *De thorace*, p 83

Thus among Harvey's predecessors and contemporaries we find not only the general modes of argumentation, but also most of the specific issues that figured in his quantitative argument for the circulation. Moreover, I believe that fuller consideration would suggest that actual lines of influence probably underlay these similarities. But even if this were not the case, it would at least be fair to say that in thinking about the quantitative aspects of blood-flow, Harvey, like his predecessors, was to some extent applying the mental habits of the dietetic physician. A clear indication of this connection is provided in chapter nine of *De motu cordis,* where, after presenting his hypothetical calculations in support of the circulation, Harvey expressed the hope that he might someday replace these arbitrary figures with real ones. He then went on to suggest that this more ambitious program of quantification would be as much of medical as of physiological significance:

> Meanwhile this much I know, and I wish all to be advised of it, that sometimes the blood passes over in greater quantity, sometimes in less, and sometimes the circuit of the blood is accomplished more quickly, and sometimes more slowly, according to temperament, age, external and internal causes, and the things natural, as well as the non-naturals such as sleep, quiet, food, exercise, passions of the soul, and the like.[56]

This may be compared with Santorio's seventh aphorism, which states the whole foundation of his own program of quantification: "The quantity of insensible perspiration undergoes some variation in accordance with differences of nature, region, season, age, diseases, foods, and other of the things non-natural."[57]

And yet beyond these striking parallels there remain some profound differences. Most obvious, perhaps, is the fact that what Santorio sought to quantify was an overall bodily process whose occurrence had been recognized for thousands of years, whereas what Harvey hoped to measure were the variations in an internal function which he himself had only recently discovered. More telling, however, is the fact that Parigiano and Hofmann, even though they had considered the specific issue of the quantity of blood transmitted by the heart, not only did not think of the circulation on their own, but emerged as major foes of Harvey's theory quite soon after 1628. In fact, each clearly saw a logical connection between Harvey's quantitative arguments and his own earlier statements, and responded accordingly. Parigiano's reaction was to strip his own theory of all trace of a massive flux and reflux between heart and periphery, and to insist instead that such exchanges occur only on an infinitesimal, "drop by drop" basis.[58] Indeed, in the remaining stock of his book of 1621 he actually seems to have had appropriate manuscript alterations made in the very passage concerning the heartbeat that was quoted above.[59] Hofmann, on the other hand, had anticipated the minimalist response to Harvey's quantitative argument, and therefore needed only to paraphrase his own earlier statement about the wave-like nature of the arterial pulse, with the quantitative assumptions made more explicit:

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[56] Harvey, *De motu cordis,* pp 44-45.

[57] *De statica medicina,* p 7

> For if this were so, the arteries would have to be emptied at each pulsation. *But where would so great an abundance [of blood] go to? And where would the blood be supplied from?* [In fact], no more blood is removed . . . than is partly used up for nourishing the members, and partly taken over into the contiguous veins through the anastomoses. *And both of these amount to very little indeed.*[60]

To try to fully account for the differences between Harvey and these two close contemporaries—neither of whom was an orthodox Galenist—would lie far beyond the scope of this article, but surely one central factor was the changing role of internal physiological evidence, referred to in the introduction. In the case of Parigiano, the difference is quite manifest: his original theory of flux and reflux had been almost wholly speculative in origin, and therefore could be arbitrarily altered by a literal flick of the pen to suit later polemical circumstances. In Hofmann's case, however, the contrast cannot be so sharply drawn, since he had shown scrupulous regard for anatomical evidence in developing his own physiological theories, and had even relied on some vivisectional observations in formulating his essentially sound interpretation of the heartbeat and arterial pulse.[61] Nevertheless, for all his attention to such evidence he never lost sight of his primary goal of explaining the manifest functions of the whole organism, such as nutrition and respiration.[62] Thus if there appeared to be a potential discrepancy between the activity of the heart in propelling blood and that of the whole body in consuming the blood, there was no question but that the heart had to conform to the limits set by the body— an assumption that is implicit in his statement of 1627, and on which he vehemently insisted in his later response to Harvey.[63]

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[58] Parigiano, *Nobilium exercitationum de subtilitate pars altera de cordis et sanguinis motu* (Venice, 1635), pp. 417-429, 443-445, 452, 458-459, 519. The word "guttatim" is used repeatedly in these passages

[59] See note 48 above One such copy is in the collection of the Institute of the History of Medicine, Welch Medical Library, The Johns Hopkins University I shall publish a separate note on this and other such copies in the near future

[60] Hofmann, *Apologia pro Galeno* (Lyons, 1668), p 103 (my italics) This discussion is followed immediately (pp 104-107) by the "Digressio in circulationem sanguinis," in which Hofmann refers back to this passage. For further details about the *Apologia* and the "Digressio," see Pagel, *Harvey's Biological Ideas*, p 197, note 101

[61] See notes 52 and 53, above.

[62] See e g , *Commentarii*, pp 94-96

For Harvey, by contrast, the study of the heartbeat had quite early become almost an end in itself, to the point that he seems to have planned a separate monograph on this subject alone. He may long have interpreted his findings in ways that were consistent with traditional physiological assumptions, but when he later began to think more critically about the problem of quantity, he did so with reference to an internal function that had become about as familiar to him as such overall processes as nutrition, respiration, excretion, and weight loss had been to his predecessors. Consequently, he apparently had little hesitation about postulating a whole new systemic function—the circulation—whose original purpose was simply to provide for the quantitative requirements of the heartbeat, though of course it soon took on much broader implications. Harvey's confidence in extrapolating from the heartbeat to the whole body is perhaps most clearly reflected in the following item from a list of his anatomical and physiological observations:

> The panting of the heart is but the pumping about of the blood, in the expansion receiving, and in the contraction sending it out; and it receives so much at every expansion, that considering the great proportion, and the many beatings of the heart in half an hour, it must of necessity come round about.[64]

He made much the same point in his reply to Hofmann in 1636: if only the latter would observe the beating heart in a live animal, and in particular take note of its external change in size when moving from diastole to systole, then the necessity for the circulation would become almost as obvious to him as, say, the fact that the Thames flows down to the sea.[65] However, he had clearly overestimated the degree to which Hofmann was open to persuasion by such evidence—one has the impression that the two men were arguing across a methodological gap almost as great as the chronological gap separating both from Galen.

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[63] Hofmann, *Apologia*, pp 104-107

[64] This list is preserved in the archives of the Royal Society, and is reprinted in Geoffrey Keynes, *The Life of William Harvey* (Oxford, 1966), pp 445-447; #2, p 445

[65] E V Ferrario, F N L Poynter and K J Franklin, "William Harvey's Debate with Caspar Hofmann on the circulation of the blood New documentary evidence," *J Hist. Med.*, 1960, *15*: 15

AFRICAN FEVER, PROPHYLACTIC QUININE, AND STATISTICAL ANALYSIS: FACTORS IN THE EUROPEAN PENETRATION OF A HOSTILE WEST AFRICAN ENVIRONMENT*

DENNIS G. CARLSON

Endemic febrile disease caused mainly by malaria has had profound influence on the course of West African history.[1] Over the past two millenia the emergence of the sickle cell trait in populations pursuing rain forest agriculture provided significant biological advantage in resistance to the intracellular malaria parasite, gradually leading to expansion of these genetic groups.[2] The African slave trade was greatly increased because of the

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[1] It seems likely that the majority of fever cases in West Africa were due to vivax and falciparum malaria Yellow fever was present endemically in some areas and erupted in epidemics periodically Most contemporary writers, as well as medical historians, have concluded that death due to yellow fever was a distant second to malaria in its many forms. August Hirsch, *Handbook of Geographical and Historical Pathology*, translated from the second German edition by Charles Greighton (London: The New Sydenham Society, 1883), vol. 1, pp 229-315. Although yellow fever is essentially controlled today, almost all Nigerians suffer from varying degrees of malarial infection during infancy and childhood.

[2] Wiesenfeld has presented strong evidence, based on mathematical models, that the advantage of protection of hemoglobin S against malaria was associated with a form of agriculture originally introduced into East African from Malaysia about the beginning of the Christian era. This complex system included new methods and new plants such as yams, taro, banana and coconut, and it produced a physical change in the tropical forest which became more conducive to the multiplication of *Anopheles gambiae* mosquitoes than the *Anopheles funestus* variety. Furthermore, *A. gambiae* is able to lead to a more intensive endemic malaria than existed before, which increased the danger to people without the hemoglobin S trait In a hyperendemic malaria environment, the fertility rate of people with the sickle cell trait is much higher than that of those with normal hemoglobin. Consequently people with hemoglobin S contribute proportionately more children to the general population.

Apparently this biological advantage led to the gradual expansion of tribal groups with a high prevalence of hemoglobin S within the tropical rain forests at the expense of hunting and gathering peoples who were not as genetically advantaged. Equilibrium could be achieved in the biological factors within 1500 to 2000 years, according to mathematical calculations The distribution of sickle cell trait in tropical Africa is remarkably similar to the areas of the tropical rain forest. Stephen L. Wiesenfeld, "Sickle-cell trait in human biological and cultural evolution· Development of agriculture causing increased malaria is bound to gene-pool changes causing malaria reduction," *Science*, 1967, *157*. 1134-1140.

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economic advantage to slave importers: slaves born and raised in Africa had higher immunity to malaria and yellow fever than did indentured European servants.[3]

The third and most obvious effect of African Fever was that Europeans who tried to establish colonies in West Africa from the fifteenth to the middle of the nineteenth century suffered consistent failure and enormous loss of life. The Portuguese, as early as the fifteenth century, were frustrated in their attempts to extend the plantation model from the Mediterranean basin to West Africa because their European managerial personnel could not survive the endemic illness.[4] The Dutch, English, French and Danish who also sought a foothold on African soil were able to maintain only minimal trading and military bases on the coastline.[5]

Let us examine the efforts of the English people from the middle of the eighteenth century to the middle of the nineteenth. The movement to abolish slavery encouraged business interests to seek ways to compensate for loss of Caribbean trade. "Legitimate" trade in West Africa was one significant possibility.[6] Exploring parties such as those led by Park, Denham, Clapperton, Audrey, Caille and the Landers in the late eighteenth and early nineteenth centuries were consistently crippled and usually defeated by the fever;[7] the mortality rate often exceeded eighty percent. Colonies such as the one planted at Freetown in 1787 and again in 1792 were quickly decimated by febrile and diarrheal diseases.[8] Mission societies frequently lost more than half their personnel during the first years after their arrival.[9]

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[3] In tropical America plantation workers recruited from indigenous peoples or from among white Europeans succumbed to disease much more rapidly than did black Africans. Phillip D. Curtin, "Epidemiology and the slave trade," *Political Sci. Quart* , June 1968, *83*(2): 190-216

[4] *Ibid.*, pp. 191-193

[5] Beginning at least as early as 1434 A.D., Portuguese began to explore the northwest coasts of Africa. By 1460 the area now called Sierra Leone was visited, and in 1484 the mouth of the Congo River was reached. The Dutch began to explore the African coast in the sixteenth century, and the English, French, and Danish in the seventeenth century. J D. Fage, *An Atlas of African History* (London. Edward Arnold, 1958), pp 26, 27, 194

[6] Ernest Marshall Howse, *Saints in Politics The "Clapham Sect" and the Growth of Freedom* (Toronto. University of Toronto Press, 1952), pp. 3-27, 29-64. Philip D Curtin, *The Image of Africa: British Ideas and Action 1780-1850* (Madison. University of Wisconsin Press, 1964), pp. 59-61, 95-97.

[7] Mungo Park, Surgeon, *Travels in the Interior Districts of Africa, performed under the direction and patronage of the African Association in the years 1795, 1796, 1797* (London: W. Bulmer, 1799). Mungo Park, *The Journal of a Mission to the Interior of Africa in the year 1805* (London. John Murray, 1815), pp xciv-xcv. Robin Hallet (ed.), *The Niger Journal of Richard and John Lander* (New York, Washington. Frederick Praeger, 1965), pp. 281-283. *J. D. Fage, An Introduction to the History of West Africa* (Cambridge: Cambridge University Press, 1966), pp. 113-117

[8] Curtin, *op. cit.* (n. 6 above), pp. 105-112

[9] *Ibid.*, pp. 434-486

Major British expeditions to West Africa in 1832-34 and in 1841-42 were destroyed by African Fever.[10] The 1841 expedition represented a major government investment, with 145 Europeans manning three new iron-bottomed ships, with the latest ventilating systems installed specifically to prevent miasmatically caused illness. Every accepted medicine, treatment, and precaution was used to protect the health of the crews. Nevertheless, the fever began to strike the seamen within three weeks of crossing the river bar at the Niger delta. Less than four months later all three ships had been forced to return to the offshore island, Fernando Po, with almost all the crew members either sick or dead.[11] The government in London decided to call the expedition home to England. Despite the best science and technology which the British had been able to muster, febrile illness again prevented them from achieving their goal.

Why were the British ineffective in preventing and treating malaria at that time? They had cinchona bark available. Thomas Sydenham had used it for prophylactic and curative purposes in the seventeenth century; James Lind had prescribed it for sailors in the eighteenth century.[12] Were the British theories of disease causation, pathology, or therapeutics faulty? Were the clinical regimens used for febrile illness inadequate? Even though the malaria organism was not microscopically identified until 1880 and the mode of transmission was not demonstrated until 1898, how did it happen that the problem of preventing malaria was essentially solved by 1850?

The questions relating to fever were not new for British medical and political groups. At home in England, because of the large number of fever cases, special hospitals were built,[13] and medical theorists spent much time trying to explain fever phenomena on the basis of "predisposing" causes such as night dew, bright sunlight, exertion, intemperance, and "excitatory" causes such as "miasms." The miasms were thought to be produced mainly from decaying material in moist environments and were in some ways unique to the particular geographic location concerned. Thus it was possible, they theorized, to have "African," "Niger," or "Bullam" fevers in African areas of the Empire, or "Bengal" or "Guzzerat" fever in India.[14] Southwood Smith tried to resolve the dilemma of unique manifestations versus general disease processes by writing,

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[10] Macgregor Laird and R. A. K. Oldfield, *Narrative of an Expedition, into the Interior of Africa, by the River Niger, in the steam vessels Quorra and Alburkah in 1832, 1833, 1834* (London. Richard Bentley, 1837, 2 vols.), vol. 1, pp. 32-33.

[11] James Ormiston M'William, M.D., Surgeon of *H.M S Albert* and senior medical officer of the expedition, *Medical History of the Expedition to the Niger during the years 1841-2 comprising an Account of the Fever which led to its abrupt Termination* (London John Churchill, 1843), pp. 1-25

[12] *The Whole Works of that Excellent Practical Physician, Sydenham*, 7th ed., corrected from the original Latin by John Pechey, M.D. (London: M. Wellington, 1717), p. 47. James Lind, *An Essay on Diseases Incidental to Europeans in Hot Climates*, 6th ed. (London: J & J Richardson [and others], 1808), pp. 318-319. James Lind also advised, "Take a little bark occasionally in damp weather, or during an easterly wind, to prevent a relapse " *Ibid* , pp 328-329

[13] Dr Millar of Glasgow wrote in 1818 that "it is only by a universal sweep of the sick into Fever Hospitals, joined to a universal or nearly universal purification of their dwellings, that anything is to be hoped for in the way of suppressing our epidemic." Cited in Charles Creighton. *A History of Epidemics in Britain*, 2nd ed.

> Fever is a genus consisting of several species, and each species presents many varieties. The fever of one country is not the same as the fever of any other country; in the same country, the fever of one season is not the same as the fever of any other season; and even the fever of the same season is not the same in any two individuals.[15]

Treatment modes varied widely, consistent with the theory that there were many species of fever, but many therapists agreed that the main pathophysiologic processes affected the circulatory system in an excitatory manner and that congestion of various parts of the body was the result. The major therapeutic measures of bloodletting and mercurials were intended to correct these abnormalities as vigorously and as early as possible. James Johnson, a widely read author on tropical medicine, urged practitioners to "Bleed boldly and decisively till the head and praecordium are relieved, or draw no blood whatever."[16] Mercurials were intended to clear congested organs such as those of the intestinal tract. Other measures such as diaphoretic powders were aimed at assisting Nature in her healing process.

Later in the clinical course, tonics such as cinchona bark, arsenic, and wine were given during the afebrile period to restore general vitality. Many practitioners such as James Johnson believed that the Bark was specifically contraindicated during the acute febrile phase.

It may seem difficult to understand why Sydenham's clear directions on treatment of the ague were so disregarded. However, in addition to theoretical considerations about relating bleeding and mercurials to

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(London: Frank Cass, 1965; 1st ed , Cambridge: Cambridge University Press, 1894), vol 2, pp 178-179. The London Fever Hospital opened in 1802 and a similar one in Leeds in 1804. A debate went on about whether "fever beds" or "fever wards" should be maintained in general hospitals *Ibid* , pp. 160-161, 164, 165.

[14] William Pym, *Observations upon Bulam Fever, which has of late years prevailed in the West Indies* (London· J. Callow, 1815), J. O M'Williams, *op cit* (n 71 above), p 198. James Boyle, *A Practical Medico-Historical Account of the Western Coast of Africa* (London Highley, 1836), pp 222-223.

[15] Southwood Smith, *A Treatise on Fever* (Philadelphia. Carey and Lea), p. 47.

[16] James Johnson, *The Influence of Tropical Climates on European Constitutions*, 3rd ed. (Philadelphia: Dobson, 1821) p 21

excitation and congestion, we must understand that it was difficult to obtain reliable preparations of the Bark which would produce consistent results. Not only did the original sources in South America vary in potency, but as Paris said in 1833, "In the form of power, cinchona is always more or less adulterated."[17] Therefore, the Bark was not used widely as an effective curative or preventive agent, although some naval surgeons did use the drug prophylactically in isolated circumstances, and most warships had small supplies of the Bark or quinine aboard.

The increasing availability of quinine as a reliable and much less distasteful drug began to have an effect in the 1830s on the priorities given to bloodletting and mercurials by some practitioners on the African coast. The problems of dosage and timing had not, however, been worked out adequately. For the most part, doses of only 2 or 3 grains were given when at least 10 to 20 grains were needed. Even the small dose was almost never given during the acute phase of the disease. Quinine as a prophylactic agent was seldom employed, since most Navy veterans discounted its usefulness. During the desperate weeks of the 1841 Niger expedition, however, a young surgeon, T.R.H. Thomson, who was sent back down the river with the first ship, began to use larger doses of 8-10 grains per day during the acute fever stage and achieved excellent results. He also began to experiment on himself by taking a prophylactic dose of 10 grains of quinine daily. However, these events were not published until 1846.[18]

Other developments in Britain in the 1830s were to have a major influence on the control of fever in West Africa. One crucial change was the increasing interest in statistical studies. When the new British Association for the Advancement of Science was organized in 1831, a statistical section was included. Statistical societies started in the major cities of Manchester, London, Liverpool and Glasgow; their goals were to analyze social problems using statistical tools.[19] In 1835 a young Army officer, Alexander Tullock, who had instigated numerous reforms as a lawyer working in India,

[17] J A. Paris, *Pharmacologia* (London: Sherwood, Gilbert and Piper, 1833), p. 462.

[18] T. R. H. Thomson, "On the value of quinine in African remittent fever," *Lancet*, 1846, *1:* 244-246. Other medical practitioners working in malarious areas on the frontiers of their societies were making observations on the prevention of fever with quinine. These therapeutic pioneers do not seem to have been in communication with one another, nor did their results seem to have much impact on the practice of the medical centers. John Sappington, working in the Mississippi Valley and F. C. Maillot in North Africa both used quinine prophylactically in adequate doses in the 1840s. Dr. John Sappington, Saline County, Missouri, *The Theory and Treatment of Fevers* (Arrow Rock. Published by the author, 1844), pp 84, 85, 96. François Clement Maillot, *L'Oeuvre de F. C. Maillot*, edited by P. Tropland (Paris: Alger, Remordet, 1893), pp. 80-81.

[19] *Journal of the Statistical Society of London* (J S.S.L.), 1838, *1.* 52.

published in a military magazine a comparative twenty-year statistical study of mortality in Indian Army posts.[20] Earl Grey, the Secretary of War, almost immediately arranged for Tullock to analyze British Army records for postings throughout the Empire. These comprehensive analytical reports were published as Parliamentary Papers in 1838 and led to some administrative changes, particularly in the Caribbean region.[21] The Navy also began statistical analsyis of health files in late 1836, but their first Report was not published until 1841 and included few significant findings; for some reason, West African reports were not included in this first Navy study.[22]

During this time the British Government and people were paying unusual attention to West Africa; this interest continued throughout the 1840s. British taxpayers were spending about 10 million pounds in the anti-slavery movement; in addition, they gave 20 million pounds as reparations to slave owners. Up to 30 ships and 3000 men were assigned to patrol the African coastline. Morbidity and mortality for crews chasing slaving ships in the lagoons was extremely high, and the annual death rate for the African fleet was between five and six percent, mostly due to fever.[23]

Dr. Alexander Bryson, a seasoned Naval surgeon who had served on three different ships in West African waters, was appointed to edit the second Report for the years 1837-43, and this time materials from the African stations were included. Before Bryson could complete this study, the Lords of the Admiralty requested in October 1846 that Sir William Burnett, Director General of the Medical Department of the Navy, prepare a special report

> upon the African coast, upon the illnesses contracted there—the localities which are regarded as most injurious to the health of those employed,—the precautions which may be taken to divert or diminish Fever, and the modes of treatment that may be regarded as most effective. (Underlining added)[24]

Under Burnett's direction, Bryson worked for the next four months analyzing medical and nonmedical records of each ship which had been in

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[20] Alexander M. Tullock, in *Colburns United Service Magazine*, 1835, cited in *D. N. B.*, vol. LVII, p. 306.

[21] Alexander M. Tullock, "Statistical Reports on the Sickness, Mortality, and Invalidicy among the troops in Western Africa, St. Helena, the Cape of Good Hope, and the Mauritius," *Parliamentary Papers*, vol 30 (London: W. Clowes, 1840).

[22] John Winslow, M.D., R.N., *Statistical report of the Health of the Navy 1830-1836* (London: William Clowes and Sons, 1841).

[23] Christopher Lloyd and Jack L. D. Coulter, *Medicine and the Navy* (Edinburgh: E & S. Livingston, 1957-63), vol 4, p 162.

[24] Memorandum of Oct. 2, 1846, to Sir William Burnett from the Lords of the Admiralty Public Records Office, London (P.R O) ADM.105/39 3/53.

West African waters, going back to 1820 whenever possible. He reviewed all kinds of activities relating to health and disease, constructed statistical tables, calculated simple arithmetical mortality figures, and made very careful comparisons of experiences of different crews under similar conditions. The following is an example of his study:

> Two boats were detached from the *Hydra* in the year 1844 to examine the Sherbro River; the whole of the men were supplied with bark and wine, and not one of them was taken ill, while the whole of the Gig's crew, with the exception of the captain, who were similarly exposed for two days only, without being supplied with either bark or quinine contracted fever of a dangerous character.[25]

Citing a number of such cases, Bryson came to the following conclusions:

> Cinchona bark and the sulphate of quinine are both extremely useful agents for the prevention of fever, when properly administered on these expeditions; and although it would appear their powers have been considerably underrated, and their administration is apprehended, but indifferently understood, still the numerous instances on record in which they have been successfully employed, leave no room to doubt that their more general use upon the station is most urgently required.[26]

Furthermore, at the same time, when Panum was studying the epidemiology of measles and Snow that of cholera, and while the contagionist controversy was raging, Bryson achieved a clear idea of the length of incubation time for malaria and the resulting implications for the continuation of quinine prophylaxis after possible exposure:

> From previous observations made on the coast, followed by a careful review of the various circumstances connected with exposure on shore and on boat service, it is firmly believed that although neither quinine nor bark has the power of preventing the germs from lodging in the system, where they may lie dormant for a period of from fourteen to twenty days, or even longer, nevertheless, from their peculiarly antagonistic properties, they most decidedly have the power in many instances of preventing their development and pyrexial action. Hence the frequently supposed failure of the medicine persisted in for a sufficiently long time after exposure to the exciting causes; namely throughout the entire probable period of incubation.
>
> It is therefore suggested that it would be advisable not only to administer daily one of these febrifuges so long as they are exposed to the influence of the land and the vicissitudes of the weather in open boats, but to continue its use for at least fourteen days after their return on board.[27]

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[25] Alexander Bryson, *A Report on the Climate and Principal Diseases of the African Station* (London W Clowes and Sons, 1847), p. 218.

[26] *Loc cit.*

[27] *Ibid*, p 219.

By June 1847 the 250-page *Report on the Climate and Principal Diseases of the African Station* was printed, copies were presented to the Lords of the Admiralty and were distributed throughout the Medical Department of the Navy. Special shipments of the report were made to officers and surgeons of the African fleet.[28]

Burnett also took an extremely important additional step to implement Bryson's recommendations. He requested and obtained authorization from the Lords of the Admiralty for a new program of mixing quinine with wine in England before shipping it to foreign stations in casks or bottles which would be stored in locked chests.[29] Bryson later wrote,

> The object in supplying the cruisers with medicated wine chests was, that they might at all times be ready and at hand to put into boats suddenly required to proceed on detached service. Thus the quinine—which, when carried in paper, or bottles, was not only apt to be lost or blown away, but had to be given in uncertain doses, and therefore could not be husbanded—was secured, and the wine was effectively destroyed for any other purpose.[30]

Bryson did not say that often when wine was given along with Bark or quinine, unmixed, the sailors would drink the wine and throw the Bark and quinine overboard. Such action could be rationalized if necessary, for wine was supposed to have intrinsic tonic qualities of its own.

By April of 1848, after about nine months, stores of quinine in wine were on their way to West Africa with an explanatory memorandum from Burnett.[31] The Army Medical Department also made changes in its regulations regarding the uses of quinine late in 1848 and began to get good reports in 1850.[32]

Alexander Bryson was the key person in solving the difficult problems of malaria prevention. Many different kinds of experience and thought contributed to his ability to analyze the phenomena and then put together an implementation program which would have a systematic effect on disease prevention in West Africa. His brief tours of duty on ships posted to the African Station in 1831-32 and 1841 gave him first-hand experience. He was incorporated by Dr. William Burnett about 1843 into the pioneering process of statistically analyzing the Navy's medical records as a whole, and then into a study of West Africa in records in particular, in 1846, a study which resulted in the important Report of 1847.

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[28] Memorandum of June 6, 1847, from H G. Ward, Secretary to the Lords of the Admiralty, to Sir William Burnett, Director General of the Medical Department of the Navy P R.O ADM 105/39 1279

[29] Memorandum from the Lords of the Admiralty to the Medical Director General. June 29, 1847, P R O ADM. 105/39.

[30] A. Bryson, "On the prophylactic influence of quinine," *Medical Times and Gazette*, Jan. 7, 1854, n.s. *8*· p. 6.

[31] Memorandum from Sir William Burnett to the Lords of the Admiralty that the quinine wine had been shipped to the medical stores at the Ascension Islands and to the ships in the naval squadron on the African station with receipts for medicated wines at Deptford. 50 cases of 8 bottles each and 36 casks containing 6 gallons each. March 28, 1848, P R.O. ADM. 105/39 13.

[32] Curtin, *op cit.* (n 6 above), pp 356-357.

Bryson's writing seems to show some evidence of his being influenced by yet another mode of analysis gaining prominence in the 1840s. In 1843, during the year Bryson probably began his major studies, John Stuart Mill published his work, *A System of Logic: Ratiocinative and Inductive,* a book which quickly received attention from a large audience.[33] The likeness of Bryson's methods to the principles which Mill set forth appears great.[34] The following passages illustrate the similarities. In Mill's "second canon" or "method of difference" this statement is found:

> If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.[35]

Bryson applies this principle to his studies of disease. In the preface to the 1849 report he explains his rationale of comparison:

> The medical treatment, therefore, viewed in this light (of comparison between groups which were treated differently) inevitably becomes an important element in any statistical inquiry respecting the relative degrees of sickness and mortality of different classes of civilized men. It will, nevertheless, however, be found that opportunities seldom occur for fairly contrasting the relative results attributable to different modes of treating the same malady; for, even under the most favourable circumstances, unless all things were the same— the time, the place, the conditions of the patients, the type of the disease and the exciting cause—it would not be safe to place much reliance on any comparative results so obtained.[36]

With publication of the 1847 *Report* Bryson was thrust into the center of scientific and intellectual circles in London. J.F.W. Herschel, noted astronomer and mathematician, asked him to contribute a chapter on "Medicine and Medical Statistics" for the *Manual of Scientific Enquiry* which he was editing for the Navy.[37] Other contributors included Whewell, Charles Darwin, Richard Owen, and Evans Pritchard.

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[33] John Stuart Mill, *A System of Logic, Ratiocinative and Inductive. Being a connected view of the principles of Evidence and the methods of Scientific Investigation,* 8th ed., (New York: Harper and Bros., 1847; 1st ed , London, Parker, 1843)

[34] The writer has not been able to find specific evidence that Bryson was directly influenced by Mill. However, considering the rapid sale of the first edition and the widespread enthusiasm for the book, it would not be unlikely that Bryson had read and studied it.

[35] Mill, *op cit.* (n. 33 above), p. 218

[36] Alexander Bryson, *Statistical Report of the Health of the Navy (1837-43),* 2nd ed. (London· Parliamentary Papers, ordered by the House of Commons, 1849), vol. XXXII, p. 281.

Bryson was also participating in epidemiological developments, which then were focussing on problems such as fever, cholera, and measles. Bryson read a paper on cholera at the first two meetings of the Epidemiological Society of London in December 1850 and January 1851.[38] His military employment gave him extraordinary advantages in pursuing these scientific endeavors: records were kept carefully, comprehensively, and were preserved for long periods; and the postings and movements of ships exposed to similar influences allowed him to observe groups in ways not possible in civilian society. In effect he was able to study "experimental" and "control" groups in retrospect.

Perhaps most remarkably, Bryson was able to take the findings which he had made using statistical, epidemiological, scientific methods and translate them into practical statements which could be implemented and tested. He could follow the process through because he was posted to the Medical Director General's Office and continued there until the end of his career. In 1864 he was himself appointed Director General of the Navy's Medical Department.

Bryson reported in 1854 that quinine prophylaxis had achieved excellent results and expressed concern for the crews of merchant ships, who were not receiving similar treatment.[39] Then the opportunity for a major test of the new quinine regimen was provided. The British Government decided in 1854 to act upon a report sent by Barth in 1853 from the Sahara, stating that the eastern tributary to the Niger River, the Tchadda (or Benue) River, was readily navigable and would make an excellent inland commercial waterway. Bryson was on hand to supervise medical preparations for this expedition, the first since the disaster of 1841. Specific care was given to prophylactic quinine administration.[40]

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[37] Sir John F W. Herschel, editor, *A Manual of Scientific Enquiry prepared for the use of Her Majesty's Navy. and adapted for travellers in general* (Published by authority of the Lord's Commissioners of the Admiralty. London, J Murray, 1849), p. iii

[38] Alexander Bryson, M D., Surgeon R.N., *On the Infectious Origin and Propagation of Cholera* (London· William Tyler, 1851), p iii

[39] Bryson, *op. cit* (n. 30 above), p 7.

[40] *Ibid.*, p 7.

Unexpectedly the young surgeon-naturalist assigned to the trip, William Baikie, was placed in command of the expedition just before the ships entered the delta of the Niger River in 1854. Proceeding precisely according to plan, Dr. Baikie directed the journey up the Niger and Tchadda Rivers and completed mapping activities along the 700 miles of the rivers during a four-month period. Prophylactic quinine in wine was given in doses of 6-8 grams per day from the day before the ships entered the river until three weeks after they returned to the coast. Not one life was lost, and only a small amount of mild illness occurred. For the first time, Europeans were able to live for extended periods along the inland rivers.[41]

For better or worse, a new era had opened. Dr. Baikie himself returned to the upper Niger River in 1857 and established a permanent colony at Lokoja, where the Tchadda River joins the Niger. He remained there with a small settlement until 1864, as the informal representative of the British Government, while engaged in a variety of activities which included establishing a trading center, mapping the surrounding area, studying local African languages, and translating parts of the Bible and the Book of Common Prayer into the Hausa language.[42] British ships came irregularly to Lokoja in the 1860s, but by the 1880s British influence had been established in the Niger basin and was an important part of the European "Scramble for Africa."

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[41] William Balfour Baikie, M D. R.N., F.R.G.S , F.S.A., Scot , *Narrative of an Exploring Voyage up the River Kwo'ra and Bi'nue (commonly known as the Niger and Tsa'dda) in 1854* (London: John Murray, 1856), pp 452-453.

[42] S. Jenkinson, "Dr Baikie, M D., Surgeon, R.N., naturalist, traveller, philologist," *J Royal Med Service*, 1943, *29*· 203–205

A CLINICAL IDEAL: PARIS 1789

TOBY GELFAND*

During 1789-90, the Paris *Société Royale de Médecine* formulated a clinical ideal to the extent where it could serve as the fundamental theme in a lengthy project for a new medical system or "constitution" for France. Between the *Société*'s *Nouveau Plan de Constitution* (1790), the *Comité de Salubrité's Projet de Décret* (1791), and law of 4 December 1794 finally establishing the new medical schools, the flow of ideas is fairly clear, remarkably so considering the ideological turmoil of these early revolutionary years. The conversion of hospitals into medical schools remained at the center of the reform.[1]

The *Société* and the *Comité* together received hundreds of suggestions and plans which poured into Paris from all parts of France. Some of this material was published at the time; much more survives in manuscript or only in the form of entries by title in the *Comité de Salubrité's procès verbaux*.[2] To analyse systematically this immense discourse for the *idées forces* of a clinical ideal would be a laborious task.[3] Fortunately, it is, for our purposes, probably also an unnecessary one. For, although the *Société* and the *Comité* welcomed suggestions from all interested parties, they were not conducting a national plebescite on medical reform. Neither group intended to be a passive conduit for the ideas of others.

The *Société* evidently developed its project largely in regular private

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[1] See D. B Weiner, "Le droit de l'homme à la santé—une belle idée devant l'Assemblée Constituante 1790-1791," *Clio Medica*, 1970, *5* 209-223. For extensive discussion and documentation of this and the following two paragraphs, see my unpublished doctoral dissertation, "The Training of Surgeons in Eighteenth-Century Paris and Its Influence on Medical Education," Johns Hopkins University (Baltimore, 1973), chaps. X-XII. On the central role of the Société Royal de Médecine as the locus of progressive medicine in France during the last decades of the Old Régime, see J P. Peter, "Disease and the Sick at the End of the Eighteenth Century," transl E Forster, in *Biology of Man in History*, ed R Forster and O. Ranum (Baltimore: Johns Hopkins University Press, 1975), pp 81-124; and C. C Hannaway, "The Société Royale de Médecine and epidemics in the Ancien Régime," *Bull Hist Med* , 1972, *46* 257-273

[2] Paris, Archives Nationales, AF I* 23

[3] An extremely fragmentary attempt using published materials is in M Foucault, *Naissance de la clinique*, 2e éd (Paris, Presses Universitaires de France, 1972), pp 37-52, 63-86

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séances and committee meetings. Thus, a relatively small group of Paris physicians were the major contributors. The *Nouveau Plan* is sometimes attributed to one man—Vicq d'Azyr. By virtue of his official position, his contacts with government officials, his powerful intellect and apparently equally forceful personality, the permanent secretary tended to overshadow his other gifted and influential colleagues. In fact, Vicq d'Azyr was but one (and probably not the most important) member of a committee within the *Société* which began work on a new medical constitution in August 1789.[4]

It would be imprudent to deprive Vicq d'Azyr of priority of authorship simply to confer credit upon another individual for what was clearly a collective effort. Between October and December 1789, the *Société* heard reform proposals by Andry and Fourcroy, Vicq d'Azyr, Poissonier, Doublet, and others.[5] One contributor was especially conspicuous: Nicholas Chambon de Montaux (1748-1826), physician at the Salpêtrière hospital, member of the *Société* since 1780, and author of a number of clinical treatises.[6]

At the very *séance* in which the *Société* committee began its work, Vicq d'Azyr drew special attention to Chambon's "plan for teaching clinical medicine"; this was a reference to a manuscript just completed or, perhaps, to Chambon's recently published *Moyens de rendre les Hôpitaux plus utile à la Nation* (1787). In any case, in October 1789, Chambon presented a new memoir on clinical medicine. His colleagues heard the project several times, reviewed it favorably, and formally adopted Chambon's "conclusions" on 27 October 1789. His "idea for excellent reforms," they noted, would be incorporated into the *Société*'s comprehensive project. The final plan in fact acknowledged explicitly Chambon's and Doublet's proposals for hospital medicine.[7]

Chambon's memoir of 1789 thus occupies an intriguingly strategic niche in the history of the clinic, nestled, as it were, like the smallest in a series of Chinese boxes, near the conceptual core of a reform plan which in turn was central to contemporary medical aspirations. If one were to choose a single text in which to search for a "clinical ideal of 1789" (as I intend to do for the remainder of this paper), then Chambon's never-published manuscript seems a most eligible candidate. With it, one can identify and analyse certain dominant elements of the medical reform movement.

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[4] Paris, Académie de Médecine, Ms 11 bis (séances 4 and 7 August 1789). Vicq d'Azyr joined the 14-member committee at the second séance Among his colleagues were Andry, Carrère, Fourcroy, Chambon, Doublet, Dehorne, Hallé, and Thouret

[5] *Ibid* The loss of the Société's *procès-verbaux* for 1790 prevents tracing this effort, which included contributions by corresponding members The final plan was completed by October 1790.

[6] See J. Génévrier, *La vie et les oeuvres de Nicolas Chambon de Montaux* (Paris Steinheil, 1906). Chambon's publications in the 1780s included books on anthrax, fevers, diseases of women and childbirth, and a collection of clinical observations.

[7] *Nouveau Plan de Constitution pour la Médecine en France*, in *Hist. et Mém de la Société Roy de Méd.*, IX (Paris, 1790), p. 63 Chambon's Ms "Projet d'enseignement de médecine pratique" and the "très detaillé" committee report dated 23 October 1789 are in Académie de Médecine, Archives de la Société Royale de Médecine, Carton #115. Chambon presented the "Projet" at the séance of 16 October. Further discussion took place on 23 Oct. and 18 and 22 Dec See Acad de Méd Ms 11 *bis*.

The Clinic and Medical Knowledge. Chambon understood the clinic as essentially a didactic instrument which would teach medical students how to recognize diseases. At the patient's bedside, the student, fully equipped with the theory of his art (*des études préliminaires*), encountered something ostensibly quite different and, for him, quite bewildering—the complexity of diseases as they actually manifested themselves. He came to realize the gross inadequacy of books or theoretical lessons, and indeed, of reason, to communicate knowledge of pathological conditions, for "authors, as well as masters, in discussing a given morbid condition are forced to present an abstraction of its main characteristics."[8] Formal accounts did not (and could not if they wished to be of finite length and comprehensible) attempt a total picture of diseases, replete with the multiple variations which occurred. No simple one-to-one relationship held between describable symptoms and physical effects of diseases and their underlying "states." Similar outward manifestations in two patients might mask quite different, even opposite, disease states. This happened commonly in fevers; conversely one kind of "inflammatory" fever could present a multitude of different symptoms.

Furthermore, "accidents" (by which Chambon meant any and all environmental factors) complicated the ideal textbook description. These arose from age, sex, temperament, climate, habits, nourishment, and moral disposition. City life acted via the nervous system to pervert conditions which in the countryside would be simple and easily recognizable.[9] Hereditary "vices" also caused aberrations in pathology.

Clinical experience somehow taught what authors and masters could not. It would perhaps be unfair to inquire exactly how, since this learning process, by definition, operated at a different level than cognitive transmission. Nevertheless, Chambon did not attempt to mystify clinical learning; if anything, he reduced it to a simple inductive model of cumulative observations. Observation lies at the heart of clinical medicine and is the guiding methodology for its three principal tasks: (1) the compilation of an exact history of the patient—this includes all that has occurred prior to the doctor's active intervention, (2) the enumeration of the medical means employed to deal with the ailment and their effects, and (3) the "opening of the cadaver" in the event of the patient's death. This protocol imparted medical knowledge by furnishing concrete and complex examples (*circonstances donnés*) of what had been imperfectly communicated in school.

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[8] Chambon, "Projet." The manuscript is unpaginated, but the paragraphs are numbered. The citation comes from paragraph #5, and the remainder of this section draws from ##5-13

[9] A commonplace notion in 18th-century medicine, the assumed simplicity of diseases in the countryside, fit in with Rousseau's cult of the primitive Peter, "Disease and the Sick," claims that provincial correspondants of the Société de Médecine began to question such assumptions as a result of their experience. Nonetheless the doctrine persisted

Chambon's concept of clinical knowledge was scarcely novel. Nor did he make any claims for originality. Besides implicit acceptence of ancient Hippocratic doctrine on the role of environmental factors, i.e. the so-called non-naturals, Chambon explicitly invoked Boerhaave as his model. It was the 18th-century Dutch physician, Chambon noted, who had made observations at the hospital bedside the basic data from which teaching by example was conducted. Chambon's three-phase method and the technical terms he employed—"predisposing and remote causes"; "character of the disease," i.e. the "morbidic revolutions" which occurred as "nature" struggled with the malady, sometimes aiding the cure, but at other times hastening death; and the use of autopsy to verify or correct the prior clinical history—all were to be understood in the same sense as Boerhaave understood them.

Chambon's concept of diseases as specific entities to be studied "in the field" in the manner of the natural historian derived from Sydenham and the medical nosologists of the 18th century. The clinic provided a space not for research and discovery but rather for recognition and labelling of disease species whose classification had previously been established, whose history, Chambon noted, was already written.[10] The hospital clinic offered the most efficient means by which the young doctor could become a practitioner; it imparted much knowledge to the student, but little to the art. In short,

[10] Chambon's likening of the young doctor to the naturalist who could recognize minerals in his cabinet, but became hopelessly confused "s'il déscend dans les abîmes où les métaux sont confondus entre eux comme avec les terres ou les pierres qui les environment," placed him firmly in the tradition of medical taxonomists See Foucault, *Naissance*, pp 1-19

Chambon's notion of the relationship of the clinic to medical knowledge was thoroughly traditional.[11]

The Clinic as Written Observation. Turning from the epistemological aims of the clinic to its actual functioning, Chambon attached central importance to keeping serial written records.[12] As illustrations, he reproduced and described in detail two charts which he used at the Salpêtrière: the *feuille de visite* or record of daily rounds and the *cahier d'observations* or case history book. Both documents monitored the hosptial patients at short periodic intervals over time. The *feuille de visite* listed in successive columns: the patient's bed number, disease designation, date of onset of the malady, date of entry into the hospital, medications, dietary regimen, and clinical observations. (See Fig. 1.)

Precise dates enabled one to calculate, in good Hippocratic fashion, "days of revolution" for each ailment, and to determine "times of crisis" so that appropriate treatment could be conducted. The *feuille de visite* was primarily intended as an *aide-mémoire* for the busy hospital doctor and to keep him informed of changes which occurred during his absence, such as the "conversion" of a tertian into a continuous fever. The *cahier d'observations,* similar in structure to the *feuille,* discussed the clinical course in greater detail since it served an explicit teaching function.

Similar kinds of systematic records of hospital patients are mentioned in medical literature of the second half of the 18th century. Chambon noted their use in military hospitals.[13] In 1772, Jean Colombier, the future inspector-general of French military hospitals and a member of the *Société Royale de Médecine*, proposed a chart or *cadastre* for military hospitals "to be placed at the head of each patient's bed"; clinical changes were to be noted at two-hour intervals. Colombier cited English precedents for the *cadastre*.[14]

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[11] A traditon which continued to employ humoral pathology and Latin as in Chambon's *Observationes clinicae* (Paris, 1789) The Société de Médecine in giving its endorsement to this collection of case histories of patients of the Salpêtrière agreed with the author's attribution of acute lung diseases to a "humoral fluxion " See p 475

[12] Chambon called the clinical charts "deux points essentiels" for the implementation of his plan "Projet," # 14. This section is based on ## 14-22

[13] *Ibid* , # 14 He faulted military hospitals for not keeping track of dates, but left the precise format of the clinical chart to the taste of each hospital physician

[14] J Colombier, *Code de Médecine Militaire* (Paris, 1772), vol 2, pp 18-21 Colombier reproduced the "Cadastre de Clifton, médecin du prince de Galles," *ibid* , p. 143, as well as his own version. Colombier apparently was familiar with Francis Clifton's *Tabular Observations Recommended as the Surest Way of*

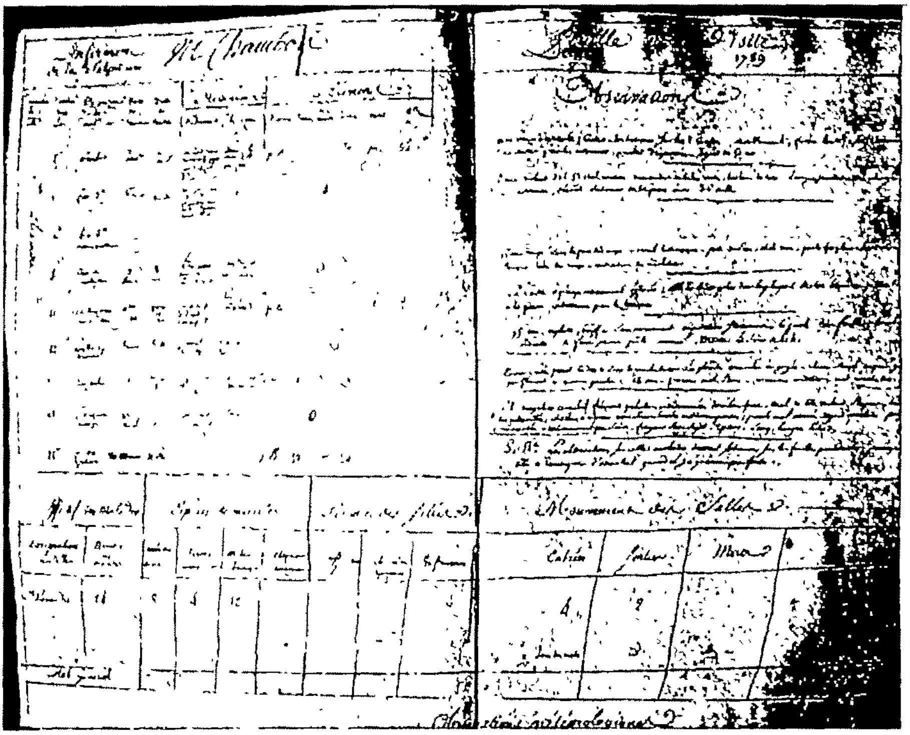

Fig. 1. Chambon's *Feuille de Visite* at the Salpêtrière (1789). Courtesy of the Bibliothèque of the Académie de Médecine, Paris.

Although the subject needs further study, I submit that clinical registers of this type were an innovation, one which had important implications for medical theory as well as practice. Colombier remarked that Sauvages "who has published the best nosology to date" would have done better to follow a *cadastre* than scattered observations. One could, he thought, use such systematic records to build a whole new history of diseases.[15]

Clinical charts differed from traditional clinical literature in that they recorded a series of discrete instantaneous bedside observations rather than a unified later reflection. As discourse, the chart may be likened to a newspaper or journal whose entries recount a story in progress. The books of an Hippocrates, a Sydenham, or a Morgagni presented retrospective accounts of completed cases. In addition, the traditional case history

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Improving Physic, which was published in London in 1731. I am indebted to Dr. Lloyd G. Stevenson for this reference.

[15] *Ibid.*, pp. 143-144

derived from the interaction of one physician with one patient, and a summation of that basic unit over time. The clinical register, inherently more complex, recorded multiple patient-doctor interactions at any given time.

Clinical charts obviously answered pragmatic needs of urban and military hospitals, whose growth had been extraordinary during the 18th century. Large institutions endowed with capital resources and working staffs, the hospitals required sophisticated regulations in order to function. Written records on patients were as necessary a part of their organizational structure as financial and other records.

A complex legal-administrative structure, of course, was not peculiar to hospitals. Other institutions dealt with similar problems in much the same manner. Their response exhibits one common feature which transcended practical needs. One notes a positive zeal to record, to order, to manage, and, ultimately, to master the complexity of experience by means of precise, concise language. Materials as diverse in provenance as government reports by intendants and the minute books of academic societies share this confidence in the power of the written record.[16] They collectively express a mentality which, in a word, was bureaucratic and which became translated into immense archives. There is a certain uniformity, if not yet standardization, in this institutional discourse of the late 18th century, as if guidelines were being followed.[17]

What may have begun as an administrative imperative for hospitals had direct consequences for the acquisition of medical knowledge. Students were to keep hospital journals, for only they could be required to be present virtually continuously: " . . . it is indispensable to fix young doctors in the hospitals and to have them stay there."[18] The consultant physicians, who visited but once a day or less often, would inevitably miss critical changes in the clinical course, such as the shifting of the "painful point" in "rheumatic pleurisies." Through the intensity and responsibility of his hospital experience, the student-reporter gradually learned his art.

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[16] Minutes of academies, for example, often were written in several drafts, the *procès-verbaux* being a refined version of the *plumatifs*. On the subject of the legalistic and bureaucratic nature of 18th-century academic culture, see D. Roche, "La siècle des lumières en province," unpublished *thèse* for the *doctorat d'état* (Paris, 1973), *passim*. I wish to thank the author for making available to me a copy of his *thèse* due to be published by Mouton

[17] This is evident in the *Délibérations de l'Ancien Bureau de l'Hôtel-Dieu*, ed. L Brièl, *Collection de documents pour servir à l'histoire de hôpitaux de Paris* (Paris, 1881-1883), 2 vols In the case of government questionnaires or *enquêtes*, definite guidelines often were prescribed for the responses The Société Royale de Médecine followed similar kinds of procedures in seeking to inventory disease problems and personnel of the realm. See Peter, "Disease and the sick," and J P Goubert and F Lebrun, "Médecins et chirurgiens dans La Société Française du XVIIIe. Siècle," *Annales Cisalpines d'Hist Sociale*, 1973· 119-136.

[18] Chambon, "Projet," #29

The reduction of visual to written observation formed an important element in the clinical ideal of 1789. Language interposed itself as an ordering principle between the observer and the observed. It is thus somewhat anachronistic to picture the ideal clinician of the time poised over his patient or over a cadaver directly perceiving disease. This figure, equipped with stethoscope and scalpel, would become the clinical hero of the age of Balzac. His less romantic predecessor might best be portrayed, as Jacques-Louis David painted his own doctor, holding a pen and writing clinical notes.[19] (See Fig. 2.)

Surgery and the Utilization of Hospitals. On the eve of the Revolution, Chambon's essay prescribed how hospitals could be better utilized for training physicians. In the case of the French surgical profession, the clinical ideal had, in large measure, already become a reality during the course of the 18th century. Young surgeons kept the bedside journals in military hospitals and their large urban counterparts. Surgeons had established the kind of intensive relationship with hospitals, involving residence, therapeutic responsibility, and clinical learning opportunities, which Chambon envisioned for medical students.[20] As the son of a surgeon, Chambon was sensitive to the disjunction between the two healing professions: " . . . it has been deemed appropriate to fix surgeons there [in hospitals] in the hope that the nation will thereby acquire men instructed by experience. The same reasoning holds for medicine."[21] How illogical it was, he wrote, to have hospitals inadequately provided with physicians and stocked with surgeons when medical diseases were ten times as prevalent as surgical or external ailments.[22]

Chambon offered a speculative "historical" explanation for the paradoxical distribution of medical personnel in hospitals. Military needs had originally demanded army surgeons. With the growth of the army, surgeons eventually set up military hospitals. The latter, in turn, had "perhaps . . . served as a model" for civil hospitals, thus leading to the present abundance of surgeons

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[19] David painted his physician, Le Roy, in 1783 See R. Cantinelli, *Jacques-Louis David* (Paris: van Oest, 1930). Le Roy is shown leaning on a Hippocratic treatise while writing his notes. David's portrait of Lavoisier (1788) conveys virtually the same image of the late 18th-century savant.

[20] I have discussed this development in "The gestation of the clinic," paper read at the International Congress of the History of Medicine (Quebec, 1976) and in "The Training of Surgeons," chap. VI

[21] Chambon, "Projet," #45.

[22] *ibid* , #40

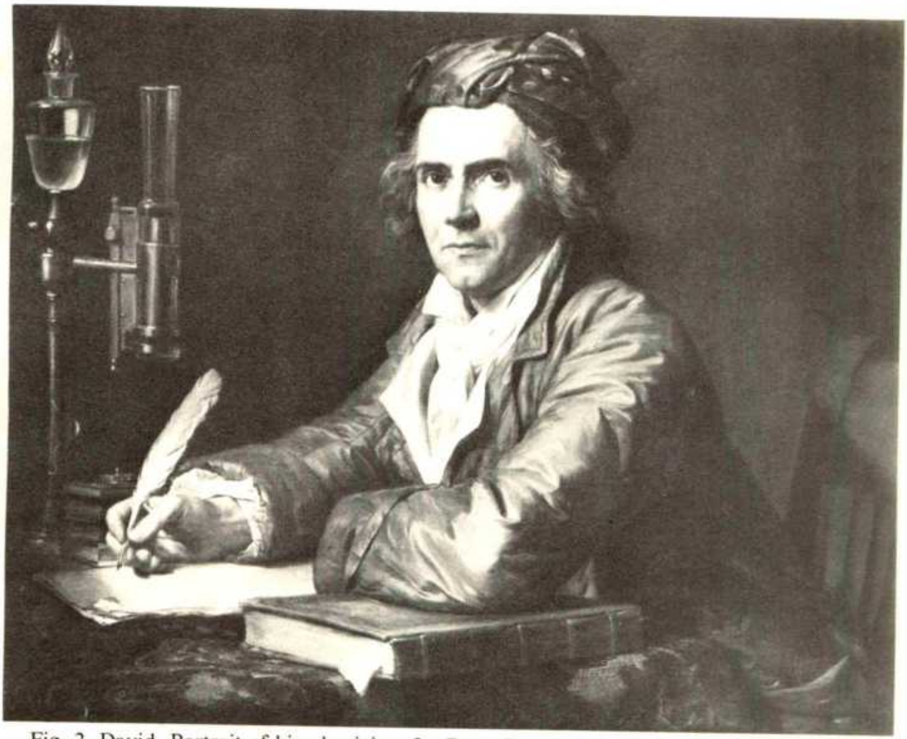

Fig. 2. David. Portrait of his physician, Le Roy. Courtesy of Musée Fabre, Montpellier.

and dearth of physicians.[23] Chambon had made the same argument in his essay of 1787, but now he introduced a stronger note of political protest. The military mentality of potentates, their ambition for conquest and desire for revenge, dictated measures for preserving the health of soldiers while, at the same time, the medical needs of the rest of the population were neglected. In 1787, Chambon had pleaded for someone "learned in the science of politics" to "enlighten the minister."[24] In 1789, still absolving the King from direct blame, he denounced "ministerial despotism" as a "monument of indifference" to the welfare of ordinary citizens: " . . . if these institutions [teaching hospitals] are beneficial, let us set up similar ones in the center of our towns."[25]

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[23] *Ibid.*, ##37-38.

[24] *Moyens de rendre les hôpitaux plus utiles à la nation* (Paris, 1787), p. 269.

[25] Chambon, "Projet," #39. Chambon appears to have been politically moderate during the Revolution. His brief tenure as Mayor of Paris (December 1792-February 1793) happened to coincide with the execution

Chambon recognized that the surgical profession had successfully exploited the hospital's potential for clinical training. The surgeons had tapped a resource which still awaited physicians in dozens, if not hundreds, of institutions all over France.[26] Yet, Chambon maintained an ambivalent attitude toward surgery's usefulness as a model for medical reform. Young surgeons, he believed, did not and could not adequately fulfill the medical responsibilities entrusted to them in hospitals. They lacked the scientific principles of the entire art. It was necessary, not simply to imitate hospital surgeons, but to put young doctors in their place.

Unlike Vicq d'Azyr and other physicians who pointed out the fundamental analogy between external and internal diseases and thus the relevance of surgery for medical knowledge, Chambon kept silent on this theoretical question, a question which implied the "reunification of surgery and medicine."[27] Nonetheless, surgical precedents for the utilization of hospitals clearly contributed to Chambon's clinical ideal.

The Clinical Contract. Hospitals, Chambon declared, gave the sick poor comfortable shelter, diligent care, nourishment, and medical remedies. The teaching hospital which he envisioned would provide still better care because young doctors would be present to supervise treatment and to modify it according to changes in the clinical course.[28]

What could the sick give in return? Chambon's answer was simple. Lacking all other means of recompense, hospital patients in effect owed their bodies to society. If the disease terminated in death, provision of the cadaver for dissection was expected. In any case, "the government" could legitimately require from hospital patients "the history of their infirmities . . . and the means by which they have been cured."

The patient's contribution appears modest and reasonable by modern standards. For Chambon and his contemporaries, however, it was a

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of Louis XVI, in which act he took an official role. Later, he claimed he had tried to protect the King, but, at the time, of course, he declared himself a "véritable Républicain." *Lettre de Nicolas Chambon, maire de Paris aux citoyens, composant la Section de la Halle aux Bleds* (Paris, 19 Jan. 1793). Copy in Bibliothèque Nationale. See Génévrier, *Chambon*, pp. 18 ff.

[26] See Archives Nationales, F 17 1147, doss. 8, for list of some 40 towns with large hospitals suitable for medical teaching. This undated document (probably prepared in the 1790s) lists the number of beds in each hospital. Jacques Tenon drew up a list of more than 400 Hôtel-Dieus in France in 1792 and a grand total of 2352 "hospitals" in provincial "places." Most, of course, were tiny foundations. Bibliothèque Nationale, Tenon papers, NAF 22750.

[27] See O. Temkin, "The role of surgery in the rise of modern medical thought," *Bull. Hist. Med.*, 1951, *25*: 248-259; Gelfand, "The Training of Surgeons," chap. XI.

[28] Chambon, "Projet," ##30-34. This section is based on ##23-28.

substantial demand, one which doctors could not make in private practice. Disease, Chambon stated, was normally a private, even a shameful, matter, an "inviolable secret" between doctor and patient: "it is embarassing enough to have to expose one's ailments to a stranger's view when one needs his help; it would be insufferable to permit someone whose presence is not really necessary to cast a curious eye upon infirmities. . . . "[29]

Yet, in a hospital, the secrets of the body, secrets which often involved the intimate moral life of the sick person, were to be rendered public and to be made the subject of lessons. Chambon justified this clinical intrusion on economic, scientific and humanitarian grounds. A hospital patient was an unemployed citizen, one who could no longer support himself or his family or fulfill obligations to his fellow citizens by working. Nevertheless, "the suffering man does not cease to be a citizen; if there is a way in which he can contribute to the public welfare, his infirmities do not exempt him from a duty which gratitude and humanitarianism likewise imposed upon him."

In such circumstances, it was not cruel to take advantage of the sick, to oblige them to reveal their secrets. On the contrary, the hospital patient owed knowledge of the history of his ailment to society and, specifically, to the medical profession. His own care and cure depended upon similar contributions by other indigent patients to whom he owed a debt of gratitude. His payment of that debt in turn added to the corpus of medical knowledge, made clinical training of young doctors possible, and instilled in him, if he were at all sensitive, a "ravishing and tender satisfaction" inaccessible to the rich patient who merely contributed his gold.

The notion of an unwritten clinical contract between the sick poor and society appeared in other medical discourse of the period. The reciprocal obligations of one toward the other were a tacit assumption, if not always an explicitly stated provision, of voluntary hospital foundations in England and America and government foundations in France.[30] The concept had its roots not within Christian hospital or medical tradition, but in a century and more of Enlightenment political philosophy.

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[29] Chambon contrasted the *état monarchique* in which disease was a private matter with the ancient Greek republic Although he admitted that very little was in fact known about medical education in ancient Greece, Chambon nevertheless deduced that it must have been conducted in a public and open fashion " . chaque ville formant en quelque sorte une famille dont les moeurs, les usages et la manière de penser étaient à peu près uniforme, un citoyen n'était point étranger dans la maison d'un autre citoyen; une maladie n'était point un secret qu'on voulait cacher à ses voisins. En effet la pluspart des malades places à l'entrée des temples des Dieux recevaient des secours multipliés et là sans doute on recueillait précieusement les maximes des hommes célèbres qui allaient y donner leurs conseils. On ne peut pas douter non plus que les maisons particulières n'eussent été ouvertes aux disciples des savans . "

[30] See Foucault, *Naissance*, pp 82-86.

The clinical contract was a special application of the ideology of the social contract between the monarchical state and its constituent individuals. Chambon drew from Montesquieu's *Esprit des lois* the view that "all private interests were isolated" in a monarchical government as opposed to a republic in which communal relationships derived from the inherent political constitution.[31] The monarchical state, therefore, had to impose upon its people certain institutions necessary for social interaction and harmony. Otherwise, the poor would be abandoned in times of need.

Laws of "sociability and humanity" obligated the state to the hospital patient and vice versa. The political interests of the monarchy also required hospital foundations. Without such charitable institutions, discontent or "demoralization" of the poor was inevitable. This could easily lead to "general disorder" which ultimately threatened property owners much more than the working class.[32] Chambon recognized the hospital as an instrument for social order and control, and the clinical contract as a refinement of that function. In so doing, he associated himself with a tradition of social contract philosophers who sought to stabilize and improve society. At the same time, the model which they put forward—the contract—symbolized the legal, orderly, and somewhat impersonal protection of private property.[33]

The clinical contract broached a serious ethical dilemma which lay concealed under the harmonious façade of the social contract. What if the interests of society did not in fact coincide with welfare of the individual? What if the laws of "sociability and humanity" led in opposite directions? In the case of the clinic, it was not difficult to conceive of this in terms of research and learning opportunities versus the welfare of the hospital patient.

Chambon recoiled from the idea of dangerous human experimentation, though he believed the ancients had profited from the "inhumane" practice of observing the natural course of diseases without intervening.[34] In his essay of 1787, Chambon sought to allay popular apprehensions by framing a definition of clinical experimentation limited to the testing of drugs, already known to be beneficial for one disease, in an analogous ailment. Dangerous operations were to be undertaken only when the patient would otherwise die. Chambon vigorously defended the sanctity of the individual, citing an array of Enlightenment authors as well as Moses and Plutarch.[35]

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[31] See Chambon, *Moyens*, pp 169-178, for explicit reference to Montesquieu

[32] *Ibid* , pp 6-20. The expression *classes des ouvriers* is Chambon's, not mine.

[33] See P. Hazard, *La crise de la conscience Europèene* (1680-1715) (Paris: Boivin, 1935), *passim*, esp. pp 275-347.

[34] Chambon, "Projet," #11.

Not surprisingly, there is scant evidence that Enlightenment physicians ever transgressed moral barriers against medical experimentation. Yet it is worth noting that the issue became relevant once one accepted the use of the hospital for clinical learning. Chambon himself apparently tried out alternative remedies on patients at the Salpêtrière; the leading Paris surgeons conducted clinical trials at their hospice.[36] That such occasional empirical efforts probably were neither scientific nor inhumane in a systematic sense is beside the point. They had the potential to be either or both. The moral ambiguity of the Enlightenment social contract is perhaps most dramatically illustrated by another text dealing with medicine: Denis Diderot's article *Anatomie* in the *Encyclopédie*. Here, Diderot advocated radical experimentation on humans for the sake of scientific progress. An even more disadvantaged individual—the condemned criminal—would be sacrificed for the benefit of "humanity."[37] Diderot may not have intended his suggestion to be taken at face value. He probably used it as a polemical tactic against contemporary laws which prohibited scientific anatomical dissection and, at the same time, called for the terrible judicial torture of capital offenders. Nevertheless, Enlightenment values contained this darker strand. Utilitarian science, of which medicine was a prime example, raised serious questions about the sanctity of the individual.

The Calculation of Profits. Chambon's specific proposal—to place two young doctors in each 300-bed hospital—would require additional expenditures by hospitals. The resident physicians had to be given living quarters, meals, etc., at a cost which came to slightly more than 1200 £ per year. Yet the hospitals stood to gain, according to Chambon's calculations, a net profit many times greater than their investment.[38] Expert management and supervision of medical care would reduce the duration of illnesses. An intermittent fever, for example, could be cured in six weeks instead of the usual six to eight months. At 20 sous per hospital patient-day, one obtained a saving of 138 £ on the single case.

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[35] Chambon, *Moyens*, pp 154-163 He quoted Beccaria, Pufendorf, Montesquieu, and Grotius

[36] T Gelfand, "The Hospice of the Paris College of Surgery (1774-1793)," *Bull. Hist Med.*, 1973, *47* 375-393, Chambon, *Observationes, passim*.

[37] *Encyclopédie, ou Dictionnaire raisonné des sciences, des arts et des métiers*, vol. 1 (Paris, 1750), pp 409-410. Diderot noted several radical surgical procedures which he thought might be useful to try; e g , extirpation of the spleen, removing some portion of the brain, transfusions, etc. If the criminal survived, he would be given his freedom.

[38] Chambon, Projet," ##46-48· " l'hôpital en avançant la somme de 1200£ environ, gagnerait peut-être plus de 15000£ "

Chambon claimed that resident physicians would eliminate at least half of the "errors" in the management of illness, errors which occurred at a rate of 12 to 15 each day in a hospital with 300 patients. Such mistakes typically took place during the convalescent period, leading to a prolongation of the recovery, if not a relapse. In large hospitals, literally thousands of patient-days could be saved.

At the same time, less tangible but no less real profits accrued to the medical profession and, ultimately, to society. One could calculate the advantages for medical training. Eight large Paris hospitals would employ 21 young doctors (the Hôtel Dieu, alone, with 2500 patients would need 8 resident posts). A four-year period of hospital service meant that about five young doctors would complete their training each year and be "distributed to the kingdom." Assuming an average of twenty years additional active practice for each physician, Chambon calculated that the program would reach its peak capacity after 20 years when 100 Paris hospital-trained physicians would have been produced. Hospitals in large provincial cities would add to the ranks of this new medical corps.[39]

Chambon recognized that this program could produce but a small proportion of the physicians of the kingdom. The hospital residents would constitute an elite of the best young doctors selected by competitive examination or *concours*. Other medical students who did not have posts would enter the hospital only for clinical lessons similar to those already available in foreign centers.[40] Chambon thus saw the nation's interests best served by a two-level investment in hospitals as medical training institutions. The higher level required a more intensive investment in a student elite which in turn promised greater returns to society.

Such were the main elements of Chambon's clinical ideal. If the medical theory was largely traditional, the hospital setting introduced a new factor which subtly transformed the resulting product. One must remember that hospital lessons of any kind were an innovation for French medical education in 1789. Intensive clinical residence training did not yet exist anywhere in Europe for physicians.

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[39] *Ibid.*, # 49. Such *calculs positives*, as Chambon called his speculative numerical projections, drew upon a political economic tradition going gack to William Petty. They appeared particularly common in late 18th-century medical reform thought, perhaps as a result of the specific medical interests of such Physiocrats as Quesnay and Turgot.

[40] *Ibid.*, ##50-51.

Chambon's medical project, it should be clear by now, embodied general assumptions and values of the Enlightenment. State support for education, technological progress, and public assistance—these pervasive goals of late 18th-century reform thought found common ground in the projected hospital clinic. The clinical account book, the efficient utilization of hospital personnel, the clinical contract, and the calculation of profits articulated a unified strategy which can perhaps best be termed broadly economic. For Chambon and other physicians, the central problems of medical reform were essentially organizational and technical in a generic sense rather than specifically medical or scientific. They concerned the communication, distribution, and application of knowledge. Physicians' solutions—like those of the enlightened monarchy itself—took the form of professional bureaucratic initiatives and controls, and rational, i.e. economic, administration of institutions. The guiding spirit was managerial and bourgeois.

On the eve of the Revolution, Chambon's clinical ideal expressed widely-shared medical reform aspirations. One might trace certain themes through the ensuing decades. Pinel's medical theory, for example, was not far removed. yet the Paris clinic of the early 19th century emerged as something substantially different from the ideal of 1789. Chambon himself, badly disillusioned by the Revolution, bitterly criticized the Paris medical school, which he observed in 1814, for its "bizarre nomenclature" and other defects.[41] The clinical ideal of '89 envisioned a perfecting of 18th-century medical knowledge and institutions, a reform not a revolution. A product of a society which had begun to change drastically even before Chambon presented his project to his approving colleagues in the autumn of that fateful year, this clinical ideal would shortly become an historical specimen.

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[41] Génévrier, *Chambon*, pp. 59 ff. Chambon's voluminous and largely unpublished reactionary medical writings from the early 19th century are in the Paris Faculté de Médecine, Mss 5133-5143 To contrast the clinical ideal we have described here with that of the so-called Paris clinical school is beyond the scope of this paper. Suffice it to say that I accept, in general, Foucault's contention that a basic change in medical epistemology took place. Of central importance was the commitment to Bichat's general anatomy and pathology. Around 1800, this new interpretation of morbid phenomena fused with the hospital "gaze" of the 1790s in a kind of integration of the sense data visible at death with those observed in the living patient. This involved an irreversible change in clinical thinking for the history of which the reader is referred to Foucault, *Naissance de la clinique*, and E H. Ackerknecht, *Medicine at the Paris Hospital 1794-1848* (Baltimore. Johns Hopkins Press, 1967).

THE DEVELOPMENT OF HUGHLINGS JACKSON'S APPROACH TO DISEASES OF THE NERVOUS SYSTEM 1863-1866: UNILATERAL SEIZURES, HEMIPLEGIA AND APHASIA

SAMUEL H. GREENBLATT

Our view of important historical figures is often somewhat distorted by our use of the "retrospectoscope." Since we know what they ultimately achieved, we tend to analyse their early efforts in terms of the later outcome. For the historiography of John Hughlings Jackson (1835-1911), this natural tendency has resulted in a strong emphasis on his contributions to our understanding of epilepsy and cerebral localization. But these were not the only issues which attracted his scrutiny.

A brief survey of Jackson's bibliography for the period 1861-1870 reveals that he dealt with a seeming potpourri of subjects.[1] In this paper I will present an analysis of the basic themes in Jackson's work for the years 1863 to 1866, the period when his major theories began to take shape. More specifically, I will offer and explore three propositions: (1) Jackson deliberately worked out a methodological approach to neurological phenomena in 1863-1864, and he followed this method, consciously and conscientiously, into his major achievements of the succeeding years; (2) Jackson's pathophysiological speculations during this critical early period (1863-1864) were almost wholly derived from his analysis of clinical phenomena which shared the general characteristic of being unilateral or associated with unilateral lesions; and (3) his efforts to understand the neurological significance of aphasia in relation to other unilateral phenomena were critically important to his later success in epilepsy and localization, not simply because aphasia forced him to cerebral localization, but because it led him to consider some basic issues in the mind-body relation.[2]

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[1] See bibliographies in S. H. Greenblatt, "The major influences on the early life and work of John Hughlings Jackson," *Bull. Hist. Med.*, 1965, *39*: 374-376; and J. Taylor, ed., *Selected Writings of John Hughlings Jackson*, vol. 2 (New York: Basic Books, 1958), pp. 485-488.

[2] R. M. Young, *Mind, Brain and Adaptation in the Nineteenth Century* (Oxford: Clarendon Press, 1970), pp. 197-223, has provided a thorough analysis of Jackson's place in the development of the associationist psychophysiology and his application of its principles to cortical localization. H. T. Engelhardt, Jr., "John

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Method. Early in 1863 Jackson published for private circulation a fascinating pamphlet containing some *Suggestions for Studying Diseases of the Nervous System on Professor Owen's Vertebral Theory.*[3] This was his first systematic attempt to create a neurological methodology. Richard Owen (1804-1892), the prominent comparative anatomist, had expanded on the older "vertebral" theory of cranial development, which held that the cranium is derived from four primordial vertebrae.[4] Jackson proposed that the neuraxis and associated vascular structures could be divided into segments corresponding to each "vertebra." By this device, he hoped to provide for "the arrangement of the symptoms of disease on a natural, rather than an arbitrary system."[5]

In practical terms, Jackson's vertebral theory died at birth. None of his papers published after the *Suggestions* bespeaks any attempt to apply its principles to the detailed analysis of clinical phenomena.[6] On the other hand, the writing of the *Suggestions* was not an isolated incident in Jackson's development. With regard to methodology, the most important fact about the *Suggestions* is its existence. At the very beginning of his career as an independent thinker, Jackson tried to define his method. This attempt failed, but the next one did not.

Even as he was writing the Preface to the *Suggestions,* Jackson apparently realized that the approach was inadequate, because it was not sufficiently oriented toward the analysis of disease and disordered function: "I shall begin at once to study over again the whole subject, and in doing this, shall try to get aid from Pathology, as well as from Physiology." True to his word, Jackson soon came up with another approach. In June 1864, he delivered a

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Hughlings Jackson and the mind-body relation," *Bull Hist. Med*, 1975, *49*. 137-151, has recently analysed Jackson's psychophysical parallelism from the historical and philosophical standpoint. However, neither of these excellent reviews has looked specifically at the origin of Jackson's views on these subjects in his early clinical work on aphasia.

[3] (London. H. K. Lewis) For a more complete analysis of this fascinating but largely unavailable publication, see S H. Greenblatt, *John Hughlings Jackson. The Development of his Main Ideas to 1864*, unpublished M A. thesis, John Hopkins University, 1964, pp. 41-72; and Greenblatt, 1965, *op. cit* (n. 1 above), pp. 365-370 On internal evidence it is clear that the *Suggestions* was actually written in late 1862 (see Greenblatt, 1965, *ibid.*, p 365, ftn. 75)

[4] *Suggestions*, pp 3-5

[5] *Ibid.*, Preface

[6] Jackson did occasionally refer to the *Suggestions* in later years; see "Notes on the physiology and pathology of language," *Med Times and Gaz*, 1866, *1* 660, ftn (a), and "A physician's notes on ophthalmology," *Roy London Ophth Hosp Rep.*, 1866, *5* 273. However, these citations did not occur until some years after the publication of the *Suggestions*, and his remarks about it were in an autobiographical vein

lecture at the London Hospital "On the study of diseases of the nervous system."[7] His opening remarks are eminently quotable:

> Gentlemen,—"It is easier," Lord Bacon says, "to evolve truth from error than from confusion." If we look back, most of us would say of our mental culture that the great want, was not a want of industry nor of opportunities, but of method. And in the study of Disease it is better to have even a mechanical arrangement, than to work without a plan.[8]

Instead of the rather grandiose morphological speculations found in the *Suggestions,* he now offered a much more concise definition of his approach:

> Just as we study, as physiologists and anatomists, the vegetative life of general tissues, the structure of organs for special functions, and the universal harmony of most diverse functions in individuals, so we ought, as workers in the field of Practical Medicine, to study every case that comes before us; as presenting:—
> (1) DISEASE OF TISSUE. (Changes in Tissue.)
> (2) DAMAGE OF ORGANS.
> (3) DISORDER OF FUNCTION.[9]

This tripartite emphasis on tissue, organs and function was specifically mentioned repeatedly in the following years.[10] But the clarity of this scheme was far more important than its specifics. It allowed Jackson to gain an insight into some very basic problems by avoiding confusion at the clinical level. An example of his kind of insight is provided in some statements about epilepsy later in the lecture:

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[7] *Clinical Lectures and Reports of the London Hospital,* 1864, *1.* 146-158 (reprinted in *Brain,* 1903, *26.* 367-382; all references in this text are to the original publication of 1864).

[8] *Ibid.,* p. 146.

[9] *Ibid* , p. 147. Jackson's emphasis on tissue pathology was certainly *au courant,* because Virchow's *Cellularpathologie* had been published in 1858. Although there is no evidence that Jackson was directly familiar with this work, he certainly knew about it, because his friend Samuel Wilks lectured on the subject, in a noncommittal way, in 1862, Wilks' lecture was published in Hutchinson and Jackson's reporters' column in the *Med. Times and Gaz.,* 1862, *2* · 32-33. Jackson was very much in touch with British work in the same area. In particular, he was well acquainted with J Lockhart Clarke (of Clarke's column fame) They published papers together ("On a case of disease of the posterior columns of the cord—locomotor ataxy," *Lancet,* 1865, *1:* 617, and "On a case of muscular atrophy, with disease of the spinal cord and medulla oblongata," *Med-Chir. Tr , Lond.,* 1867, *50* · 489-496) and Jackson often referred to "Lockhart Clarke's plan" for "minute examination of nervous tissue" ("Involuntary ejaculations following fright—subsequently chorea," *Med. Times and Gaz.,* 1865, *1* · 89).

[10] *E g* , "On a case of loss of power of expression," *Brit. Med. J* , 1866, *2:* 328-329, "A lecture on cases of cerebral haemorrhage," *Clinical Lectures and Reports of the London Hospital,* 1866, *3* 239; and "A physician's notes on ophthalmology," *op. cit.* (n 6 above), p 257

> . . . the paroxysm, the disorder of functions, is often taken for the whole disease when it is only the outward part of it by which we ought to try to learn the rest. It is not at all a rare thing to hear of a patient, after his first attack of epilepsy, being treated as if the convulsions were a sign of inflammation—disordered function being taken as evidence of tissue changes, of inflammation or congestion.[11]

In the two passages quoted immediately above, Jackson is speaking of his method as it might be applied in the analysis of an individual case. But, of course, it is equally applicable to a symptom or a disease in more general terms. Jackson used his method very effectively in this broader fashion, both as a teaching device and as an investigative tool. The scientific power of this kind of analysis was well illustrated later in the same year (1864), when Jackson made a major contribution to clinical neurology by including aphasia with left hemiplegia among the basic symptoms of the syndrome of the left middle cerebral artery. He was able to make this correlation because he noticed "the concurrence of hemiplegia with loss of speech and valvular disease of the heart. . . ."[12] Since he was already thinking in regional vascular terms, as evidenced by the strong vascular emphasis in the *Suggestions,* he readily concluded that the probable cause of the correlation of aphasia with left hemiplegia was embolism of the left middle cerebral artery.

I think it is fair to say that most of Jackson's major contributions can be traced to his early use of this *comparative method* of clinical analysis. Because he had clearly defined for himself the categories of phenomena which were to be compared, he was able to extract basic pathophysiological principles from the confusing wealth of material which came under his observation at the National and London Hospitals. Jackson's interest in unilateral phenomena apparently preceded his tripartite formulation of method by a few years. But his intuitive appreciation of the potential importance of comparing unilateral seizures to hemiplegia was brought into much sharper focus by the conscious application of his method. Since hemiplegia and focal seizures are both unilateral motor phenomena, they share some important similarities as "disorders of function." It also appeared to Jackson that the underlying tissue and organ abnormalities are similar for both conditions, and he recognized that the resolution of this problem would represent a major advance in clinical medicine and basic physiology.

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[11] *Op. cit* (n. 7 above), pp 153-154.

[12] "Loss of speech: its association with valvular disease of the heart and with hemiplegia on the right side," *Clinical Lectures and Reports of the London Hospital,* 1864, *1*: 389

Unilateral Phenomena and the Corpus Striatum. Since none of Jackson's personal papers have survived,[13] we can know how his ideas developed only through an analysis of his published writings. However, his numerous articles seem to be accurate reflections of his internal intellectual turmoil, because they are seldom finished academic pieces. Rather, they bespeak a constant process of backing and filling, while moving erratically toward a deeper understanding of neurological complexities. Therefore, it is reasonable to presume that at any given time he was thinking most about those subjects which appeared most often in his publications. In these semi-quantitative terms, it is worthwhile to attempt an overview of his primary concerns for the years 1861-1866.

During 1861-62, Jackson's reporter's columns in the *Medical Times and Gazette* contained many case reports of epilepsy and "paralysis,"[14] but his first creative effort is represented by the *Suggestions* (1863). In that pamphlet he was already concerned with hemiplegia and unilateral seizures, as well as chorea. These same subjects appeared repeatedly during the rest of 1863, along with a growing emphasis on ophthalmoscopy. A similar list applies to 1864, but the sudden impact of aphasia was overwhelming. The flood of Jackson's writings abated somewhat in 1865, though hemiplegia was still much in evidence. In 1866 the outpouring began again. His studies of hemiplegia, unilateral seizures and language disorders were now leading him toward a coherent theory of brain function. The beginnings of his mature ideas about seizures and localization can be discerned in his papers of 1866, along with some important early efforts to untangle the neurological implications of the mind-body relationship. In summary, then, the predominant themes in Jackson's thoughts during the period under study were, in historical order, unilateral seizures, hemiplegia and aphasia.[15]

Temkin has provided an excellent analysis of Jackson's work on epilepsy up to and including the *Study of Convulsions* (1870),[16] and the next section of this paper will constitute a continuation of my own efforts to sort out some of the basic themes in his work on aphasia.[17] Hence, the remainder of this section will treat Jackson's interest in unilateral phenomena primarily from the point of view of hemiplegia.

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[13] They were apparently burned, in accordance with his will, see S. H Greenblatt, "The English neurologist, John Hughlings Jackson (1835-1911)," *Yearbook 1964* (Philadelphia. American Philosophical Society, 1965), pp 522-523

[14] See Appendix 2 in Greenblatt, M.A thesis, *op. cit* (n. 3 above), pp. 162-171.

[15] For references to bibliographies, see n. 1 above

[16] O Temkin, *The Falling Sickness*, 2 ed. (Baltimore. Johns Hopkins Press, 1971), pp. 328-346

[17] S H Greenblatt, "Hughlings Jackson's first encounter with the work of Paul Broca," *Bull Hist Med*, 1970, *44*· 555-570

In his analysis of hemiplegia, Jackson addressed three basic questions: (1) Exactly which functions are disordered at the clinical level? (2) Which cerebral areas ("organs") are involved? and (3) What is the pathophysiological significance of the clinical coincidence of hemiplegia, unilateral seizures and aphasia? These problems obviously overlap each other, and Jackson worked at them simultaneously. Indeed, even before he had outlined his simplified tripartite method, Jackson had posed the first two questions to himself on the first page of the *Suggestions*.

> I am aware that in, for instance, hemiplegia, I might treat the patient just as well if I knew nothing more than that one-half of the body was paralysed. If, however, I inquire more closely into the physiology of this disease, I find that it really is not paralysis of half the body. In most cases sensation is not affected at all. Every special sense escapes; all the viscera escape; and even of the motor nerves, the third, fourth, fifth, sixth, and orbicular branch of the portio dura and many others escape. I wish, then, to be able to think of it as a disease of a certain centre which commands a certain tract of the body.[18]

The last sentence in this passage is the methodological key, because the "centre" he was seeking would "command" only those functions which were actually shown to be disordered by his clinical investigations.

Midway through the *Suggestions,* Jackson elaborated on his analysis of hemiplegia in a section entitled: "Ordinary hemiplegia due to disease of the corpus striatum, or thalamus opticus, which are centres for the muscles of the limbs."[19] The corpus striatum had been considered the uppermost part of the motor tract for some years,[20] but the importance of this title lies in its last clause. Jackson pointed out, as had others before him, that the muscles of the upper face and trunk are not generally involved in "ordinary hemiplegia," and sensation often is spared altogether.

> I think we find only paralysis of the muscles of the limbs . . . Is this another way of saying that those muscles less under the control of the will escape, and hence that the corpus striatum and thalamus opticus are able to direct (voluntarily) the limbs only?[21]

Over the next few years Jackson devoted much effort to a careful clinical investigation of hemiplegia and especially to the nature of the upper facial sparing.[22] By 1865 he had seen 500 cases of hemiplegia. This experience had confirmed his previous impressions with regard to the motor sparing in the upper face and trunk and the general absence of sensory involvement.[23] On the other hand, he admitted to some confusion about the cerebral "organs" involved, "because I have not been able to make out what the difference is betwixt the hemiplegia from disease of the corpus striatum and hemiplegia from disease of the thalamus opticus."[24] In other words, Jackson had been unable to find clinically observable differences between cases of hemiplegia with proven pathological lesions in these two adjacent locations. He knew that this was not in keeping with current theories about the functions of these two structures, because William Carpenter (1813-1885) had postulated that the thalamus has a sensory function.[25]

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[18] *Op cit.* (n. 3 above), p 1 The "portio dura" is the seventh cranial (facial) nerve.

[19] *Ibid.*, p 24

[20] See Temkin, *op cit* (n 16 above), pp 313-314

[21] Suggestions, *op cit.* (n 3 above), pp 24-25 Jackson points out (p. 25) that Todd had remarked on these motor findings in hemiplegia It is well to keep in mind that we now distinguish between cortical and capsular strokes, and that sensation may or may not be affected in either Jackson did not understand this distinction, of course. Furthermore, he did not distinguish between loss of primary sensory modalities (thalamic) and cortical modalities, nor did he have the clinical tools to examine for these differences

In 1866 William Broadbent (1835-1907) published a paper entitled "An attempt to remove the Difficulties attending the application of Dr. Carpenter's Theory of the Function of the Sensori-motor Ganglia to the common form of Hemiplegia."[26] Broadbent began by summarizing Carpenter's ideas:

> This theory, as is well known, is that the thalamus is the organ of conscious sensibility, to which all impressions made on peripheral sensory nerve-fibres must be transmitted in order to be recognized as sensations, and the corpus striatum the organ or instrument of voluntary motion, the downward starting point of volitional motor impulses, or it might be said of all cerebral motor impulses. These two ganglia are again associated according to the theory of Dr. Carpetnter in sensori-motor action, impressions reaching the thalamus being passed on to the corpus striatum, and giving rise to automatic movements differing from those which have their centre in the cord, only in being accompanied by sensation.[27]

After he had outlined Carpenter's theory, Broadbent pointed out two apparent problems with it. In hemiplegia,

1. Why is not sensation more frequently and profoundly affected?
2. Why is not the entire half of the body, head and neck paralysed as to voluntary motion, instead of merely the limbs, and in partial degree the face and tongue?

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[22] See "Illustrations of diseases of the nervous system," *Clinical Lectures and Reports of the London Hospital*, 1864, *1* 337-387.

[23] "Lectures on hemiplegia," *Clinical Lectures and Reports of the London Hospital*, 1865, *2* 303-312

[24] *Ibid.*, p 301.

[25] W B Carpenter, *Principles of Human Physiology*, new American ed. (Philadelphia, 1858), pp. 490 and 508, quoted by Temkin, *op. cit.* (n 16 above), p 314.

[26] *British and Foreign Medico-Chirurgical Review*, 1866, *37*. 468-481.

[27] *Ibid.*, p. 468

> The first of these questions has recently been put forward prominently by Dr. Hughlings Jackson. . . .[28]

In answer to the first problem, Broadbent stated that there often is sensory involvement in *acute* hemiplegia; he claimed that Jackson had relied largely on an analysis of chronic cases, in which sensation tended to be more fully recovered. A few months later, Jackson acknowledged Broadbent's criticism and accepted this point.[29] With regard to the partial motor sparing in hemiplegia, Broadbent was much more in agreement with Jackson's views. However, where Jackson had postulated that the distinction between paralysed and non-paralysed muscles had to do with their relative degree of voluntary control, Broadbent had perceived another difference: "The muscles of the trunk, on the other hand (which escape paralysis), act in pairs, are almost always bilaterally combined in their action, and the two sides engaged in similar and associated movements."[30]

Jackson had defined the functional difference between axial and limb musculature in essentially psychological terms, but Broadbent had appreciated a biological difference.[31] Hence, Broadbent was able to offer a physiological solution to the clinical problem of axial muscle sparing in hemiplegia:

> The hypothesis suggested by these considerations is, *That where the muscles of the corresponding parts on opposite sides of the body constantly act in concert, and act independently, either not at all, or with difficulty, the nerve-nuclei of these muscles are so connected by commissural fibres as to be* pro tanto *a single nucleus. This combined nucleus will have a set of fibres from each corpus striatum, and will usually be called into action by both, but it will be capable of being excited by either singly, more or less completely according as the commissural connection between the two halves is more or less perfect.*[32] [italics in original]

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[28] *Ibid.*, p 469. This is one of the earliest published citations of Jackson's work by another medical author There is another reference to Jackson's work in the article which immediately follows this one; see W Moxon, "On the connexion between loss of speech and paralysis of the right side," *ibid.*, p 481 Moxon was "Pathologist and Curator of Museum to Guy's Hospital " He began his article with a reference to the observations of Broca, "which have been brought before the profession in England by Dr H. Jackson, in a comprehensive and able record of cases " This apparently refers to Jackson's article on "Loss of speech," *op. cit.* (n. 12 above) Clearly, Jackson was beginning to make his mark in the London medical community.

[29] "Note on the functions of the optic thalamus," *Clinical Lectures and Reports of the London Hospital*, 1866, *3:* 374-375

[30] *Op. cit* (n 26 above), p. 476

[31] It would appear that Jackson's admixture of psychological and physiological concepts was violating his announced adherence to psycho-physical parallelism; but, in fact, he had not yet reached that stage of clarity in his thinking about such matters The final section of this paper will explore some of the early elements in Jackson's personal resolution of the mind-body quandry, though he was still many years away from that resolution in 1866.

[32] *Op cit* (n 26 above), p. 477.

Exactly where Broadbent's "combined nucleus" might be located is not stated, but the idea of *bilateral innervation* of axial musculature is clearly enunciated.[33] Jackson saw the importance of this principle immediately and began to apply it to his analyses of epilepsy and chorea, as well as hemiplegia.[34] He referred to "Broadbent's hypothesis" repeatedly throughout the rest of his life's writings, and it became an integral part of his theory of cerebral localization.[35]

In addition to providing an explanation for the axial muscle sparing in hemiplegia, Broadbent's paper solved Jackson's problem with regard to sensory function and the thalamus. It also had the effect of allowing Jackson to think more precisely about the function of the corpus striatum. In a lecture published at the same time as his first acknowledgment of Broadbent, Jackson discussed his ideas about the concept of a motor "centre" in the corpus striatum.

> It will be observed that disease of the corpus striatum produces partial paralysis of the arm, and not paralysis of part of the arm. The whole of the arm is, I believe, represented in every part of the corpus striatum, and so, if the damage to this region be great, there is complete paralysis of the limb, and if slight, the whole of it is weakened. . . . This is important as regards the views we should hold as to what we call "centres". The centre for the arm is, I hold, the whole of the nervous system of the arm (from the corpus striatum to the termination of the nerves in muscles, and their origin in skin and other parts) and I think we may find by a consideration of the whole of its nervous system, a gradual ascent from grouping of muscles for simple movements like flexion or extension, to completer and more complex actions.[36]

This passage was published in late 1866. As of that time, it serves to re-emphasize Jackson's continuing belief in the central importance of the corpus striatum for the understanding of hemiplegia. With regard to both hemiplegia and unilateral seizures, he had received this opinion from his immediate predecessors, and it still remained with him.[37] On the other hand, hemiplegia and epilepsy both may be associated with disorders of speech, and the lesions which correlated with aphasia did not seem to involve the corpus striatum directly. There was no pre-existing tradition which claimed that they should. In 1864 Jackson had sought to understand the clinical coincidence of aphasia and hemiplegia by postulating that obstruction of the left middle cerebral artery caused simultaneous damage to the corpus striatum and to the cortical area containing the "faculty of speech" in the frontal lobe.[38] In that view, the clinical correlation of these two conditions was quite accidental. Physiologically, the corpus striatum and the "convolutions" near it were part of the same vascular distribution, but they were not directly connected together by any other biologically definable mechanism.

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[33] Broadbent's statement is vague and confusing in modern terms, because he does not distinguish between the possibility of simple ipsilateral (uncrossed) innervation *versus* double decussation. In fact, he could not have been thinking about either, because he did not have the neuron theory at his disposal. His "commissural fibres" had no cell bodies and therefore could be intercollated at any convenient level in the neuraxis

[34] See "Note on the functions of the optic thalamus," *op. cit* (n 29 above), p. 375, ftn , and "A physician's notes on ophthalmology," *op cit* (n. 6 above), p 270, ftn

[35] See *Selected Writings*, vol 1, *op. cit.* (n 1 above), *passim.*

[36] "A lecture on cases of cerebral haemorrhage," *op cit* (n. 10 above) pp 238-289, ftn

[37] See Temkin, *op cit.* (n 16 above), pp. 311-316, 328-335.

In September 1865, Jackson published the autopsy results on a woman who had had a dense right hemiplegia and a severe non-fluent aphasia. Her lesion involved the left corpus striatum, thalamus and adjacent hemisphere, although the frontal lobe was grossly intact. His remarks on this case are intriguing:

> The disease . . . was far too extensive to help us to determine anything precisely as to the seat of speech. The case has, however, this value, that it shows that what may appear at first glances to be a mere defect of articulation is often due to disease of parts far above the ninth nerve or its nucleus. *The probability is, that the convolutions near the corpus striatum have to do with guiding muscles in articulation.*[39] [my italics]

This is a transitional statement. Until this point, Jackson had followed the tradition which held that only mental functions are located in the cortex, including the "faculty of speech." But here he was raising the possibility that a strictly motor function might be represented there, at least for the motor aspects of "articulation."

With the benefit of a century's hindsight, we can now appreciate that this was an important opening wedge. If any motor processes are located in the cortex, then the barrier to all such non-mental entities has been breached. Depending on one's view of the mind-body relation, it might even be claimed that any assignation of *ordinary* physiological processes to the hemispheres is logically incompatible with a continued belief in their *special* mental functions. But Jackson did not see it that way until many years later. For the time being, he was content to carry along his newer speculations in juxtaposition with the older, established view of the hemispheres as the seat of the mind. In June 1866, he published some important "Notes on the physiology and pathology of language."[40] His simultaneous adherence to both the old and the new is nicely illustrated by the following passages from two separate paragraphs on the same page:

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[38] See n. 12 above.

[39] "Hemiplegia on the right side, with defect of speech—death—autopsy," *Med Times and Gaz*, 1865, *2* 284

> Although the difference in the execution of voluntary and involuntary movements is very striking in some cases of loss of speech, the degree of loss of power to utter words must not be taken as a certain index of this difference. The difference probably depends on the difference in quantity of brain damaged near to (as well as in the exact relation of the part damaged to) the highest part of the motor tract, the point of emission of the orders of the "will" to muscles and to centres for muscular groups. . . .
>
> . . . Again, there is no more difficulty in supposing that there are convolutions near the corpus striatum for superintending those delicate movements of the hands which are under the immediate control of the mind, than that there is one, as Broca suggests, for movements of the tongue in purely mental operations. The phenomena of chorea, I believe, point to this conclusion.[41]

In the first passage, Jackson was still faithful to the idea that the "will" connects to the motor system at some anatomical point—"the highest part of the motor tract." In earlier (unquoted) parts of the same paper, it is clear that this phrase referred to the corpus striatum. By contrast, in the second passage he was postulating the existence of more mundane motor processes in the "convolutions." Since there were no widely accepted contemporary views on the pathophysiology of chorea, he apparently felt free to draw an analogy to aphasia and to put the lesion of chorea in a similar location.[42] Thus, by the end of 1866, Jackson's comparative method had led him to postulate that aphasia and chorea both might be manifestations of abnormalities in the same cerebral "organ," the cortex. But he still felt that the basic lesion of hemiplegia and unilateral seizures was in the corpus striatum.

Jackson's self-imposed method also demanded that he investigate clinical phenomena at the tissue level, and he did not ignore this problem. In particular, he appreciated the *temporary* nature of many of the conditions which he studied. It is easy enough to understand how permanent hemiplegia or aphasia results from destruction of relevant cerebral areas. But what is going on in brain tissue when a patient's arm suddenly starts to shake and then stops almost as abruptly? The same question applies to the clinical manifestations of post-ictal paralysis, temporary speech disorders associated with seizures, and chorea.

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[40] *Op. cit.* (n. 6 above), pp 659-662.

[41] *Ibid*, p 660 It is of interest to note that the location(s) of the lesion(s) responsible for hemichorea is still quite unsettled, though it does not appear that there is a major cortical component; see I S. Cooper, *Involuntary Movement Disorders* (New York: Hoeber, 1969), pp 293-315. Jackson did not usually distinguish between chorea and hemichorea, though it appears that he was often referring to the latter when he mentioned simply "chorea."

[42] On the sensory side, Jackson was also beginning to think about the possibility of direct neural representation in the cerebral cortex, at least for the "special senses", see "A physician's notes on ophthalmology," *op. cit.* (n 6 above), p 275

Post-ictal hemiplegia (Todd's paralysis) had been classically described by Robert Todd (1809-1860) in 1854 and attributed to exhaustion of the tissues.[43] In the same year, Charles B. Radcliffe (1822-1889) hypothesized that the hyperactivity of seizures also indicated a subnormal state of tissue.[44] Jackson recognized the physiological analogy between Todd's paralysis and ictal and post-ictal aphasia, which he called "epileptic aphemia." In 1864 he applied Todd's and Radcliffe's reasoning to all of these phenomena, invoking the common element of spasm or other vascular compromise in the region of the middle cerebral artery.[45] But Radcliffe's theory was incomplete. While it might be true enough to say that epileptogenic tissue is really partially damaged, rather than destroyed, one must still account for the fact that it is hyperfunctional for brief periods.

Jackson took up this challenge in 1866. Apparently he began to realize that an explanation based on vascular considerations was not adequate.

> Dr. Jackson believes, adopting the views of his colleague, Dr. Radcliffe, that nervous tissue is below and not above par in the paroxysm. How it happens that the arteries begin suddenly to contract, if they do, or why that state of tension suddenly begins to run down, it is not possible to say. It is possible that there are waves of periodicity in the system, in which certain enfeebled regions do not, as it were, come to time.[46]

Radcliffe had used the term "periodicity" to designate what is now meant by "biological rhythms."[47] But these phenomena were only vaguely understood at the time, and Jackson really had something else in mind. Toward the end of 1866, he managed to express what he was thinking more directly.

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[43] *Clinical Lectures on Paralysis, Diseases of the Brain, and Other Affections of the Nervous System* (London John Churchill, 1854), pp. 294-295. An historical review and dissenting critique of this doctrine has been given by R. Efron, "Post-epileptic paralysis. theoretical critique and report of a case," *Brain*, 1961, *84:* 381-394.

[44] *Epilepsy and Other Affections of the Nervous System* (London John Churchill, 1854), pp 60-61.

[45] *Op. cit.* (n. 12 above), pp. 465-467

[46] "Clinical remarks on cases of temporary loss of speech and of power of expression (epileptic aphemia? aphrasia? aphasia?), and on epilepsies," *Med. Times and Gaz.*, 1866, *1*: 443.

[47] *Op. cit* (n. 44 above), pp. 109-120.

> It may be, that the enfeebled hemisphere, or scattered parts of it, acquires a certain amount of tension from the nutritive changes of which it is capable, and that the tension is discharged through the *healthy* corpus striatum. Such speculations have, I think, this value, that they may guide our microscopical inquiries to the exact position and nature of changes which occur in connection with disease of the hemisphere when unilateral epileptiform seizures . . . or unilateral paralysis . . . have been the symptoms during life.[48] [italics in original]

The idea that seizures might be related to abnormal discharge "through the *healthy* corpus striatum" implies that the discharging lesion is located above that structure. The fact that Jackson italicized the word *"healthy"* may indicate that he had some inkling of this implication, but it was not brought out at the time.

The search for the "discharging" lesion of epilepsy at the microscopic level is one of the most fascinating and elusive pursuits of modern neurophysiology. It was first bequeathed to us by Hughlings Jackson in the characteristically speculative words of the above quotation. Since the publication of the *Suggestions* in 1863, Jackson had been struggling to create an integrated neural pathophysiology. Though he was still some years away from his mature formulations, by 1866 he seemed to know instinctively that if he could understand the functional relationship of unilateral seizures, hemiplegia, and aphasia, he would find they were all comprehensible within the same pathophysiological model. But two rather large stumbling blocks remained. The first, in Jackson's personal chronology, was the mind-body relationship; the second was the contemporary ignorance of normal physiological processes at any level above the brainstem. As it turned out, his approach to the mind-body problem also led him to a potent source of ideas about hemisphereic function.

Aphasia and the Mind-Body Relation. Jackson had been intrigued by aphasia in the late 1850s, many years before he began to appreciate its significance.[49] What particularly caught his attention was "the strangeness of this association" of "the loss of a purely mental faculty" with a decidedly physical deficit such as hemiplegia.[50] In this vague way, the mind-body problem intruded itself into Jackson's thoughts about aphasia from the beginning. But these musings lay dormant for several years. Some recent authors have assumed that Jackson also applied the evolutionary-associationist philosophy of Herbert Spencer (1820-1903) to this problem *from the beginning*.[51] However, such statements cannot be substantiated in any of the biographical memoirs written by associates who knew Jackson during his lifetime.[52] Among these biographers, only Jonathan Hutchinson (1828-1913) knew Jackson in his youth, and Hutchinson states that Jackson's early literary hero was Thomas Carlyle (1795-1881).[53]

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[48] "A physician's notes on ophthalmology," *op. cit.* (n. 6 above), pp 263-264, ftn By the time he published this paper in the latter part of 1866, Jackson was consciously aware of the methodological importance of studying unilateral phenomena. He stressed the potential significance of "the slightest one-sided symptom," especially if it were the "first symptom," as a guide to "those minute abnormal changes" which might lead to "where the epileptic process begins in the brain " (See pp 252, 282-283). He had used the term "aura" in another ophthalmological paper earlier in the same year, but without the same functional implication ("Observations on defects of sight in diseases of the nervous system," *Roy London Ophth. Hosp Rep* , 1866, *5*. 75) Since these statements about unilateral symptoms are the first to be found in Jackson's writings, I would infer that his attention to unilateral phenomena was not categorically deliberate until around this time.

[49] See Greenblatt, 1965, *op. cit* (n. 1 above), pp 370-371

Jackson made no references to Carlyle in any of his early writings, and it is highly unlikely that Carlyle's purely literary influence would have had any effect on Jackson's perception of the mind-body relation. In fact, Jackson's earliest contributions to aphasiology were made entirely in a clinical context, without the intrusion of anyone's speculative philosophy. By April of 1864, Jackson had concluded that aphasia is generally associated with right hemiplegia and therefore with left brain damage. His only speculations about these findings related to his postulation of an obstruction in the left middle cerebral artery to account for the clinical coincidence of hemiplegia and aphasia.[54] By May 1864, he had become aware of Broca's earlier observations of the same clinical facts, though Broca had not brought up the idea of arterial occlusion.[55] The subject of aphasia apparently occupied

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[50] "Loss of speech," *op. cit.* (n. 12 above), p. 402.

[51] *E.g.*, M. Critchley, "Hughlings Jackson, the man; and the early days of the National Hospital," *Proc. Roy. Soc. Med.*, 1960, *53*: 614, and Young, *op. cit.* (n. 2 above), p. 197.

[52] See biographical notes by J. Taylor, J. Hutchinson, and C. Mercier in J. Hughlings Jackson, *Neurological Fragments* (London: Oxford Univ. Press, 1925), pp. 1-46, and also H. Head, "Hughlings Jackson on aphasia and kindred disorders of speech," *Brain*, 1915, *38*: 1-27; E. F. Buzzard, "Hughlings Jackson and his influence on neurology," *Lancet*, 1934, *2*: 909-913, W. Harris, "John Hughlings Jackson, 1835-1911," *Post-Graduate Med. J.*, 1935, n.s. *11*: 131-134. Although most of these memoirs mention Spencer's early and pervasive influence on Jackson, none of them states explicitly that Jackson had already absorbed Spencer's ideas before he arrived in London in 1859. Jackson began to publish in 1861, and my own research has shown that there is no mention of Spencer in Jackson's published writings prior to late 1864 (see Greenblatt, 1965, *op. cit.* [n. 1 above], pp. 373-374). In the absence of Jackson's personal papers (see n. 13 above), there is thus no verifiable evidence that Jackson had ever read any of Spencer's writings until 1864.

[53] Hutchinson (*op. cit.*, p. 38) says of Jackson, "In early life he had been much influenced by Carlyle's writings, but subsequently he found those of Emerson more to his taste." Even here, it is only a presumption that "early life" refers to Jackson's pre-London period.

[54] See Greenblatt, 1970, *op. cit.* (n. 17 above), p. 558.

[55] *Ibid.*, pp. 561-564.

Jackson's thoughts quite intensively during the rest of the year, because he published a major article on "Loss of Speech" toward the end of 1864.[56]

Jackson's first references to Spencer are found in this rather lengthy review, but they are concerned only with the psychological distinction between intellectual and emotional speech.[57] Moreover, the passages which Jackson quoted from Spencer are taken from one of Spencer's short essays, not from his *Principles of Psychology,* which had been published in 1855.[58] The only psychological treatise cited by Jackson in late 1864 was an obscure work with the intriguing title of *Physiological Psychology* by an equally obscure author, Robert Dunn (1799-1877).[59] Jackson also referred to a few other long-forgotten pieces on similar subjects, but not in any prominent way.[60] It is apparent that he was beginning to appreciate the potential importance of the scientific study of mental phenomena, because he made a remark about "the special language of Psychology,"[61] but it is impossible to know whether this was really a response to his reading of Spencer or of Dunn.

Early in 1865 Jackson discussed the case of a young boy with "chorea" who often uttered the word "bloody" involuntarily. At other times the patient suddenly and uncontrollably blew out his candle while reading.

> In this instance quasi-mental acts were performed without the intervention of the patient's will, even when he was apparently quite conscious. They look like reflex actions, though what the exciting cause is it would be difficult to say. . . .Now, it is in some classes of cases of disease of the nervous system hard to say where obviously motor symptoms end and where the purely mental ones begin. . . .We may, perhaps, find all shades, degrees, and analogues betwixt obvious and coarse motor reflex actions, and disorder of what Dr. Laycock has described as the reflex function of the brain.[62]

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[56] See n. 12 above

[57] *Ibid.*, pp. 440, 448. See also Greenblatt, 1965, *op cit* (n 1 above), pp. 373-374

[58] H. Spencer, *The Principles of Psychology* (London: Longman, *et al.*, 1855; republished in 1970 by Gregg International Publishers, Farnborough, England).

[59] Jackson, *op. cit* (n. 12 above), p. 404, refers to "Mr Dunn's work, *Physiological Psychology*, 1858 " I have been unable to locate a copy of this work It apparently made a significant impression on Jackson, because he referred to Dunn at least once again in 1866. See "Notes on the physiology and pathology of language," *op. cit.* (n. 6 above), p. 660.

[60] *Op. cit.* (n. 12 above), pp 399, 459.

[61] *Ibid.*, p. 395.

[62] "Involuntary ejaculations following fright—subsequently chorea," *Med. Times and Gaz.*, 1865, *1*. 89. In this title, "ejaculations" refers to the patient's sudden production of sounds. The description of the case is reminiscent of what is now known as "Gilles de la Tourette's syndrome" or "tic convulsif," which is probably a variant of dystonia rather than a form of chorea. See Cooper, *op cit.* (n 41 above), pp. 274-279, for description and historical references.

There are two important observations to be made about this passage. In the third sentence, Jackson brought out a fundamental clinical impression; *i.e.*, experience was teaching him that the traditional distinction between purely mental and purely physical abnormalities did not always bear close scrutiny. And in the final sentence, his reference to the reflexology of Thomas Laycock (1812-1876) indicated the resurgence of an earlier influence on his own development.

Laycock had been one of the most impressive professors at the York Medical School, which Jackson attended from 1852 to 1855. In all probability, he was responsible for Jackson's very early interest in the nervous system.[63] Laycock was a pre-Spencerian associationist, and it seems reasonable to assume that he made Jackson one also. But in the early 1860s Jackson's clinical introduction to aphasia was received from a French tradition, which depended on the somewhat antithetical "faculty" psychology. When Jackson first made acquaintance with Broca's work in 1864, he seemed to accept the faculty psychology on which it was based. By 1866, however, he had rejected this view and turned back toward his own native associationism.[64] We are thus left with the question of what happened between 1864 and 1866.

Jackson's published writings in 1865 were not very voluminous, and they contained no fresh thoughts about aphasia. There was no mention of Spencer or any other associationist author. But in 1866 the deluge began. Jackson's first paper of that year contained many references to Spencer, and there were also extensive quotations from the philologist Max Müller (1823-1900).[65] From that time forward Jackson's papers were laced with the frequent Spencerian quotations that became one of the most salient hallmarks of his literary style. In addition to Spencer, he also referred to John Stuart Mill, Alexander Bain, and a number of lesser lights in the associationist tradition.[66]

An important biographical fact must be interjected here. Jackson was married to his cousin, Elizabeth Dade Jackson, in 1865.[67] It was a happy marriage, though tragically short and without issue. The assumption of marital responsibilities may well account for the transient decrease in Jackson's literary output. At the same time, the advent of domestic quietude may have fostered a contemplative mood. Whatever the reason, it seems an inescapable conclusion that Jackson dug voraciously into the writings of the British associationist philosophers during the period encompassed roughly by the calendar year 1865. What emerged from this time of intense study was the beginnings of a different view of the mind-body relation.

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[63] See Greenblatt, 1965, *op. cit* (n. 1 above), pp 348-353

[64] See Greenblatt, 1970, *op. cit* (n. 17 above), pp. 566-568; also, Young, *op cit* (n. 2 above), pp. 206-207.

[65] "Clinical remarks on emotional and intellectual language in some cases of disease of the nervous system," *Lancet*, 1866, *1* 175. Jackson refers to Müller's term, "Pooh-Pooh Theory," which is found in M. Muller, *Lectures on the Science of Language*, 4 ed. (London Longman, *et al.*, 1864), pp. 371 *et seq.*

[66] The earliest statement which shows unequivocally that Jackson had actually read some major works of the associationist authors is found in a paper published on April 28, 1866; *op cit* (n. 46 above), p. 442 "The books to read to make us feel the importance of single cases of loss of speech as flaws of mind were those of Max Müller, Herbert Spencer, J. S. Mill, etc "

In its most basic form, the tradition which Jackson had to overcome had taught him that the cerebral hemispheres above the basal ganglia were the *terra incognita* of the mind. Hence his constant references to the corpus striatum as "the point of emission of the orders of the 'will' "[68] to the bodily mechanisms. Hence also his early juxtaposition of psychological terms to physiological concepts without any apparent thought to the problem of how these two levels of phenomena might actually relate to each other. The faculty psychology was compatible with the older view of the hemispheres because each "faculty" was simply a mental entity—a part of the "mind." But the associationist view carried with it the inherent possibility of extending basic neurophysiological principles to the functional analysis of the hemispheres. This step had been taken in a purely speculative manner by Laycock, who had proposed that the entire neuraxis functions on the reflex principle.[69] The implication of this idea is that the hemispheres are subject to the same sensory-motor reflex paradigm as the lower centers. A full decade after Jackson's contact with Laycock, Jackson's clinical analyses of unilateral phenomena had led him back to this fundamental concept. In the course of Jackson's development, Laycock had paved the way for Spencer.

Jackson's ideas about the mind-body relation did not come to fruition in his own personal concepts of psycho-physical parallelism until many years after 1866.[70] But his change of course in favor of associationism is seen in his "Notes on the Physiology and Pathology of Language," which were published in June 1866. In the earlier part of that paper he stated his position vis-à-vis Broca.

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[67] See J Taylor, "Biographical memoir," in Jackson's *Neurological Fragments*, *op. cit.* (n 52 above), pp. 14-15.

[68] *Cf.* quotation above marked by n. 41 above.

[69] See M. P Amacher, "Thomas Laycock, I M. Sechenov, and the reflex arc concept," *Bull. Hist. Med*, 1964, *38*· 168-183.

[70] See Engelhardt, *loc. cit.* (n. 2 above)

> I must here say that I believe less in some of the views propounded by Broca than I did, although I think the scientific world is under vast obligation to him for giving precision to an important inquiry . . . . Indeed, I think the evidence I have collected goes to confirm many of his statements as to facts, but I have for some time ceased to go with him in a few of his inferences. . . .I think, then, that the so-called "faculty" of language has no existence, and that disease near the corpus striatum produces defect of expression (by words, writing, signs, etc.) to a great extent, because this is the way out from the hemisphere to organs which the will can set in motion. Hence, as I believe, disease of the convolutions near the corpus striatum is the cause of chorea, which, as regards the limbs, is not so much a disorder of mere motion as disorder of those movements which are voluntary and educated, or at least co-ordinated.[71]

In this passage, Jackson's break with the faculty psychology was not complete. He claimed that the " 'faculty' of language has no existence," but he really had nothing coherent with which to replace it. So he fell back on the "will."

Further along in the same paper, Jackson again tried to define his differences with Broca, this time by comparing direct quotations from Broca and Spencer. Broca had understood aphasia as a loss of "*the memory of the means of coordination which one employs to articulate words,*" whereas, in Jackson's words, Spencer had understood memory as "the obtruding of some of the motor impulses on the consciousness."[72] But "consciousness" was little more than a substitute for "will." Despite Jackson's efforts to harmonize the views of Broca and Spencer, their marriage was incompatible, and Jackson was caught in the middle. His disclaimers of the faculties notwithstanding, he was not quite able to think of memory as anything else.

This situation changed by the end of 1866, when Jackson had absorbed the concepts and terminology of associationism more completely. He was then able to offer a coherent sensory-motor view of hemispheric function.

> Defects of speech occur from damage near the corpus striatum, because here the groupings of motor or motor-impulse nerves narrow towards a connection with the immediate instruments of motion. This knowledge of location of damage which disorders the most elaborate form of expression by motion . . . will urge us to ask where the complex impressions from several sense systems . . . embrace the cerebral systems of motions and motor-impulses. It is easy to say this point is the thalamus

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[71] *Op cit.* (n. 6 above), p. 660.

[72] *Ibid*, p. 661. The quotation from Broca is my translation of the original French, which Jackson quoted directly from Broca's "pamphlet he was good enough to send me a year or two ago. (Extrait des *Bulletins de la Société Anatomique*, juillet, 1863.)"

> opticus. Indeed, it may be that here the various threads begin their simplest interweavings, as the groupings of fibres for the simplest weavings of muscle in higher mental operations seem to be near the corpus striatum. But I should think the highest groupings of sensory nerves are continuous, deep in the cerebrum with the highest groupings of motion. . . .It is obvious that motion and sensation are everywhere interweaved. . . .In the higher evolutions of motion and sensation, no doubt, the two phases of nerve action are so intermingled that incoherence (whatever the most striking phenomenon may be) is a defect in the highest evolutions of both motion and sensation, of their widest interrelations and associations.[73]

In the broad scheme of things, Young has shown that Jackson's basic contribution to the history of cerebral localization was his extension of the "sensory-motor psychophysiology to the cerebral cortices."[74] But on the other hand, "If Jackson's statements on the convolutions and the corpus striatum up to 1870 are brought together and compared, the simple truth is that they defy integration into a consistent, unified view."[75] From the perspective of the history of cerebral physiology, this is an undeniable conclusion. Within the context of Jackson's own personal development, however, it is a little beside the point.

Hughlings Jackson was, first and foremost, a clinician by deliberate intent. The clinic, the bedside, and the autopsy room were his only laboratories. He became a theoretical neurophysiologist almost by default, because he found that the science of his day was inadequate to explain clinical phenomena. Especially during the earlier part of his career, his physiological ideas were always derived from and answerable to his clinical observations. Up to 1866, his method of clinical analysis and comparison had not led him to conclude that all unilateral phenomena were comprehensible within the same physiological framework. At this point in his development, his physiological speculations could not go any further than his clinical observations would allow.

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[73] "A physician's notes on ophthalmology," *op. cit* (n. 6 above), p. 278, ftn.

[74] *Op. cit.* (n. 2 above), pp. 204-210.

[75] *Ibid* , p 220.

VETERINARY MEDICINE AND RURAL HEALTH CARE IN PRE-REVOLUTIONARY FRANCE*

CAROLINE C. HANNAWAY**

The eighteenth century was a formative period in the history of veterinary medicine. For the first time veterinary schools were opened in a number of European countries offering an organized training in subjects relating to animal care and disease.[1] France was a leader in these innovations, founding veterinary schools at Lyon in 1762 and at Alfort, outside Paris, in 1766.[2] The institution of these schools resulted in the appearance of a sizeable new group of medical professionals and, by 1795, it was estimated that over 1000 students had passed through the schools.[3] Important as these events are for veterinary history they also afford some interesting insights in the broader context of the development of medical science and the provision of medical care, human as well as animal, at the end of the Ancien Régime. In this paper I propose to examine three aspects of this development: the expansion of veterinary medicine from a primary preoccupation with the horse to include domesticated and farm animals; the link that was seen between epizootic and epidemic diseases and the effort to advance veterinary science in the context

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[1] This paper concentrates on the developments in veterinary medicine in France. Similar developments took place in other European countries often as a result of the French innovations and a comparative study would be revealing of differences and similarities in plans and concepts. I thank Dr. George Rosen for bringing information about veterinary medicine in Germany and Poland to my attention. See for example Hans Jenzer, *Dr med Johann Friedrich von Herrenschwand Ein Berner Arzt im 18 Jahrhundert* (Bern: Francke Verlag, 1967), pp 64-82, 104-9.

[2] Once Alfort was established it soon outstripped Lyon and became the focus of attention in veterinary matters but the School at Lyon continued to graduate students and well over 400 are estimated to have attended it by 1793. See Henri Hours, *La lutte contre les épizooties et l'école vétérinaire de Lyon au XVIIIe siècle* (Paris. Presses Universitaires de France, 1957), pp. 25-7. The major history of Alfort is A. Railliet and L. Moulé, *Histoire de l'Ecole d'Alfort* (Paris: Asselin et Houzeau, 1908).

[3] *Instructions et observations sur les maladies des animaux domestiques, avec les moyens de les guérir, de les préserver, de les conserver en santé, de les multiplier, de les élever avec avantage et de n'être point trompé dans leur achat*, 6 vol (Paris, 1782-1808), vol. V, p. 7

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of a unified medical science of humans and animals; and finally the efforts to employ the trained corps of veterinarians in an overall rural health care plan.

The focus of attention in veterinary medicine in the earlier eighteenth century was the maintenance and ailments of the horse. The horse was the one domestic animal whose worth had marked it out for specialized and systematic care before the eighteenth century. It was the most valued animal, having defined the status of the nobility since feudal times, and was used for purposes of war, recreation, and transportation by the upper classes. The breeding and training of horses were subjects of both noble and government attention in France; horse regiments were important parts of the army; and schools of horsemanship and the finer points of riding technique flourished. The upkeep of horses had given rise to special organized groups, the *maréchaux* or farriers, who in France during the eighteenth century constituted distinct guilds controlling the practice of veterinary medicine. The art of farriery was learnt by a system of apprenticeship to an established master.[4]

Claude Bourgelat, the founder and first director and inspector-general of the French veterinary schools, was very familiar with the world of horses and their care.[5] In July 1740, at the age of 28, he obtained a position as equerry to the King in charge of a school of horsemanship and riding in Lyon and his activities in connection with this school earned him a reputation. In 1744 he published a treatise about cavalry and in the 1750s his book, the *Elémens d'hippiatrique*, expressed his belief that it was only by creating schools that knowledge of the care of horses and the principles of their medicine would be properly taught. He worked to educate himself further in these matters, learning the principles of anatomical dissection under the guidance of two members of the College of Surgery of Lyon.[6] Bourgelat's knowledge of horses led to his being appointed commissioner-inspector of the royal stud-farm in the Lyon area and later, in 1764, in addition to his veterinary school posts, he became commissioner-general of the royal horsebreeding establishments of France.[7]

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[4] For the earlier history of veterinary medicine see E. Leclainche, *Histoire de la médecine vétérinaire* (Toulouse: Office du Livre, 1936), Clément Bressou, *Histoire de la médecine vétérinaire* (Paris. Presses Universitaires de France, Que sais-je?, 1970); J. F. Smithcors, *Evolution of the Veterinary Art* (Kansas City, Mo · Veterinary Medicine Publishing Co., 1957).

[5] For details of Bourgelat's biography, career, and writings see Marc Mammerickx, *Claude Bourgelat Avocat des vétérinaires* (Brussels· For the author, 1971) See also Railliet and Moulé, *Ecole d'Alfort*, pp. 7-28.

[6] Mammerickx, *Bourgelat*, pp. 161, 166.

[7] *Ibid.*, p. 57. For information on horse breeding and the administration of stud farms see René Musset, "L'administration des haras et l'élevage du cheval en France au XVIII^{e} siècle (1715-1790)," *Revue d'Histoire Moderne et Contemporaine*, 1909-10, *13* 36-57, 133-52

This background, the continuing importance of the horse, and the role he envisaged for veterinarians led Bourgelat to emphasize the horse as the major subject of attention in the curriculum of the veterinary schools, although the care and ailments of other domestic animals were included as subjects of study. An early foreign student at the Lyon Veterinary School, Peter Abilgaard, recorded this bias:

> But great was our astonishment on entering the School to see that not only was the theory of veterinary medicine being taught there but also that the science of horse care and farriery was being thoroughly studied. The horse, being the most precious of all domestic animals and the most subject to man's activities . . . was the principal object of the lessons of the professor . . . . The cadavers of horses served as subjects for the anatomy lessons.[8]

An examination of the subjects taught confirms the priority given to the horse. In addition to courses on anatomy and the diseases of the horse, the students were taught to appreciate its external characteristics, the relationship of its parts to each other, and had to learn all aspects of the welfare of the horse.[9] Students had to become proficient at horse-shoeing and the art of blacksmithing.[10] Horses formed the majority of sick animals examined and treated in the hospitals maintained by the schools and trips to the horse market to learn what constituted a good purchase were part of the program of studies.[11]

In many respects then, Bourgelat's conception of the veterinary school curriculum represented a formalization and codification of the apprenticeship training of those aspiring to become members of the existing guilds for specialists in the care of horses. As he himself states, he felt that the sons of *maréchaux* were the most suitable recruits for the school and he anticipated that his qualified students would return to their fathers' shops and take over their craft.[12] The provision of a more knowledgeable and systematically trained group of farriers for France was one of the anticipated functions of the veterinary schools. In fact not only veterinarians for rural areas were to be trained at the schools, but also farriers for the various horse regiments of the army. Beginning in 1769, military pupils were accepted at Alfort destined to become *maréchaux-experts* for their regiments. Although the number of these diminished over the years until the demand again increased in the Revolutionary Wars, they remained as a separate category of pupil chiefly interested in horses.[13]

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[8] *Instructions et observations*, vol. V, pp 9-10 Peter Abilgaard was a Danish physician sent by his government to study veterinary medicine largely in the hope of obtaining new information about epizootic diseases amongst cattle Consequently he was disappointed by the training program offered by Bourgelat. There was continuing foreign enrollment at the veterinary schools, often by students on similar missions to that of Abilgaard, and many of these students were instrumental in the founding of veterinary schools in other parts of Europe before the end of the century. It is notable that many of them had a medical background unlike Bourgelat's recruits.

[9] [Claude Bourgelat], *Reglemens pour les écoles vétérinaires de France* (Paris, 1777), pp. 122-133, 147-151 At the Paris school, in addition to the regular students, gentlemen who sought to become inspectors of the stud farms could also take these courses about the horse [Bourgelat], *Règlemens*, p. 153, Railliet and Moulé, *Ecole d'Alfort*, p. 325

[10] [Bourgelat], *Règlemens*, pp 220-234, Railliet and Moulé, *Ecole d'Alfort*, pp 341-3

[11] [Bourgelat], *Règlemens*, pp 185-209. See also *Instructions et observations*, vol. II, pp 35-8, vol VI, pp. 42-3; Railliet and Moulé, *Ecole d'Alfort*, pp. 324-5

This concern with the horse represents the traditional aspect of the French veterinary schools but Alfort and Lyon were more than schools of *maréchalerie*. The royal support that Bourgelat obtained in establishing his schools was given largely in the hope of utilizing veterinarians as a trained corps of assistants in the outbreaks of epizootic disease, especially amongst cattle and draught animals, which repeatedly afflicted rural areas of France.[14] Previously there had been no such continuing source of help available for there were no established groups whose function was to care for cows, sheep, or other domestic animals. Farmers sought help locally in popular remedies and folk experience and sometimes asked help from local practitioners who claimed to be knowledgeable about the ailments of particular animals. In the case of more widespread outbreaks of animal disease, authorities had sought advice on an ad hoc basis from members of the medical profession and others who claimed expertise.[15] The massive outbreaks of rinderpest which devastated French cattle in the forties and fifties made French administrators and agriculturists increasingly more concerned about animal epizootics, especially as a new interest in the improvement of agriculture was developing, influenced by the views of the Physiocrats that the land and its products were the primary source of wealth for a state.[16] A need was recognized for some readily available and trained source of assistance to handle animal diseases and Bourgelat's proposed veterinary schools to teach the medicine of animals seemed likely to provide personnel to satisfy this want.[17]

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[12] [Bourgelat], *Règlemens*, p 10.

[13] *Ibid.*, pp. 56-60. See also *Instructions et observations*, vol. II, pp 28-30; Railliet and Moulé, *Ecole d'Alfort*, pp. 529-35. The military pupils were housed separately from the provincial students and were under the command of a military officer in their day to day activities All aspects of their studies were under the direction of the veterinary school professors

[14] For a study of outbreaks of animal disease in the eighteenth century see George Fleming, *Animal Plagues Their History, Nature, and Prevention*, 2 vols. (London, 1871 and 1882), vol. I. See also Hours, *La lutte*, pp. 5-10.

[15] L. Moulé, "Du rôle des médecins dans la lutte contre les épizooties au XVIII^{e} siècle," in *Comptes rendus du deuxième congrès internationale d'histoire de la médecine*, Paris, 1921 (Evreux: Imprimerie Charles Hérissey, 1922), pp. 237-74; Shelby McCloy, *Government Assistance in Eighteenth Century France* (Durham, N.C : Duke University Press, 1946), pp 106-34.

In making his proposals a reality, Bourgelat obtained a powerful champion in the person of the administrator and government minister, Henri Bertin.[18] Bertin was a man much influenced by physiocratic thought; an Intendant at Lyon in the period 1754-1757 where he first knew Bourgelat, he went on to become Comptroller-General of Finances for France in 1759 and later, in 1763, was given the new ministerial post of Secretary of State charged with all the affairs of agriculture in France. In these posts he played an important role in the growth and character of the veterinary schools of France. All aspects of their development remained under his jurisdiction until his death in 1780. Bertin saw the veterinary schools as part of a policy aimed at perfecting and making known information which would improve agriculture and rural economy in its entirety and he was primarily concerned about the preservation of domestic animals which would aid the farmer in putting his land to good use.[19] His interest in agrarian reform was influential in developing a conception of veterinary medicine covering the care of all domestic animals.

In an effort to gain recognition for the schools and to exploit the need felt by administrators at both central and provincial levels of government for personnel to fight animal disease, Bourgelat sent his pupils into the field. Already in 1762 and 1763 Bourgelat was giving advice on epizootics and students were diagnosing and treating ailments of cattle and other animals.[20]

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[16] The classic study of physiocratic thought is Georges Weulersse, *Le mouvement physiocratique en France (de 1756 à 1770)*, 2 vols (Paris: Félix Alcan, 1910). The most recent work is Elizabeth Fox-Genovese, *The Origins of Physiocracy Economic Revolution and Social Order in Eighteenth-Century France* (Ithaca and London. Cornell University Press, 1976).

[17] The prospectus for the school entitled *Art vétérinaire ou médecine des animaux* which was circulated to intendants, indicates that all aspects of animal disease and care will be taught not only those relating to the horse. Archives départementales de la Côte d'Or, C 20. It has been suggested by Mammerickx that Bourgelat may have had assistance in drawing up this brochure (*Bourgelat*, p. 76) and it thus reflects the ideas of administrators as well as his own.

[18] The best study of Bertin is André Bourde, *Agronomie et agronomes en France au XVIII^{e} siècle*, 3 vols (Paris· S E.V P.E N , 1967), vol. 3, Chap. XIV. Bourde's work is an indispensable source for every aspect of the development of agricultural thought, technique and practice in this period

[19] Bourde, *Agronomie*, vol. 3, pp 1216-17

[20] L. Moulé, "Correspondance de Claude Bourgelat," *Bull de la Soc centr de méd vét* , 1917, *70*· 329-34, 363-66; Hours, *La lutte*, pp 42-8.

This practice was to continue steadily; teams of students and sometimes professors investigated outbreaks of disease in many districts of France.[21] It was through this program of assistance in epizootics that veterinarians came to be more widely perceived to have broader functions in the context of general agriculture. It was for their abilities to provide care for a range of domestic animals that intendants and local communities proved willing to finance the costs of their studies including board and lodging, uniforms, and the purchase of instruments and texts for a period of at least three years at Lyon or Alfort, and sought to set veterinarians up in practice in their districts on completion of their education.[22]

One intendant, Anne-Robert Turgot, went even further. A physiocrat like Bertin, he attempted while intendant of the Limousin to found a veterinary school at Limoges in 1766.[23] Bent on agricultural improvement Turgot, in the plans for his veterinary school, placed primary emphasis on the role of veterinarians in treating the diseases of farm animals rather than caring for horses. The school did not prove to be a success. It was only supported by provincial funds and it was closed after three years. Turgot's efforts to move beyond the tradition of the *maréchaux* seem to have proved counterproductive. The prospects of having a practice largely amongst farm animals were not very alluring economically or socially and Turgot came to believe that government subsidies would be necessary to keep veterinarians in rural areas.[24]

Turgot's interest in animal medicine did not cease with the closing of the Limoges School. Like Bertin before him, he moved to Paris to become Comptroller-General of Finances of France.[25] Although Turgot's ministry proved to be short lived (1774-1776), during his tenure of this important office the agronomic conception of veterinary medicine entered a new phase when Turgot sought to have scientific medical attention turned to the study of animal disease.

The immediate causes of this new development were the massive new outbreaks of the cattle plague or rinderpest which occurred during Turgot's period of office. Turgot was very much aware of the tremendous financial disruption caused by the disease; no proposed curative measures were efficacious and the expensive solution of cattle slaughter proved necessary to prevent its spread. The whole welfare of society, which in Turgot's view depended on the soundness and viability of its agriculture, was threatened by such outbreaks. A concern for the health of both animals and peasants led Turgot to seek information and assistance from a new body, composed of physicians, which was formally constituted as the Royal Commission for Epidemics. Both human and animal diseases were to be investigated by this group which evolved into the Société Royale de Médecine.[26] It was through the activities of this Commission that epizootics came to the attention of the medical world of the capital and physicians were encouraged to take a sustained interest in animal medicine and attempt to give it a scientific basis.

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[21] See for example the consultations on epizootics in Arch de la Côte d'Or, C 20 and Arch. de l'Ain, C 924.

[22] [Bourgelat], *Règlemens*, pp 14-8, Railliet and Moulé, *Ecole d'Alfort*, pp. 709-12; Hours, *La lutte*, pp. 24-7, 42-8. The Archives de l'Ain and de la Côte d'Or contain documents relating to financing the costs of veterinarians' education and providing them with the necessary supplies Students had to wear a distinctive blue uniform with gold buttons

[23] L Moulé and A. Railliet, *Turgot et l'école vétérinaire de Limoges (22 février 1766-5 novembre 1768)* (Paris: Asselin et Houzeau, 1902). Documents relating to this school are to be found in Archives Nationales F^{10} 1195

[24] AN F^{10}1195. Letter of Turgot to Bertin dated 13 September 1768

[25] For Turgot's biography see Douglas Dakin, *Turgot and the Ancien Régime in France,* 1st ed 1939 (Reprint, New York: Octagon Books, 1965)

Central to these developments were the experiences of the physician Félix Vicq d'Azyr in the field, whilst serving as Turgot's commissioner to investigate the rinderpest. Vicq d'Azyr was to become secretary and motivating force, first of the Commission for Epidemics, and then of the Société Royale de Médecine.[27] During his travels to many parts of France, Vicq d'Azyr met and worked with a number of veterinarians of whose abilities he speaks favourably and himself became very familiar with the practice of animal medicine.[28] In his advice on handling the epizootic he accords students of the veterinary schools an equivalent status with knowledgeable farriers and surgeons of the countryside.[29] In company with physicians and surgeons, he felt that veterinarians should contribute to the collaborative effort of observing and recording needed to gain an appreciation of disease patterns of both humans and animals in France—an endeavour which was to be a major program for the Society.[30] Vicq d'Azyr believed that both men and animals required medical attention and that it was not beneath a physician to interest himself in the latter, particularly when such a study put him in a position to render important services to the state. He pointed out that a benefit of studying veterinary medicine was the opportunity it offered to try daring experiments which would be crimes if practised on humans.[31]

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[26] The Commission was instituted by an Arrêt du Conseil d'Etat dated 29 April 1776. See Caroline C. Hannaway, "The Société Royale de Médecine and Epidemics in the Ancien Régime," *Bull. Hist. Med.*, 1972, *46*: 257-73.

[27] For the biography of Vicq d'Azyr see Jacques Moreau de la Sarthe, "Discours sur la vie et les ouvrages de Vicq d'Azyr" in *Oeuvres de Vicq d'Azyr* (Paris, 1805), vol. 1, pp. 1-88, P. J. G. Cabanis, "Eloge de Vicq d'Azyr," in his *Oeuvres complètes*, (Paris, 1825), vol. V, pp. 177-216. See also P. Huard and M. J. Imbault-Huart, "Vicq d'Azyr, Félix," in *Dictionary of Scientific Biography* (New York: Charles Scribner's Sons, 1976), vol. XIV, pp. 14-17.

[28] Félix Vicq d'Azyr, *Exposé des moyens curatifs et préservatifs qui peuvent être employés contre les maladies pestilentielles des bêtes à cornes* (Paris, 1776).

[29] Vicq d'Azyr, *Exposé*, p. 558. See also Félix Vicq d'Azyr, *Recueil d'observations sur les différentes méthodes proposées pour guérir la maladie épidémique qui attaque les bêtes à cornes, sur les moyens de la reconnaître par tout elle pourra se manifester et sur la manière de désinfecter les étables* (Paris, 1775), p. 34

Vicq d'Azyr's experiences and his research interest in comparative anatomy led him to postulate the unity of human and animal medicine:

> The preceding considerations on the diseases which attack men are applicable without any exception to those which attack animals. Medicine is one; and its general principles, once set out, are very easy to apply to different circumstances and species. Seen in this way, this science is greater and finer; the truths of which it gives proof are better understood and more developed. We know their real sources and we can always draw from them.[32]

Such an outlook on medicine stimulated continuing interest amongst physicians about veterinary medicine. Their endeavours were characterized by the effort to set up a scientific base for animal medicine through research and this was to effect the character and teaching of the veterinary schools.

The serious interest developed by the Société Royale de Médecine under Vicq d'Azyr's leadership in the problems of veterinary medicine necessarily brought it into a relationship with the veterinary schools, particularly that of Alfort. This relationship was not always to prove an easy one. When the Commission for Epidemics first proposed to offer assistance to intendants during epizootics in 1776, Bourgelat, then still head of Alfort, strongly opposed the plans. He felt that the role of the veterinary schools and the status of their students would be undermined if a group of academic physicians was allowed to organize continuing assistance for outbreaks of animal disease and impose their ideas on veterinary medicine.[33]

Following Bourgelat's death in 1779, however, the veterinary schools were open to new influences in their operation and development and at this juncture differing views on the future of Alfort became apparent. Vicq d'Azyr proposed that the veterinary schools should develop a research function. While the regular teaching should retain the basic style developed by Bourgelat—simple straightforward exposition of facts with a practical bias—the progress of animal medicine required that it be put on a thoroughly scientific basis. Alfort was an appropriate site for the scientific study of the anatomy, physiology, and pathology of animals, being close to the medical and scientific centers of Paris. On the other hand, Philibert Chabert, the new director of the school and a loyal disciple of Bourgelat, while not opposed to innovation in the subject of veterinary medicine, resisted the idea of making Alford into a scientific research center controlled by physicians. He sought to maintain its primary function as a school for training cadres of veterinarians to work in the field and to continue the tradition of professors being chosen amongst former graduates.[34]

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[30] Vicq d'Azyr, *Exposé*, p. 75 See also *Avis sur la correspondance de la Société Royale de Médecine, et sur les objets dont cette compagnie est chargée* (Paris, n d.)

[31] Vicq d'Azyr, *Exposé*, pp. 1-3.

[32] [Félix Vicq d'Azyr], *Mémoire instructif sur l'établissement fait par le Roi d'une Commission ou Société et Correspondance de médecine* (Paris, 1776), p. 30. Also Vicq d'Azyr, *Exposé*, p. 75· "en rapprochant la science vétérinaire de la médecine humaine, on rend à cette dernière ce qui lui est dû; on fait voir combien elle est une, combien elle est étendue, et combien elle est vraie;"

[33] Archives de la Société Royale de Médecine, Carton 190, Letters of Louis Vitet at Lyon to Vicq d'Azyr dated 8 and 22 April 1777.

Despite Chabert's resistance he was not able to prevent certain Society members developing a controlling influence in Alfort's affairs. The opportunity came in 1782 when Louis Bertier de Sauvigny, an administrator with a great interest in agronomic improvement who had close ties with many scientific and medical figures, was given the direction of certain agricultural matters including the veterinary schools.[35] Bertier proved very receptive to the idea of transforming Alfort into a research institute where work in all aspects of rural economy and veterinary medicine could be pursued.

The new era at Alfort opened with the institution of three new professorial chairs; one in comparative anatomy for Vicq d'Azyr, one in rural economy for Louis Daubenton, and one in chemistry for Antoine Fourcroy. Many new research facilities were constructed including laboratories for Vicq d'Azyr and Fourcroy and model sheepfolds and cow sheds for Daubenton. Extra personnel were appointed to assist these three professors in their endeavours. New land was bought for a farm, new gardens laid out, new experimental pastures sown, and new buildings erected for keeping far greater numbers of animals, both domestic and rare species. Vicq d'Azyr's comparative anatomy research soon ranged far beyond knowledge of the domestic animals which veterinarians had to treat, and not only a great variety of exotic animals, but also birds and even fish were housed at the School for research purposes. Daubenton sought knowledge on the better breeding, nourishment, and care of a wide range of animals including not only domestic breeds. Fourcroy pursued extensive research into the chemical analysis of animal substances.[36] Brilliant public assemblies were given to present the results of research and Alfort gained a scientific lustre which made it renowned throughout Europe.[37] Questions of animal physiology, pathology, and husbandry were well to the fore of scientific and medical attention.

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[34] AN F^{10} 1194, 134-141.

[35] Bourde, *Agronomie*, vol. 3, pp 1221, 1295-7 Bertier was intendant of the *généralité* of Paris

This research era brought veterinary medicine into scientific prominence and undoubtedly raised the status of the subject as a whole, but it did not immediately benefit the veterinary students. The new professors did virtually no teaching and involved students as assistants in research tasks of little use for their future practice.[38] Certainly some more knowledgeable students like Jean Huzard benefited from their medical contacts to go on to national prominence,[39] but the lack of instruction and the irrelevance of the research for ordinary veterinarians proved to be points of attack against the orientation taken by Alfort.[40] The dream of providing animal medicine and rural economy with a scientific basis in one great research institute did not last. The realities of veterinary practice, financial exigencies as well as Bertier's removal from power led to a reorganization and a return, in 1788, primarily to the task of producing farriers and veterinarians for work in the countryside.[41]

The distance between the scientific ideal of a unified human and animal medicine set within the context of an agronomic philosophy and the information required by veterinary students in their courses is a measure of the distance between Paris based expectations for the provision of medical care and the realities of rural life and practice. It was not possible in the world of eighteenth-century France to create a medical specialty and gain immediate acceptance for its practitioners. Economic and social realities and the continuing corporate outlook on practice of the arts militated against it.

In planning his curriculum Bourgelat had shown an appreciation of some of the difficulties that might confront veterinarians attempting to set up in practice. The students he preferred to all others were the sons of *maréchaux* because they could return to their fathers' shops to practise and because they were accustomed to the work of farriery and the forge. The sons of farmers and ploughmen had neither of these advantages and would require additional expense on the part of communities to set them up in practice on their return from the veterinary schools.[42]

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[36] Documents relating to this "academic" period of Alfort's history are to be found in AN F^{10} 1200. For discussion of the period see *Instructions et observations*, vol. IV, pp. 8-36; *Encyclopédie Méthodique, Médecine*, 13 vols (Paris, 1787-1830), vol. 9, pp 464-5; Railliet and Moulé, *Ecole d'Alfort*, pp 60-9, Bourde, *Agronomie*, vol. 3, pp 1221-25

[37] Bourde, *Agronomie*, vol. 3, p. 1222 has noted that in this period Alfort rivalled the Jardin du Roi in importance as an academic institution.

[38] AN F^{10} 1200, Letters of the Chevalier de Lormancelle to M Vaudran, July 1787.

[39] For Huzard's career see L. Bouchard, *Notice biographique sur J B Huzard* (Extrait des Annales d'Agriculture française, Paris, 1839).

[40] AN F^{10} 1200, Extrait d'un projet présenté à Monsieur le Controleur Général sur l'Ecole Royale Vétérinaire établie en 1766.

[41] Railliet and Moulé, *Ecole d'Alfort*, pp. 69-77.

Bourgelat anticipated a limited educational background for his students. The entry requirements stipulated that a pupil should be between the ages of sixteen and thirty, should know how to read and write, and should be strong enough to manage the heavy labour involved in handling animals.[43] For the teaching program he developed exercise books of the material for each course which the students learnt by rote. Theory was kept to a minimum and demonstration or practice greatly favoured.[44] Every aspect of the students' lives and studies was strictly regimented.[45] Bourgelat was strongly opposed to speculative discussion of such matters as the cause of disease and it is said that in order to protect the students from harmful exposure to the theories of other authors on veterinary matters he would confiscate any such books he found in the student's possession.[46]

By his choice of students and by organizing his teaching in this way, Bourgelat sought to prevent the new schools proving to be a means of social mobility, drawing students away from an essentially rural craft and giving them ideas above their status. He also no doubt sought to circumvent the problem of the social definition of the veterinarian by attempting to place him in an existing category of personnel. As a number of veterinary graduates found out to their cost, this did not take place as easily as might have been expected.

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[42] AN F^{10} 1194, 102-104, Letter of Bourgelat to De Fourqueux dated 20 February 1777. "Nous préférons les fils de maréchaux à tous fils de fermiers, labourers, habitans etc. Ceux-ci n'ayant aucune habitude de marteau et exigeant beaucoup plus de tems pour s'instruire dans la ferrue, d'ailleurs les autres trouvent à leur retour dans leur province l'établissement de leur père qu'ils pourraient secourir de leurs lumières tandis que les généralités se voyent souvent obligés de concourir aux premières dépenses que les fils des fermiers sont obligés de faire pour leur former une boutique " See also [Bourgelat], *Règlemens*, p. 10.

[43] [Bourgelat], *Règlemens*, pp 7-9 These requirements allowed the entrance of students of limited intellectual capacity The Archives de l'Ain, C 924, contain documents attesting to the inability of certain students to manage the courses and suggesting their withdrawal from the Lyon School

[44] [Bourgelat], *Règlemens*, pp 31-47; *Instructions et Observations*, vol. VI, pp 31-2, *Encyclopédie Méthodique*, *Médecine*, vol. 9, p 463 Bourgelat's various published texts are analysed by Mammerickx, *Bourgelat*, pp. 77-93

[45] The authoritarian character of School discipline is illustrated by the fact that the Schools possessed prison areas where students could be sequestered for infractions of the rules Arch de l'Ain, C 924.

[46] Railliet and Moulé, *Ecole d'Alfort*, p. 256.

In attempting to establish themselves in practice many veterinarians found themselves hampered by the corporative rights of the existing guilds of farriers. The *maréchaux* claimed that veterinarians were not entitled to shoe horses and do any other work of the forge or to care for sick horses unless they became members of the guild. As this could involve certain apprenticeship activities such as the production of a *chef d'oeuvre* and certainly the payment of a substantial fee, many veterinarians resisted. They petitioned the royal administration for the right to practice their art freely. Permissions were granted to some students giving them this right, but this did not bring an end to the difficulties.[47] During the 1770s groups of veterinary school graduates from different provinces sought further royal support. Sometimes a compromise solution was reached where they could practise freely all aspects of veterinary medicine in return for paying some sort of fee to the communities of farriers.[48] Evidence shows, however, that conflicts between veterinarians and *maréchaux* remained a problem until the Revolution.[49] Only those military veterinarians who became *maréchaux* in the army avoided these problems.

If the veterinarians were not able to fit easily into an established category of personnel as more knowledgeable farriers, neither were they readily accepted as personnel able to care for other domestic animals. This had been the role for veterinarians projected by intendants and provincial officials but in order to have care available they often found it necessary to provide continuing financial support.[50] Turgot had recognized that this might prove to be the case after his experiences at Limoges.[51] Intendants soon found that subsidizing the costs of veterinary education for selected students and paying veterinarians engaged in treating epizootics a daily fee plus costs of transportation, board, and lodging was not the end of their expenses. Requests by veterinarians for funds to set up in a shop, to be relocated in a town where there was not already a veterinarian, or for an annual salary were frequent.[52] Further the expected clientele of aspiring veterinarians, the farmers, proved reluctant to seek their services for the treatment of animal ailments. The major difficulties stemmed from the veterinarian's assumed expertise and his fees. After his years of training the veterinarian wanted recognition of his superior knowledge and status in animal medicine and sometimes charged fees to match. In these circumstances, the farmer, in more routine illnesses, continued to call upon local empirics whose rates were cheaper and more in line with the value of the animal. Often it was only in desperate cases that a veterinarian was called in when there was no longer much chance of saving the animal. Then if it died he got the blame, thus lowering his reputation and the presentation of a bill for his services on such an occasion served to alienate the farmer further.[53]

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[47] This issue is discussed by Railliet and Moulé, *Ecole d'Alfort*, pp 619-23 AN F^{10} 1195 contains royal edicts and other documents relating to the conflicts between *maréchaux* and veterinarians. One factor in choosing Alfort as the site for the Paris veterinary school was that it was far enough out to be beyond the jurisdiction of the *maréchaux* of Paris

[48] See for example Arch. de la Côte d'Or, C 20, Extrait des Registres du Conseil d'Etat du 14 Janvier 1775 This was an authorization for the veterinary students of Burgundy to exercise "l'art vétérinaire dans toutes les parties qu'il embrasse, ouvrir et tenir boutique de maréchal, mettre en pratique la forge et la ferrure des bestiaux et bêtes de charge, composer et débiter tous les médicamens nécessaires pour leur pansement et guérison, et faire ou faire faire généralement tout ce qui conviendra pour l'exercice dudit art" without being troubled by the community of masters of *maréchalerie* as long as they paid their portion of the charges of the community and no more.

[49] In 1787 and 1788, for example, a veterinarian named Perray at Bordeaux had his shop broken into and his horseshoeing tools taken by the *maréchaux* of the town. He was asked to sign a paper saying that he would become a member of their company and pay 200 livres for costs of seizure. He protested to Chabert and for his resistance was physically assaulted by the *maréchaux*. AN F^{10} 1195.

[50] AN F^{10} 1194, 109-112.

[51] AN F^{10} 1195, Letter of Turgot to Bertin dated 13 September 1768

These difficulties in establishing a rewarding practice for veterinarians in rural areas were recognized by many and a variety of possible remedies suggested. Some thought veterinarians should provide themselves with an income by taking up supplementary positions such as agriculture, the marketing of animals, or the management of a coaching station. With these positions to support them, they could give advice on animal diseases for modest sums and gradually build their reputations as practitioners.[54]

Others thought that the veterinarians' education was at fault. They were not taught enough about animals other than the horse while at the schools to be really in a position to treat them. It was also suggested that perhaps more stress should be laid on animal hygiene during training to prevent animals becoming diseased.[55] In fact one argument used to justify the establishment of the new professorial chairs in 1784 was the beneficial effects more detailed knowledge of animal care and rural economy would have on the veterinarians' practice. With information on breeding, nourishment, and improvement of stocks at their fingertips, veterinarians, it was claimed, would be sought after and amply rewarded for their services.[56] As has been already noted, the veterinary students benefited little from the new research. The regular professors came to recognize that an expansion of courses and outlook was needed at the veterinary schools but not in the direction of making veterinarians research scientists which might only further distance them from their rural neighbours.

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[52] Arch de l'Ain, C 924; Arch. de la Côte d'Or, C 20.

[53] This fate of the veterinarian is described by Gilbert and Huzard in "Rapport fait au Comité d'Agriculture et des Arts de la Convention Nationale, le 28 Nivose an III, par la Commission d'Agriculture et des Arts, sur l'organisation des Ecoles Vétérinaires," *Instructions et observations*, vol. VI, pp 22-4

[54] *Ibid*, pp. 24-5.

[55] *Ibid*, pp 17-8.

The most radical suggestions for improving the position of veterinarians came from Henri Bertin, while he was still director of agricultural affairs in France. Bertin, concerned about extending rural medical services, came to believe that veterinarians should not only care for animals but also provide care for humans in certain circumstances. To initiate this policy Bertin instigated two courses at Alfort in 1780 relating to human surgery which were to be taken by students who had completed their other studies. One was a course in midwifery and the other a course in the setting of broken bones. It was claimed that the course in midwifery would not interfere with the prerogatives of surgeons because the veterinarians would be applying this information in villages and hamlets where no person with any knowledge of childbirth techniques practised. The course in bone setting was justified by the large number of workers lost to agriculture every year because their fractures were improperly set by empirics or travelling bone setters.[57] Bertin also envisaged veterinarians treating human patients for diseases of the eyes and giving emergency assistance to the asphyxiated and the drowned.[58]

The new courses were only given for two or three years. It appears that few occasions were offered to the veterinarians for practising these skills, the public not being sufficiently convinced of their abilities to act in the domain of human childbirth and surgery and the surgeons being opposed to this infringement of their sphere of knowledge.[59] It is difficult to know if veterinarians did treat many human patients but there is evidence that it did occur occasionally. The pharmacy at Alfort distributed free medicine to the poor[60] and Bertin decreed that veterinarians in the countryside, whose practice was not situated in a place controlled by an apothecary's corporation, could prepare drugs for human illnesses as well as for animal ones.[61] One veterinarian, Edouard, after specializing in cataract operations on the horse, turned his skills to human patients who requested his assistance. His case reports indicate some success.[62] Veterinarians were called upon to treat victims of the bites of rabid animals and of other ailments supposedly acquired from contact with animals.[63] Further there are some instances where practitioners were reported to be practising both human and veterinary medicine but it is not always clear which was their primary training.[64]

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[56] AN F^{10} 1194, 323-324, Discours tendant à développer le plan et le régime des Ecoles Vétérinaires par M Loir, subdélégué de l'intendance à l'Ecole d'Alfort.

[57] AN F^{10} 1194, 150-151, *Instructions et observations,* vol I, pp 33-5, vol III, pp 20-3

[58] AN F^{10} 1194, 150-151; *Instructions et observations,* vol. III, pp 23-5

[59] AN F^{10} 1194, 322. Loir noted this. "Mais des sentiments d'inquiétude de la part de la classe inférieure de ceux à qui la conservation de l'homme est confié, et la répugnance d'appeller à leur secours les médecins des animaux, se sont opposés aux succès que les artistes vétérinaires pouvaient esperer de cette partie de leurs occupations " The surgeons' opposition is recorded in AN F ^{0} 1194, 150. "Les vues d'humanité que M Bertin s'étoit proposées dans cet Etablissement, n'ont pas eu le succès qu'il en avoit esperé Ces deux branches nouvelles de connoissances tenant plus à la chirurgie humaine, qu'à l'art vétérinaire, ont excité la jalousie des chirurgiens et ont formé contre les artistes vétérinaires, un parti qui a rendu leur existence encore plus pénible " See also *Instructions et observations,* vol. III, pp 20-3.

The potential development of veterinarians as all-purpose medical practitioners for the countryside, initiated by Bertin, did not take place. Some of those veterinarians who wished to practise human medicine seem to have left veterinary medicine behind and to have become surgeons. This problem of social mobility and the relationship between veterinarians and surgeons had been seen from the start. Bourgelat had been concerned to avoid giving veterinarians expectations of moving into surgery and Turgot had realized the more enticing prospects that surgery offered economically and socially to a young man investing several years without wages while studying.[65] When Vicq d'Azyr had suggested that perhaps a certain number of surgeons could participate in the teaching of the veterinary schools because a knowledge of surgery was in several respects linked with that of animal medicine and because surgeons were very widespread in the countryside, Chabert had countered that rivalry would result from the conflict of practitioners and, in fact, might bring about a loss of veterinarians to surgery.[66] Bertin's courses in midwifery and bonesetting supposedly had this effect.[67] In spite of the endeavours of the directors of the veterinary schools to prevent social mobility, some veterinarians undoubtedly used their education to move upward in the medical world. One recurring problem after having taken these students out of their rural environment was how to ensure their returning home to practise the skills acquired during a lengthy training. Alfort with its close location to Paris presented particular problems in this respect. There are frequent references in the documents to means of preventing students being enticed by the possible opportunities and life of the city.[68]

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[60] *Instructions et observations*, vol I, pp 22-3

[61] AN F^{10} 90, Letter of M de Bezance à M l'Intendant de Paris dated 6 April 1781

[62] *Instructions et Observations*, vol. IV, pp 269-89

[63] *Ibid* , vol. I, pp. 301-3, 387-8.

[64] For example *Instructions et observations* vol I, pp 45-6: vol III, p. 371

[65] AN F^{10} 1195, Letter of Turgot to Bertin dated 13 September 1768. "Et comme un bon chirurgien sera toujours plus souvent appelé, gagnera d'avantage et sera plus considéré que ne peut l'être un médecin des bestiaux Il s'ensuit qu'à talent égal tous les jeunes gens doivent préférer d'étudier la chirurgie, d'autant plus que les élèves en chirurgie peuvent même s'instruire à moins de frais en exerçant la barberie ou en travaillant en qualité d'aydes chez des maîtres chirurgiens pendant qu'ils suivent le cours de leurs études."

[66] AN F^{10} 1194, 134-141

[67] AN F^{10} 1194, 150 "D'un autre côté, élevant les prétentions de quelques uns d'entr'eux, ils se sont adonné à la chirurgie, qu'ils ne savoient pas, et ont abandonné l'art vétérinaire, dans lequel ils auroient pû rendre des services."

Even by the Revolution the precise role of the veterinarian still remained ambiguous. The most suitable education for veterinarians, their functions in practice, and the future of the veterinary schools were all matters of concern. The debates of the period on the veterinary schools and the development of veterinary medicine reflect many of the issues already discussed in this paper.[69] Nevertheless it is worth noting that important Revolutionary medical plans such as those of Vicq d'Azyr and Talleyrand incorporate discussion of veterinary medicine.[70] This is one aspect of reforming plans that has been little noticed. Attention has been focussed on the new role for surgery, the rise of clinical medicine, and the reform of the hospitals—all of them urban phenomena. The concern with veterinary medicine and providing rural health care for people and animals serves as a reminder of the context in which the reforms were set. France was still predominantly an agrarian country.

To some extent the history of the veterinary schools reveals how veterinary medicine reached out to engage the attention of physicians. Now physicians were largely town based in eighteenth-century France. Their interest in animal care in the countryside undoubtedly reflects the opportunity they saw to extend their controlling influence in medicine to the educational institutions, scope, and practice of rural health care. The physicians gained political support for this in terms of physiocratic doctrine. In the process veterinary medicine which had been evolving steadily from a rural craft was caught up in the global view of a unified medical science embraced by the physicians. Conservative veterinarians such as Bourgelat and Chabert tried to resist these changes but were not initially able to prevent the physicians developing a dominant influence. Physicians' activities proved to be their downfall. In pursuing veterinary science through research they lost sight of the actual role of veterinarians.

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[68] AN F^{10} 1200. See also [Bourgelat], *Règlemens*, pp. 44-7.

[69] *Instructions et observations*, vol. VI, pp 404-468 has a summary and critique of the major proposals concerning veterinary education and the nature and location of the veterinary schools. The plan of Gilbert and Huzard is referred to in ftn 53, also in vol VI, pp 7-66.

[70] [Félix Vicq d'Azyr], *Nouveau Plan de Constitution pour la Médecine en France* Présenté à l'Assemblée nationale par la Société Royale de Médecine, in *Histoire de la Soc. Roy de Méd.*, vol. IX, pp. 135-141 (Paris, 1790). Vicq d'Azyr was also very much concerned with the provision of health care for the people in the countryside See especially pp. 68-75, Charles de Talleyrand-Périgord, *Rapport sur l'Instruction publique, fait au nom du Comité de Constitution à l'Assemblée nationale, le 10, 11, et 19 Septembre 1791* (Paris, 1791), pp. 72-5, 183-8, 195-8

The ambiguous professional and social role of veterinarians is also revealing, for it throws light on aspects of the provision of health care in France at the end of the Ancien Régime. Simply creating such a group did not ensure their acceptance as practitioners. They had to fit into a network of guilds with established prerogatives on the one hand and, on the other, to offer care to clients unused to seeking professional help for the ailments of their animals. Moreover the impact of prolonged and intensive education on students drawn largely from a rural peasantry created problems. Bringing such people into a medical context raised the question of social mobility. They became, in a sense, a group of specialists in an underdeveloped society with sometimes no wish to return to the places from which they came. In this respect the study of the evolution of veterinary medicine provides an opportunity to appreciate some of the practical problems of bringing health care to the countryside of eighteenth-century France.

THE BIOGRAPHY OF DISEASE: LESSONS FROM CHLOROSIS

ROBERT P. HUDSON

There are many reasons for studying the biography of disease. The working physician may benefit from such endeavor in practical ways. He may be reminded, as just one example, that diseases are not immutable realities, but rather they are dynamic conceptions that change constantly in response to cultural and social forces, among which at times have been the interventions of physicians themselves.

For the historian the attempt at understanding older diseases in light of newer information can be an end in itself—a sort of scholarly mystery story. It can also be elusive, frustrating and treacherous. More than one of Clio's disciples, driven to the wall by the ultimately unknowable, has flown the coop of uncertainty on the wings of faith. Such Icarian temptations will always be with us. Within the single year of 1976 the journal *Science* carried an article arguing that convulsive ergotism might explain the physiological phenomena observed in the Salem witchcraft crises of 1692, and a rebuttal demonstrating that the symptoms described did not resemble ergotism at all.[1]

At the risk of disrupting Pope's repose, one might say that the proper study of diseases is disease. This is not to say that the biographer of disease must be trained in pathology or clinical medicine in order to study tuberculosis in the eighteenth century, but one must at least speak the language of those who were so trained at the time. In a broad sense problems in the history of disease are nothing more than the problems of history generally. Still, for those interested in the biography of disease, it can be helpful to view the subject in the context of a specific disease. What follows is an attempt at identifying a number of problems in the history of disease as they were exemplified by the decline and fall of chlorosis, a disease well known to late nineteenth-century physicians, but one rarely seen and even considered defunct by some clinicians as early as 1930.

There are descriptions of what could be chlorosis dating back to Hippocrates,[2] but the account which all later commentators would agree

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[1] L. R. Caporael, "Ergotism: the Satan loosed in Salem?" *Science*, 1976, *192* 21-26, and the rebuttal by N. Spanos and J. Gottlieb, "Ergotism and the Salem witch trials," *ibid.*, *194*: 1390-1394

[2] Not unexpectedly there is no way to feel confident about clinical descriptions of this era. For one discussion see A. von Hansen-Kolding, "Die Chlorose im Altertum," *Arch. f. Gesch. d. Med.*, 1931, *24*

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upon as describing chlorosis was published by Johannes Lange in 1554.[3] Lange registered surprise that older physicians knew nothing of the nature or treatment of the disease. In calling the condition *morbo virgineo* and implicating the "filthy" nature of the menstrual blood, Lange fixed in medical minds a basic orientation which endured for almost 400 years. The name chlorosis apparently was coined by Jean Varandal in 1615, for whom the term denoted an entire class of anemias,[4] not the single disease it came to be after Friedrich Hoffman's description of 1731.[5]

Despite admirable clinical descriptions that embodied most of the symptomatology of chlorosis as it was finally understood, all the earlier accounts contained confusing aspects as well. Into the nineteenth century chlorosis resembled a lion in the grass; all observers were certain of its existence, but none could delineate it with any ultimate clarity. Ashwell began his long 1836 article in this way, "Diseases of menstruation, *especially chlorosis and amenorrhea,* are, in this country, of very frequent occurrence . . . they are too *greatly neglected in their early stages, when they are most curable;* and lastly, *there is by no means an agreement of professional opinion as to their precise nature.*"[6]

After centuries of nosologic tortuosity chlorosis came to be associated almost exclusively with girls as they first encountered puberty. But it was advance in laboratory medicine which would finally bring physicians close to consensus on the nature of chlorosis. By the mid-nineteenth century reasonably accurate determinations were possible for the number of red blood cells and their hemoglobin content.[7] So armed, newly evolving hematologists demonstrated that the *sine qua non* of chlorosis was an iron deficiency anemia. This salutary development separated chlorosis from a

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175-184 So-called descriptions of chlorosis in the *Ebers Papyrus* more likely refer to the anemia of Schistosomiasis and hookworm, later called Egyptian chlorosis W. M Fowler, "Chlorosis—an obituary," *Ann. Med. Hist.*, 1936, *8*: 168

[3] J. Lange, *Medicinalium epistolarum miscellanea* (Basle, 1554), p 74, quoted in R. Major, *Classic Descriptions of Disease* (Springfield: Charles C Thomas, 1932), pp 444-447

[4] C. Mettler, *History of Medicine* (Philadelphia: Blakiston, 1947), p. 366.

[5] F Hoffmann, "Dissertatio de genuina chlorosis indole, origine et curatione (Edita 1731)," *Operum omnium physico-medicorum supplementum secundum* (Geneva, 1753), pars II, pp. 389 ff , cited in H Immermann, "Chlorosis," in A. H. Buck (ed.), *Von Ziemssen's Cyclopedia of the Practice of Medicine*, vol. 16 (New York: William Wood, 1877), pp 497 and 499.

[6] S. Ashwell, "Observations on chlorosis and its complications," *Guy's Hosp Rep* , 1836, *I*: 529

[7] For a useful account of the development of hematology at this time see A. H T Robb-Smith, "The history of the hedgehog's rosary," *St Bartholomew's Hosp. J* , 1933, *40* 149-152, 166-168, 211-216, 238-240 There is greater detail but narrower coverage in C Dreyfus, *Some Milestones in the History of Hematology* (New York: Grune & Stratton, 1957).

host of mimics, and clarified the clinical picture to the point that for Osler the disease in many instances could be "recognized at a glance."[8]

Sydenham's description of the green-sickness some 300 years earlier contained all the important clinical features of chlorosis as it came to be diagnosed by Osler's glance. "The face and body lose colour, the face also swells; so do the eyelids and ankles. The body feels heavy; there is tension and lassitude in the legs and feet, dyspnoea, palpitation of the heart, headache, febrile pulse, somnolence, pica, and suppression of the menses."[9]

The earlier incidence of chlorosis is impossible to determine, but by the turn of the twentieth century it was an extremely common diagnosis. "The chlorotic girl," wrote Sir Clifford Allbutt in his thorough review of 1901, "is well-known in every consulting room, public or private."[10] This observation is significant when viewed against the belief as early as 1915 that chlorosis was disappearing,[11] and the finding that between 1924 and 1930 a mere seven females were diagnosed as chlorotic on the wards of Guy's Hospital.[12] In 1936 Fowler was moved to wonder, "What disease . . . can compare with chlorosis in having occupied such a prominent place in medical practice only to disappear spontaneously while we are still speculating as to its etiology?"[13]

With the conclusion by Patek and Heath in 1936 that chlorosis had never been anything other than a simple iron deficiency anemia caused by poor diet and loss of menstrual blood,[14] one writer was ready to conclude that "this disease about which so much was written and disputed has finally ended its stormy career."[15] But chlorosis was dead only if it is granted that deceased diseases can leave their ghosts behind. References to the condition persisted thereafter and it remained one of the five major categories of hypochromic anemia which Witts regarded as diseases *sui generis* as late as 1969.[16]

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[8] W. Osler, *The Principles and Practice of Medicine* (New York: D. Appleton, 1893), p. 689.

[9] *The Works of Thomas Sydenham* (London: The Sydenham Society, 1850), v. 2, p. 288.

[10] C. Allbutt, "Chlorosis," *A System of Medicine by Many Writers* (London: Macmillan, 1909), v. 5, p. 701.

[11] For the United States the decline of chlorosis is documented statistically in W. Osler and T. McCrae, *Modern Medicine* (Philadelphia: Lea & Febiger, 1915), v. 4, pp. 645-646. To his satisfaction the author ruled out all factors that might have masqueraded as an absolute decrease in chlorosis. In an analysis of admissions to a number of British hospitals variously covering the years 1882-1922, J. M. H. Campbell came to the same general conclusion. "Chlorosis: a study of the Guy's Hospital cases during the last thirty years, with some remarks on its etiology and causes of its diminished frequency," *Guy's Hosp. Rep.*, 1923, *73*: 247-297. Overall Campbell's article is one of the better extant.

[12] L. J. Witts, "Chlorosis in males," *Guy's Hosp. Rep.*, 1930, *10*: 417.

[13] W. M. Fowler, *op. cit.* (n. 2 above), p. 168.

[14] A. J. Patek and C. W. Heath, "Chlorosis," *J.A.M.A.*, 1936, *106*: 1463-1466.

[15] A. L. Bloomfield, "Chlorosis," *A Bibliography of Internal Medicine. Selected Diseases* (Chicago: The University of Chicago Press, 1960), p. 87.

An obvious maxim in studying older diseases is that the terminology which must be used does not translate safely and easily to modern terms. Often it is simply impossible to decide if a given case report represented the disease in question. Descriptions of chlorosis before the mid-nineteenth century varied so widely that more than one entity must have been involved. In this circumstance the historian can usually identify erroneous diagnoses more readily than those that were accurate. For example, if a case history labeled as chlorosis reported large amounts of protein in the urine, one may at least suspect confusion between chlorosis and kidney disease, of which undoubtedly there were many. If on the other hand no findings were reported that were incompatible with a diagnosis of chlorosis, it still may not be assumed that the case indeed represented chlorosis. Clinical diagnosis well into the nineteenth century simply permitted no such accuracy for any but a few diseases, and these mostly of the epidemic contagious variety.

The pitfalls of imprecise terminology remain common, and commonly ignored, in medicine even into recent times. The longstanding confusion in the 1940s and 1950s over the therapeutic benefits of electronarcosis in treating schizophrenia came about largely because the investigators, in reporting their results, used the same unqualified word, electronarcosis, while their individual techniques for inducing the treatment varied widely.[17]

Before an iron deficiency anemia was established as the necessary basic defect in chlorosis, cases so labeled must have included instances of nephritis, abnormal thyroid function, subacute bacterial endocarditis, mitral stenosis, epilepsy, pulmonary tuberculosis and others.[18] Even after hemoglobin and erythrocytes could be assayed with reasonable accuracy, there was still no reliable way "to distinguish chlorosis from other anemias, simple or toxic."[19] Thus, even when anemia was present, subdivisions became necessary to account for clinical observations.[20] Categories were devised to allow for male chlorosis,[21] the "late chlorosis" needed to explain the delayed onset of symptoms in middle age,[22] "febrile chlorosis,"[23] for those exhibiting fever, and *chlorosis hiemalis* to explain cases which were symptomatic only during cold months.[24] Not unexpectedly, in their use of these terms contemporary writers did not have in mind the intellectual equanimity of later historians.

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[16] L. J. Witts, *Hypochromic Anemia* (Philadelphia. F A Davis, 1969), p. 1

[17] J Bogen, "Glissando electronarcosis: some semantic confusions in psychiatric electrotherapy," *Diseases of the Nervous System*, 1963, *24* 401-413

[18] For examples of the confusion that abounded before the laboratory came to the rescue, see Dr Barker, "Clinical remarks on cases of chlorosis," *London Med Gaz* 1843, *1*: 353-358; F. Churchill, *The Diseases of Females. Including Those of Pregnancy and Childhood* (Philadelphia: Lea and Blanchard, 1847), pp 126-127, and H Immermann, *op cit*. (n. 5 above), p 500.

[19] C. Allbutt, *op. cit*. (n 10 above), p 717

[20] The evolution of the term, chlorosis, during the nineteenth century was described by J. M H Campbell, *op cit* (n. 11 above), pp. 253-257. Campbell believed the differences in meaning after 1890 were slight and could not account for the diminution in the incidence of chlorosis agreed to by almost all observers

In the biography of disease one sees at times a variation of the Heisenberg effect. In therapeutic intervention physicians and patients have unwittingly altered the equation of pharmacology and physiology in ways that confuse the observed results, and may only be appreciated historically. It follows that the history of a disease can be fully reconstructed only if all possible effects of medical intervention are taken into account. The Heisenberg effect can be seen in chlorosis and its treatment with iron. As far back as Sydenham, iron was advocated in the treatment of chlorosis,[25] and by the mid-nineteenth century, with few exceptions,[26] mars [iron] was considered specific for the green sickness. Disputes persisted over the relative efficacy of organic versus inorganic compounds, the dose, and the proper duration of treatment, but on the general indication for iron there was little debate.[27] In the face of all this, inconsistent results and many outright failures were being reported.[28] Some such instances undoubtedly related to inadequate dose and duration of treatment, and others to the type of preparation used. Beyond this, however, both physicians and patients probably contributed to the uneven results observed by altering the absorption of prescribed iron in ways they could not appreciate at the time.

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[21] The concept of male chlorosis was something of a burr under the saddle of theory over the years. The association between chlorosis and problems unique to young girls was, as noted, strong from Lange on. Through the years, however, repeated instances in males were encountered wherein the symptomatology was indistinguishable from that of chlorosis A certain Dr. Scott was willing to recognize chlorosis in males if they took on what amounted to feminine characteristics, i.e if they became "wan and emaciated, despondent and indolent; averse from both bodily and mental exertion, and wanting resolution to encounter the storms of active life . . " *Med. & Phys. J.*, 1800, *3*: 215. Gradually male chlorosis was perceived as so rare that when it was encountered, the physician was obliged to consider other conditions such as phthisis. S. Ashwell, *op. cit.* (n 6 above), p. 534. Male chlorosis became a theoretical problem when chlorosis was associated with an iron deficiency anemia, because, as one physician observed, " . any cause which will lessen the number of these [red blood] corpuscles will produce chlorosis in either sex without distinction." J. T. Evans, "Cases of chlorosis in the male," *Dublin Hosp. Gaz.*, 1845, *1*: 79. Indeed, it followed that the existence of chlorosis in males militated against any crucial role for either amenorrhea or menorrhagia See e.g. S Fox, *Observations on the Disorder of the General Health of Females Called Chlorosis* (London Samuel Highley, 1839), p. 2 As the many causes of secondary anemia came to light, male chlorosis was included therein, and its relative rarity thus squared with the role of the menses in women. A. L. Bloomfield, "Relations between primary hypochromic anemia and chlorosis," *Arch. Int. Med.*, 1932, *50* 335 and L. J. Witts, *op. cit.* (n. 12 above), p. 419

[22] A L. Bloomfield, *op. cit.* (n 21 above), pp 328, 336.

[23] Allbutt, while respecting Osler's belief in febrile chlorosis, managed to exculpate himself by hinting that fever, when present, was "significant of some complication." *Op cit.* (n. 10 above), pp 704-705

[24] A Stengel, "Diseases of the blood," in T. L. Stedman, *Twentieth Century Practice* (New York: William Wood, 1896), v. 7, p. 328

[25] Sydenham observed that ". . . when one gives mars [an old term for iron] in the pale color the pulse becomes at once fuller and slower, the pallor disappears and once again the face is rosy and ruddy." *Op cit* (n. 9 above), p. 98. Short accounts of the history of iron in therapeutics have been recorded by R. L. Haden, "Historical aspects of iron therapy in anemia," *J.A.M A.*, 1938, *111*: 1059-1061, and H. A Christian, "A sketch of the history of the treatment of chlorosis with iron," *Med Libr & Hist J*, 1903, *1*: 176-180. A useful short history of the treatment of chlorosis with iron is found in V. F Fairbanks *et. al*, *Clinical Disorders of Iron Metabolism* (New York: Grune & Stratton, 1971), pp. 9-21

The patient's unwitting contribution to her therapeutic failure involved what was called a "morbid appetite." This symptom describes the craving for exotic foods in unusual amounts, but also the eating of a variety of substances not normally ingested, and having no nutritive value such as dirt, clay, and chalk, where the phenomenon is termed pica. Practically every writer, in describing the usual symptoms of chlorosis, included a morbid appetite, and the condition remains medically significant today, principally in youngsters and pregnant women.[29] Pica is now thought to be the result of iron deficiency and can be cured by the administration of iron.[30] For our purposes the point is that pica could in turn be expected to aggravate the iron deficiency of chlorosis. Not only do the substances involved in pica diminish the appetite while adding nothing nutritional, but certain clays bind dietary iron in complexes that cannot be absorbed through the intestinal wall.[31] Thus chlorotics who ate clay or chalk could have remained anemic while ingesting what would otherwise have been therapeutic levels of iron. This would help explain the fact that the physicians who had the best results were those who insisted on large doses of iron.[32]

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[26] The most notable American to be unsure about iron therapy in chlorosis was T. Gaillard Thomas, and he was somewhat self-contradictory. Thomas did not believe chlorosis was simply an anemia, and, in fact, used the frequent failure of iron therapy as a differential diagnostic point When an anemia complicated chlorosis, however, he resorted to iron *A Practical Treatise on the Disease of Women* (Philadelphia Henry C Lea, 1876), pp. 772-778 See also J. R. Reynolds, *A System of Medicine* (Philadelphia· Henry C. Lea's Son & Co., 1880), pp. 471-472.

The use of iron suffered a setback in the late nineteenth century due to the influence of Bunge's theory Bunge, who believed in general that animals could only catabolize, not anabolize, argued that inorganic iron could not be absorbed through the intestinal wall, and in any event, could not be used by the body to synthesize hemoglobin. Lacking controls, he argued that the observed successes of iron therapy could as well be due to the power of suggestion as to the iron itself. G. Bunge, "Ueber die Assimilation des Eisens," *Hoppe-Selyer Ztschr. Physiol. Chem*, 1885, *9* 49-59, quoted in V. F. Fairbanks, *op. cit.* (n 25 above), p 14

[27] Credit for popularizing the use of iron in chlorosis is often accorded Pierre Blaud, whose preparation of ferrous sulfate was never improved upon despite many attempts. "Sur les maladies chlorotiques et sur un mode de traitement, spécifique dans ces affections, *Rev. méd. franç. étrang* 1832, *1*: 337-367. Despite his role in developing one of the few specific drugs available to nineteenth-century physicians and his recognition of the essential nature of chlorosis, Blaud did not make his way into any of the texts of medical history by Garrison, Major, Mettler or Castiglioni Blaud's conclusions were given a firmer experimental footing by Ralph Stockman, who, in a series of papers near the end of the century, cleared up many of the disputes surrounding iron therapy, and delineated chlorosis generally in a way that left little to be done by his followers. See "The treatment of chlorosis by iron and some other drugs," *Brit. Med. J* 1893, *1*. 881-885, 942-944, "On the amount of iron in ordinary dietaries and in some articles of food," *J Physiol.*, 1895, *18*· 484-489; "Observations on the causes and treatment of chlorosis," *Brit Med. J.* 1895, *2*: 1473-1476; and "A summary of sixty-three cases of chlorosis," *Edinburgh Med. J.* 1895, *41* 413-417 The evidence favoring iron in the treatment of chlorosis became so overwhelming that a failure came to cast doubt either on the diagnosis, W Osler, *op cit* (n. 11 above), p 653, or on the adequacy of the iron compound used. As Allbutt put it, "One often suspects that incurable chlorosis means insoluble pills . . ." *Op cit* (n 10 above), p. 720.

[28] R. L Haden, *op. cit* (n. 25 above), *passim*

Physicians too were at times defeating their own best efforts. Constipation was considered an invariable symptom or complication of chlorosis, and to some physicians, sluggish bowels were even considered the first link in the development of the disease.[33] In addition the constipative nature of iron was known at the time, so that laxatives were looked upon as necessary adjuncts to the treatment of the anemia. Indeed, for many physicians, a mild aperient was necessary to cleanse the bowels in preparation for the administration of iron.[34] The Merck Index for 1896 listed a variety of magnesium salts as laxatives.[35] Chief among them in popularity was Epsom Salts or magnesium sulfate. Some of these magnesium salts are now recognized as binders of intestinal iron, whose effect would be to prevent dietary iron from reaching the bloodstream. Easily the most popular form of iron administration during the epidemic of chlorosis was the pill devised by Pierre Blaud which bore his name.[36] Blaud's pills contained potassium carbonate in addition to the iron, and it is now known that much of the iron in these pills would have been bound as insoluble ferrous carbonate, and the pills could have succeeded only when taken in large doses, as Blaud, incidentally, recommended.

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[29] M. Cooper, *Pica* (Springfield. Charles C Thomas, 1957). The term derives from the Latin for magpie and, according to the author, was "attached to the habit of seeking diverse, strange substances as food because the magpie is a bird of variegated plumage, using many voices, and picking up a diversity of things to satisfy hunger or curiosity," p 3

[30] V. F Fairbanks, *op. cit* (n 25 above), pp. 247-250

[31] V Minnich, *et. al.*, "Pica in Turkey: effect of clay upon iron absorption," *Am J. Clin. Nutrition*, 1968, *21* 78-86

[32] R. L Haden, *op cit.* (n 25 above).

[33] M Hall in *Tweedie's Cyclopedia of Practical Medicine* (Philadelphia· Lea and Blanchard, 1845), p 397 Also A Stengel, *op. cit* (n 24 above), p 332.

[34] H Marsh, "Remarks on chlorosis and haemorrhage," *Dublin Quart. J. Med Sci* , 1846, *2* 345.

[35] *Merck's 1896 Index* (New York: Merck & Co), p. 152.

[36] See n. 27 above.

In addition to constipation, chlorotics frequently suffered dyspepsia, and indeed in the earlier nineteenth century gastric ulcer was considered a fairly frequent symptom or complication of chlorosis. Magnesium salts entered the picture again at this point, since magnesium oxide, an effective antacid, is one of the strongest binders of iron among the medicinal magnesium salts.[37]

Even if we cannot establish, in any quantitative sense, the extent to which magnesium contributed to the variable results reported in treating chlorosis with iron, there is something to be learned from this aspect of the story. Conflicting reports in the history of therapy, at times, may be explained by later pharmacologic understanding. Fallibility among contemporary clinical observers is always a possible explanation, but it should be invoked last and reluctantly. The historian of disease will examine all aspects of what physicians were doing in the treatment of a given condition, and not merely the principal therapeutic agent. Any number of other interventions could have distorted the observed results and thus would help to explain the reasons for the professional acceptance or rejection of a given treatment. The experience of Sir Andrew Clarke is instructive in this regard. Clarke held that constipation caused chlorosis, a belief that was buttressed by his success using only laxatives as treatment. The reason for Clarke's success was exposed by Ralph Stockman, whose late nineteenth-century work did more than that of any other one person in clarifying chlorosis. Stockman simply demonstrated that Clarke's laxative recipe contained iron in doses that would have been therapeutic in some patients.[38] Which only reminds us that there is iatrogenic history as well as iatrogenic disease.

An immediate effect of the acceptance of the germ theory of disease, one that was quickly productive but ultimately insufficient, was a simplistic approach to the problem of etiology. This line of reasoning held that tuberculosis, the illness, resulted when the tubercle bacillus invaded the human body and began multiplying. Such thinking, codified in the Henle-Koch postulates, proved powerful indeed. As it turned out, this view was too narrow. Its negative effect on scientific thinking was recounted by, among others, Richard Shope who helped demonstrate finally that swine influenza in man was not produced by a single microbe, but required a virus and a bacterium acting in concert. In describing how his efforts were so long thwarted by the prevaling mindset of one microbe-one disease, Shope wrote, "After the presence of a filterable virus in influenza was established, the etiology of the disease itself became more confused than it had been when *H.*

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[37] V Minnich, *op. cit* (n. 31 above), pp 85-86

[38] A Stengel, *op cit* (n 24 above), p. 332

influenzae was suspected."[39] Since their inception, of course, the Henle-Koch postulates have undergone numerous revisions to accommodate the complexities added to our understanding of microbial infection in human hosts.[40]

A few microbiologists realized early on that it was not sufficient to consider etiology apart from such factors as nutrition, host resistance, and variations in microbial virulency. Still, the notion that one agent equals one disease spread widely through medical thinking and is even yet discernible in physicians and their teachers today.[41] More to the point here, biographers of disease are at times susceptible to similar thinking.

Chlorosis serves as a reminder of the complex interplay of physiology with cultural and social elements in the production of human pathology. This aspect of the story is better understood in light of a few words on the current understanding of iron metabolism. To protect the body against the destructive effects of excessive iron, intestinal absorption is fixed at a rate that barely replaces the tiny amount lost physiologically. The balance is so nice that the chronic loss of two teaspoons of blood daily will exceed the body's capacity to absorb iron from a normal diet and anemia will develop. Also relevant is the fact that a deficiency in total iron stores can exist without producing an anemia, the latter developing only when the stores are greatly depleted. The anemia of iron deficiency can result from a decrease in dietary iron, an increase in bodily demands for iron, or a loss of blood. It would seem from the historian's vantage point that all three of these conditions, in varying proportions, conspired to produce the anemia of chlorotic girls.

There is more than coincidence in the fact that the occurrence of chlorosis peaked in the Victorian era. Cultural forces of this period contributed to the actual incidence and severity of the disease, as well as to how the condition was perceived by physicians and laymen. In exploring that fascinating Victorian affliction (or device), the vapors, Scarlett described the lady of good breeding at the time as "an individual of great sensibility (that beloved 18th-century word), compounded of the cloistered virtues, who was pale, had no ostensible appetite, and who swooned at the slightest provocation. She was enveloped in clothing and her viscera were constricted by tight lacing. Custom and maidenly modesty forbade voluntary employment and anything like violent exercise."[42] Every feature of this description would be expected to contribute to the incidence and severity of chlorotic anemia.

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[39] R. E Shope, "The epidemiology of the origin and perpetuation of a new disease," *Perspectives in Biology and Medicine*, 1964, *7* 270.

[40] A. S. Evans, "Causation and disease the Henle-Koch postulates revisited," *Yale J Biol. & Med*, 1976, *49* 175-195.

[41] G. L Engel, "The need for a new medical model," *Science*, Apr. 8, 1977, *196* 129-136.

A number of observers were aware that the green sickness tended to occur in families. In all probability this proclivity had nothing to do with genes, but was largely cultural. Ordinarily, new-born children have a rich supply of iron, but children of iron-deficient mothers began life short of iron, "larval chlorotics" as they came to be called.[43] For many of these, an inadequate diet during childhood preserved their deficiency of total iron stores somewhere above a level that would produce a frank anemia. Young women of these families developed the fully blown anemia of chlorosis only when confronted with a menarchal loss of blood and the simultaneous demand for iron occasioned by rapid pubertal growth.

If other factors were equal, the likelihood that a given adolescent might escape chlorosis would be expected to increase with larger amounts of a variety of foodstuffs. Many women of the time undoubtedly were getting insufficient amounts and varieties of foods. For the poor girl the reason was simply economic. She ate what she could afford, and this effectively excluded many iron-rich foods.[44] To the extent that they emulated the morality of their wealthier sisters, the poor only aggravated the tendency to anemia already guaranteed by their defective diet. In part due to a belief that foods derived from animals increased sexual appetite, late nineteenth-century women tended to eschew meat, eggs, and even milk.[45] It was this concern over dietary influence on the sexual drive that led some observers to conclude that masturbation actually contributed to the development of chlorosis.[46]

Then there was the corset. The "tight stays" came back to Britain around 1830 and "even more barbarous devices" came into vogue after 1854, for example the crinoline reinforced with whalebone and steel.[47] Such apparel became high fashion in the 1860s and '70s, and was abating by the turn of the century.[48] In a rough way, then, the incidence of chlorosis, as best it can be reconstructed, paralleled the practice of tight corsetry.

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[42] E. P. Scarlett, "The vapors," *Arch Intern. Med*, 1965, *116*. 142.

[43] A J. Patek, *op cit.* (n. 14 above) p. 1465

[44] Many observers commented on the inadequate diets of chlorotic girls, though some saw the problem as one of effect rather than cause R. Stockman, *op. cit.* (n 27 above), p. 487, S. Ashwell, *op cit.* (n. 6 above), p. 540, J M. H. Campbell, *op. cit.* (n. 11 above), p 273, A. Stengel, *op cit.* (n. 24 above), p 348, and H Marsh, *op. cit* (n. 34 above), p. 313.

[45] V. Bullough and M Voght, "Women, menstruation, and nineteenth-century medicine," *Bull Hist Med.* 1973, *47*. 79 The same mental set led a certain Dr. Walton to employ a vegetable diet in curing a case of nymphomania *Am. J. Med Sci.*, 1857, *33*: 47-50.

[46] J. T. Evans, *op cit* (n. 21 above), p. 80, and Claudian (anon) "Chlorosis," *Boston Med. & Surg. J*, 1847-1848, *37:* 55

[47] *Encyclopedia Britannica*, 1911, v. 7, p. 244

Meinert devised the first thorough thesis implicating the practice of tight body binding as one of the direct causes of chlorosis. The theory involved the notion of splanchnoptosis, or a displacement of intra-abdominal structures in ways that disrupted autonomic nervous function.[49] As late as 1955, in an article titled "Chlorosis and Stenosis," one H. St.H. Vertue was still amplifying and defending Meinert's conception.[50]

Lacking accurate measurements of the physiologic changes produced by tight abdominal binding, Meinert's thesis cannot be adopted or rejected. Putting aside the theory of autonomic disruption, however, corsets must have aggravated the chlorotic tendency in a more mundane manner. By purely mechanical pressure the devices must have decreased the volume of food ingested. Further, the mere contemplation of converting a natural bowling pin to an artificial hourglass must have been a strong psychological inducement to restrict dietary intake, at times even to unhealthy extremes.

The importance of social and cultural forces on the development of chlorosis thus draws attention to another aspect of the biography of disease, which is that for practical purposes it is insufficient to talk of the natural history of diseases apart from human activity.[51] The notion that diseases have a rather pure natural history is often encountered in descriptions of contagious diseases as they are introduced for the first time into a non-immune population. Here, disease is seen as a discrete invader which makes its way into susceptible human hosts, producing an epidemic. In ensuing passages through its host the microorganism gradually loses its virulency and the host gains immunity, so the disease gradually becomes endemic or disappears. While this sequence undoubtedly has repeated itself many times in the history of disease, it is unlikely that it has ever done so totally apart from the influence of cultural practices. The bacillus of bubonic plague found a susceptible population in Western Europe in the fourteenth century, but it also found the *ménage à quatre* without which an epidemic could not have been sustained—people, rats, fleas, and *Yersinia pestis,* all living as a single, propitious zoological family.

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[48] H. St.H Vertue, "Chlorosis and stenosis," *Guy's Hosp. Rep.*, 1955, *104*: 331-335.

[49] C. Allbutt, *op cit.* (n. 10 above), p. 637. For an account of the manner in which tight clothing increased menstrual pain, see C. D. Mosher, *Woman's Physical Freedom* (New York. Womans Press, 1923). The severity of the constriction commonly employed at the time is evidenced anecdotally on pp. 31-34.

[50] H. St.H Vertue, *op cit.* (n 48 above).

[51] For a discussion of the many aspects of the conception of disease see O. Temkin, "Health and disease," and "The scientific approach to disease: specific entity and individual sickness," *The Double Face of Janus and Other Essays in the History of Medicine* (Baltimore. The Johns Hopkins University Press, 1977), pp. 419-440 and 441-455 respectively.

On superficial examination chlorosis also appears to have undergone some sort of natural demise. As noted, the incidence of the disease began decreasing drastically around the turn of the twentieth century. Very little of this decrease can be attributed to direct medical intervention. While it is true that by this time iron was almost universally prescribed in chlorosis, and while it would have been effective in many cases, iron therapy played only a small role in the disappearance of the disease. There would have been fewer "larval chlorotics" among the girls born to mothers treated with iron, but this could not account for the observed decrease. Further, while iron therapy would have lowered the number of existing chlorotics, it would have had little effect on the number of new cases. Finally of course the decrease in chlorosis occurred before there was any routine addition of iron to commercial foodstuffs.

The fact that medical intervention played only a minor role in the disappearance of chlorosis does not necessarily lead to the conclusion that the demise of chlorosis was natural. The explanation could lie in the fact that human intervention does not have to be purposeful or even conscious to influence the incidence of a given disease. There is evidence that antioxidants added to dry cereals as preservatives may have contributed inadvertently to the dramatic decline in carcinoma of the stomach in the United States in the past few decades.[52] Whether this proves to be the case is not crucial to the point that changes in patterns of living can alter the nature and incidence of disease in ways not intended or appreciated at the time. Many such factors can be related to the early twentieth-century decline of chlorosis. Corsets were abandoned, women led more active lives, and the prejudice against meat abated. During the same period, a number of laws were passed, which, in England at least, improved living conditions generally, and which should have increased the total effective dietary intake of iron for many persons, especially the young.[53]

There is a tendency among some commentators to extrapolate the stumblings of our forebears into an attitude of disdainful distrust of everything they believed and did, a sort of ignorance by association. In these minds any medical practice that cannot be squared with today's truth was at least deplorable and at times morally reprehensible. It is rare, for instance, to find a forgiving word spoken of bloodletting, the sheet anchor of medical therapy for hundreds of years. The cautious historian, however, will be slow to conclude that all the greatest clinicians before 1830 were not seeing something that could at least *be interpreted* as salutary in the response of patients to bloodletting. For example, patients with fevers were often bled. Since a rapid pulse characterizes fever, bloodletting that slowed the pulse would have been interpreted as a desirable therapeutic response. Contrary to what most medical students still learn, the sudden loss of a pint of blood produces a slowing of the pulse in a significant number of persons, and is the rule during the syncopal stage.[54] Whether these donors would have the same response if suffering from a febrile illness can never be known, but is of little moment for the matter at hand.

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[52] R J Shamberger, "Antioxidants in cereals and in food preservatives and declining gastric cancer mortality," *Cleveland Clinic Quart.*, 1972, *39*. 119-124

[53] Among these were the Housing Act and Prevention of Diseases Act of 1890; the Factory and Workshop Act (regulating dangerous industries, ensuring fresh air, extending a number of conditions to women) and the Public Health Act of 1891, the Shop Hours Act of 1892 which limited the working week for young persons to

The green sickness, too, can illustrate the phenomenon of ignorance by association. As chlorosis came to be equated with an iron deficiency anemia, a nettlesome question remained. Why was the green skin no longer seen in the many cases of iron deficiency anemia still encountered? The easy answer was to conclude that chlorotics never had exhibited green skin, that the notion of green skin was somehow erroneously assigned to the name of the disease, after which the tyranny of tradition led all observers to see a hue that was not there. In fact there is some historical evidence to support this conclusion. Of 27 authors consulted in this study who itemized the usual signs of chlorosis, 16 listed a greenish skin as characteristic of the disease, though not always finding it in the cases they reported. Ten physicians either doubted the existence of a particular chlorotic skin or made no mention of it in the cases they reported, and one physician had seen the characteristic skin hue only once. Richard Cabot, who wrote a popular textbook of physical diagnosis, concluded in 1915 that "it takes the eye of faith to see any justification for the title of the disease."[55]

Must we conclude from such ambiguity that the green skin of chlorosis

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74 hours; the Employment of Children Act, 1903; Shop Hours Act providing earlier closing, 1904, the Education Acts of 1906 and 1907 providing medical services and free meals for needy school children; the minimum wage established by the Trade Board Acts in 1909; National Sickness Insurance, 1911, and the Public Health Act providing treatment of tuberculosis in 1913. J M H Campbell, *op cit* (n 11 above), pp. 282-283

[54] H A Shenkin *et al.*, "On the diagnosis of hemorrhage in man a study of volunteers bled large amounts," *Am J. Med Sci.*, 1944, *208:* 421-436.

[55] R Cabot in W Osler, *op. cit.* (n 11 above), v 4, p. 650

never really existed? Not necessarily. The fact that chlorosis came to be identified with an iron deficiency does not mean it was precisely the same condition as the iron deficiency anemias seen today. The medical condition known as chloroma can be instructive in this regard. Chloroma is a variant of acute myelocytic leukemia, and is characterized by the presence of a green pigment in many tissues of the body. The exact nature of the pigment is unknown, but its intriguing feature is that it disappears rapidly when exposed to light, which may explain why autopsies rarely report it in the skin.[56] There is no reason whatever to implicate the ephemeral pigment of chloroma in the green skin of chlorosis. The aim is only to underscore the possibility that unknown physiologic factors could have existed in certain chlorotics to explain the presence of a greenish caste in the skin, if, as is also possible, chlorosis was something more than a simple iron deficiency anemia.

Lacking much assistance from the laboratory, clinicians of the chlorotic era placed great emphasis on the physical examination. At times this indeed led to the error of seeing too much, but from this one may not conclude that three centuries of careful clinical observers were reporting a striking physical finding that was not there. In the question of the green skin of chlorosis, the available historical evidence permits us to go no further than a suspension of judgment.

The green skin of chlorosis illustrated another pitfall in doing the history of disease. While developing medical knowledge often clarifies previously puzzling aspects of disease, it must be remembered that today's medical understanding is itself a stand of sand. In developing this essay an attempt was made to utilize physiological principles to relate the green skin of chlorosis to the tight binding of young women at the time. When binding was begun early in life and continued over a long period, it produced profound skeletal changes reminiscent of Chinese foot-binding. One such skeleton exhibited pelvic dystocia, and an almost incredible lower thoracic outlet so constricted that it will not admit an average fist. Under such conditions it is easy to imagine a chronic compression of the common bile duct with resultant stasis of the bile in the gallbladder. So trapped, the bile would revert to biliverdin producing a jaundice in which the skin is an olive green rather than the yellow seen in the usual bilirubin jaundice. The observation that the skin of chlorotics was only tinged and not deeply green could be explained by a partial but chronic obstruction of the common bile duct, and the fact that the green skin was described only spottily squares with the practice of binding, which varied with time as well as with women in the same era. The only problem with this neat and attractive wedding of history and physiology is that a final consultation with a gastroenterologist revealed that biliverdin jaundice is now itself a deceased disease, indeed one that apparently never existed. What seasoned clinicians were seeing only a few decades ago which led them to distinguish the green skin of biliverdin jaundice is no more explicable than the hue of the skin of chlorosis.

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[56] L V. Ackerman and J. A del Regato, *Cancer· Diagnosis, Treatment, and Prognosis* (St Louis C V Mosby, 1970), pp 1008-1013

History, someone has said, purifies itself like a running stream. The analogy applies nicely to the nosological history of chlorosis, allowing that a given historical stream may be clarified and muddied many times as it seeks the sea. Indeed, to extend the simile, it should be added that not even the purity of the sea reveals all to our vision. When the basic pathology of chlorosis was identified as an iron deficiency anemia, much of the confusion surrounding a venerable mystery was dispelled. Yet the biography is incomplete. Questions remain. As late as 1969 the hematologist Witts, who arrived on the scene early enough to join the funeral procession, remarked, "However one looks at it, one is left with the uneasy feeling that the mystery of chlorosis, like that of Edwin Drood, remains unsolved."[57]

If Witts words are true, and in a strict sense they are, still there is no cause to despair of a final solution. Even in its elusiveness chlorosis can teach, which returns us to the analogy between history and a running stream. Science, just as it clarifies disease can also help fill in the biography of past disease. One rarely sees reference to the famous English Sweate without the adjective, mysterious, and indeed the essential nature of the sweating sickness has never been established. An epidemic disease marked by high fever, profuse foul sweats, delirium, and a fearful mortality, often within 24 hours, the Sweate defies identification with any known contagious disease. Epidemics struck England in 1485, 1507, 1518, 1529, and 1551, that is at intervals of 22, 11, 11, and 22 years.[58] The disease has been studied by numerous commentators, but no one has commented on its remarkable periodicity.

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[57] L. J. Witts, *op. cit* (n. 16 above), p. 35

[58] The dates of the five epidemics are a matter of some dispute, particularly the second episode. See R. H. Major, *op cit* (n 3 above), p. 152; J F. C. Hecker, *The Epidemics of the Middle Ages* (London. The Sydenham Society, 1846), pp 181-305, C Creighton, *A History of Epidemics in Britain* (London· Frank Cass, 1965), v 1, pp 237-264, *Encyclopedia Britannica*, 1911, p 186; A Hirsch, *Handbook of Geographical and Historical Pathology* (London: The New Sydenham Society, 1883), v. 1, pp. 82-83, M. Shaw, "A short history of the sweating sickness," *Ann Med. Hist* , 1933, *5*. 249-258; and H Zinsser, whose dates were used here, *Rats, Lice and History* (Boston· Little, Brown, 1935), pp. 95-98

It is not merely an exercise in science fiction to note that sun spots have cycles of roughly eleven years or that our most serious recent pandemics of viral influenza occurred in 1946, 1957, and 1968. The cyclical nature of disease, including meteorological factors, is now the subject of serious study by a group of scientists of varied stripes. This is not to suggest that the English Sweate was a particularly virulent form of influenza, but only to emphasize the point that even though we get further from the sweating sickness in time does not necessarily mean we get further from its understanding. The same can be said of chlorosis.

THE ENGLISH BLOODLETTING REVOLUTION, OR MODERN MEDICINE BEFORE 1850*

PETER H. NIEBYL

The apparent lack of medical progress before 1850 was due, in part, to a concerted attack by an influential but now largely forgotten group of physicians on views which we would now recognize as modern. This attack, which I shall call the Bloodletting Revolution, was influenced by social factors.

William H. Welch, when asked by his alma mater to talk on "Yale's Relation to Medicine," indicated he would have more to say if he could talk on Yale's relation to football or Calvinism. He finally dug up some material on Yale's first professor of medicine and surgery, Nathan Smith.[1] Smith's 1824 *Treatise on Typhus* seemed to Welch like "a fresh breeze from the sea mid the dreary and stifling writings of most of his contemporaries."[2] Welch was impressed not by a single observation or discovery but by Smith's generally modern thinking and approach to the disease. Smith considered it a distinct entity with a fairly constant form in different individuals. It did not affect the same person twice, and it had a specific natural history dependent upon a specific contagious cause. Finally, the "modern" Smith believed bloodletting would not cut short its course and indeed in some cases could be harmful. Smith's description was accurate enough for Welch to recognize that it was the disease we now call typhoid rather than typhus. Although Smith's views appear modern, it is clear from the essay itself that these views were not unheard of. On the contrary, the novelty remarked upon by Smith himself was John Armstrong's recent use of bloodletting to treat "typhus."[3] Indeed, Smith remarked that the *new* doctrine had won many converts and thus divided the practitioners of New England.[4] A review of

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[1] Simon Flexner and James T. Flexner, *William Henry Welch and the Heroic Age of American Medicine* (New York, Viking Press, 1941), p 58

[2] William H. Welch, "Relation of Yale to medicine," in *Paper and Addresses by William Henry Welch* (Baltimore, Johns Hopkins Press, 1920), vol 3, pp. 256-257.

[3] Nathan Smith, *A Practical Essay on Typhous Fever* (New York, 1824), p. 49.

[4] *Ibid*., p 50.

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Smith's work at the time also commented on the division between the bloodletters on the one hand and those who with Smith considered the disease a debilitated state worsened by bloodletting.[5] Smith considered this common fever (whatever it really was) in relation to other specific contagious diseases such as smallpox, measles, whooping cough, etc.[6]

John Armstrong, the bloodletter cited by Smith, originally believed that common "typhous" fever was due *solely* to human contagion, but soon changed his views, ascribing it solely to the mal-aria of marsh miasmata. In his popular book on typhus in 1816 he in effect abandoned the idea of contagion.[7] He denied that even contagious fevers were necessarily due to a specific contagion.[8] What Armstrong objected to most strenuously was not simply the idea of human contagion *per se* but rather what he called "the doctrine of debility."[9] As in the case of Smith, the idea that a specific contagion directly caused debilitating effects on the human organism had led to the conclusion that bloodletting would be harmful in such diseases. For Armstrong this was only an apparent rather than a real debility.[10] He contended that typhus was "congestive" and "inflammatory" thus "overloading" the patient's strength. Relief was thus accomplished by bleeding away the excessive congestion. Following Armstrong, general bloodletting became much practiced in "all forms" of fever. Previously, it had been only rarely used in "typhoid."[11] Under the influence of Armstrong's doctrine not only did specific diseases lose their identity but even the varying "types" of fevers in different years or seasons tended to lose their identity.[12] Armstrong himself stressed that it was not simply bloodletting but immediate, copious bloodletting that was required.[13] Armstrong put forth his seemingly new doctrine as a triumph of observation, and quoted Sam Johnson: "Human experience, which is constantly contradicting theory, is the great test of truth."[14] Believers in debility were

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[5] [Review of] Smith, *op cit* (n. 3 above), in *Med Rev & Anal J*, 1824, *1*: 63-80.

[6] Smith, *op. cit.* (n. 3 above), pp. 16, 22, 46-47.

[7] [Review of] Francis Boott, *Memoir of the Life and Medical Opinions of John Armstrong*, *Brit. For. Rev.*, 1836, *1*. 42, 69.

[8] John Armstrong, *Practical Illustrations of Typhus and Other Febrile Diseases* (London, 1816), p 4.

[9] *Ibid*, p. 43

[10] John Armstrong, *Facts and Observations, Relative to the Fever Commonly Called Puerperal* (Hartford, 1823), p. 40

[11] Robert Christison, "Continued fever" in A. Tweedie, *A System of Practical Medicine* (Philadelphia, 1840), p. 234

[12] Boott, *op. cit.* (n. 7 above), p. 39

[13] Armstrong, *op cit* (n. 10 above), p 57.

[14] Armstrong, *op cit.* (n. 8 above), title page.

"big wigs" who had erected a "fantastic superstructure."[15] His popularity was such that in America he was called "the modern Sydenham." At home the influential *Edinburgh Medical Journal* noted that Armstrong won the "broad seal of public favour" to the degree that there was scarcely a practitioner, even in the most sequestered villages, who had not read his works and professed to act upon his maxims.[16]

The differences between Smith and Armstrong were far greater than simply quibbles over vague pre-bacteriological doctrines of contagion. Despite his avowed commitment to experience over theory, Armstrong's contention that the apparent debility is not real revealed theoretical encroachments.

> An opinion (said Armstrong) should seldom be formed wholly from external appearances, as they often mask the true character of fever, which, for the most part, can only be perfectly ascertained, by penetrating beyond the exterior.[17]

Now it should be pointed out that the specificity of contagious disease was derived not from the supposed specificity of causative agent (which was hypothetical) but rather from the specificity of the clinical picture, particularly the specific cutaneous findings. These were best described in Robert Willan's *On Cutaneous Diseases* of 1808 where among his specific diseases the flea-bite-like petechiae of *purpura contagiosa,* a disease "propagated" in poor, crowded living conditions, leave no doubt about confusing typhus with typhoid, much less common fevers.[18] In the case of scabies he could point to the observed offending organism, *Arcus scabiii,* and to the modern eye accurately described their burrows and distribution on the skin as well as their topical therapy.[19] With equal specificity and clarity Willan described contact dermatitis due to specific metals and substances used in certain occupations.[20] Even more evanescent eruptions such as urticaria he recognized as having a specific but non-contagious etiology in such substances as shellfish, and described the time sequences, evanescent character and angio-edema so precisely as to leave no question about the diagnosis.[21] And so it was with the febrile contagions as well. One cannot simply attribute such specificity to the recurrent experience gained in the public dispensary or fever hospital, for Armstrong too had originally worked in the dispensary. Institution patient population did, however, make him more aware of the local spread of contagious eruptive fevers.[22] Mal-aria and "epidemic constitutions" of the atmosphere and climate à la Sydenham were not in theory rigorously local in spread.

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[15] Boott, *op. cit* (n. 7 above), pp. 43, 60.

[16] Francis Boott, *Memoir of Life and Medical Opinions of J Armstrong* (London, 1833), 2 vols , pp. 18-19

[17] Armstrong, *op. cit.* (n. 8 above), p. 17.

[18] Robert Willan, *On Cutaneous Diseases* (London, 1808), pp. 451-452.

[19] *Ibid* , pp 74-75.

[20] *Ibid* , p 166.

[21] *Ibid* , pp 397-406.

Nor can one simply attribute Willan's approach to his choice of subject matter. Willan's chief predecessor in "dermatology" was Daniel Turner's *De morbis cutaneis* of 1714. For Turner the skin served as an external manifestation of internal humoral disease modified to be sure by seventeenth-century iatrochemistry. It even included a humoral type of scabies. In contrast to Turner's internal humoral conditions which he treated by bloodletting,[23] Willan was decidedly cool to both the debilitating practice of bloodletting[24] and to humoral theories.[25] Willan was concerned about the directly debilitating effects of the eruptive fevers.[26] One cannot simply attribute the differences between Turner and Willan to differences in temporal proximity to ancient medicine. In fact Willan had more references to Galen and the ancients than Turner. It is ironic that Willan, having far more specific references to the ancients than his contemporaries, nevertheless seems the most modern in the precision of his clinical descriptions and distinctions. But unlike his predecessors, Willan was raiding the ancient literature, not for its humoral theory, but for its clinical terminology and its precise usage, or lack of it. In another sense Turner appeared the more modern in his desire to make use of the latest in skin physiology as the basis of his text. He included the microscopic analysis of various skin structures (sweat glands control internal humors) and the latest references to the basic science literature, *Philosophical Transactions,* but the clinical "fall-out" was so little as to leave him far behind Willan who had avoided such basic science.

Temkin, who stresses the early vague ideas of pollution and miasmas, in his historical analysis of the concept of infection, has noted the lack of progress from the time of Fracastoro and Sydenham to 1850.[27] Even with regard to therapy Fracastoro already expressed an objection to bloodletting in contagious diseases, which he clearly attributed to specific fomites rather than a diffuse uncleanliness.

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[22] *Ibid*, p 391

[23] *Ibid.*, p. 15.

[24] *Ibid.*, pp. 352-353, 369, 415

[25] *Ibid.*, p. 82.

[26] *Ibid.*, p. 271.

[27] Owsei Temkin, "An historical analysis of the concept of infection," in *Studies in Intellectual History* (Baltimore, Johns Hopkins Press, 1953), p 135, n. 27

> In these fevers (said Fracastoro) I do not approve of phlebotomy, because they [the contagious seminaria or seeds] very seldom have their origin in the body, they come from without, either from fomes or the air or they are conveyed from one person to another.[28]

Similarly, Felix Platter, professor of medicine at Basel, in 1507 could not see how bloodletting could possibly remove the *externa pestifera qualitate* or *seminaria pestis* from the body.[29] But it was Fernel, the professor of medicine at Paris, who, in his first medical work *De vacuandi ratione* of 1545, really anticipated the doctrine of direct debility that was to grow at the expense of humoralism. Fernel distinguished two types of weakness: *Oppression,* an indirect debility, due to humoral excess overloading the patient's strength and required bloodletting. *Languishing,* due to the direct debilitating effect of external causes such as contagion and contraindicated bloodletting.[30] The resemblance to Armstrong's distinction is striking but, whereas Fernel expanded direct debility at the expense of humoral indirect debility, Armstrong reasserted indirect debility as the *apparent* debility of congestion at the expense of the supposed "real" debility of directly acting contagion. The comparison between Armstrong and Fernel is not so remote as it may seem, for even the nineteenth-century opponents of the doctrine of real debility themselves trace this "false" doctrine as far back as Sir John Pringle's treatises on camp, hospital and jail fevers of the mid-eighteenth century. Pringle was accused of having "an unfounded apprehension of extreme and even fatal debility."[31] He had maintained these institutional fevers were "malignant and contagious." Pringle used bloodletting as a major diagnostic test since bleeding would make these specific debilitating fevers much worse while improving the usual humoral fevers. Pringle drew an analogy between the causative agent of these contagious fevers and the mite seen microscopically by Leeuwenhoek in scabies, showing that his view of contagion was very different from that of miasmatic theory.[32] At the same time John Huxham made two of his three major categories of fevers debilitating types—the so-called "low nervous" and the malignant fevers. Huxham maintained that since inoculation of the least amount of variolous matter will mix with the whole blood, "like a ferment with the liquor to be fermented," bleeding off a part of the blood in *any* contagious disease would be as fruitless as trying to destroy fermentation by removing part of the fermented liquor.[33]

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[28] Hieronymus Fracastorius, *De contagione et contagiosis morbis et eorum curatione,* Latin text and tr. W C Wright (New York, Putnam's Sons, 1930), p. 243.

[29] Felix Platter, *Praxeos, tractus secundus* (Basil, Waldkirdii, 1603), p. 172, This passage is not in *De febribus liber* (1597).

[30] Jean Fernel, *De vacuandi ratione* (Paris, C. Wecheli, 1545), pp. 30, 33-34

[31] Benjamin Welsh, *A Practical Treatise on the Efficacy of Blood-Letting in the Epidemic Fever* (Edinburgh, 1819), p. 104.

[32] Sydney Selwyn, "Sir John Pringle: hospital reformer, moral philosopher and pioneer of antiseptics," *Med. Hist.*, 1966, *10:* 268.

While individual epidemics of typhus in wars and in certain institutions can explain the existence of a clinical category of a debilitating contagious disease characterized by petechiae, it hardly explains the general growth of the doctrine of debility, especially during the mid-eighteenth century, at the expense of humoralism. Part of the explanation seems to be related to the growth of the cities. William Buchan's long popular *Domestic Medicine* first appeared at the time of Pringle. It indicated that the special situation of contagious disease had become the prevailing one in the growing cities. Thus Buchan noted that, while the populace still expected to be bled in fevers, due to the change in occupations and life style of urbanization the "low nervous" or putrid fevers had now become the *common* ones wherein bloodletting is most harmful, further weakening the patient's strength.[34] Similarly, Hugh Smith in 1761 noted:

> In large cities, and London, in particular, the febrile diseases will not in general require the lancet, as they mostly verge either upon low nervous, or the putrid kind. . . . In the country, where the inhabitants are generally more robust and plethoric as their fevers are for the most part of the inflammatory kind, evacuations will become necessary. . . .[35]

Contagion was the opposite of inflammation still associated with humoral plethora, the robustness of dietary excess. Nutritional deficiency, on the other hand, was associated with both "cutaneous diseases" and contagion.

> Fevers of a very bad kind (said Buchan) are often occasioned by what is called *poor living.* When the body is not sufficiently nourished, the humours become bad, and the spirits sink; from whence the most fatal consequences must ever ensue. *Poor Living* is likewise productive of cutaneous diseases. It is remarkable that cattle, when pinched in their food, are generally affected with diseases of the skin. . . .[36]

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[33] John Huxham, *An Essay on Fevers* (London, 1757), p. 102

[34] William Buchan, *Domestic Medicine; or, The Family Physician* (Edinburgh, 1769), pp. 169-170.

[35] Hugh Smith, *Essays, Pathological and Practical, On the Nature and Circulation of the Blood and on Bloodletting* (London, 1761), pp. 127-128

[36] Buchan, *op. cit.* (n 34 above), p. 131

Cutaneous, contagious disease of the growing debilitated urban poor, which became loosely termed "typhus" in Britain, unquestionably included more diseases than modern typhus. Awareness of urban poverty and its diseases in large measure accounts for the rise of the doctrine of debility in the eighteenth century and a concomitant decline in the use of bloodletting.

The extent of the physicians' perception of "debility" was of course affected by the type of patient population he encountered in his practice. In addition to private practice there were institutions (Dispensaries, Fever Hospitals, Houses of Recovery, and the Infirmaries) dealing largely with the urban, sick poor.

> In large towns, also, fevers require less the use of bloodletting than in the country; . . . and in crowded hospitals than in private practice.[37]

Institution-based physicians saw disease associated with "deficiency of nutriment" as episodic, dependent on scarcity of food in harvest failure, or large scale unemployment associated with general suspension of manufactories, or termination of a war, and thus more closely related to epidemic disease than "filth" *per se* which it was pointed out was a constant.[38] In the late eighteenth century one form of the doctrine of debility was particularly associated with an education at Edinburgh and the Infirmary there, but it should be kept in mind that the debility theories of Edinburgh's William Cullen and John Brown were simply peculiar scholastic alterations of an idea rooted in the earlier "low nervous" and malignant fevers, which Armstrong was to call the "medical phantoms, debility and malignity."[39] The so-called "London school," like Armstrong, had also stressed theoretical skepticism[40] and empirical confidence,[41] but led by Fothergill and Fordyce and, unlike Armstrong, they more than Cullen focused on debility, putrid (malignant) fever and opposition to bloodletting.[42] Said Armstrong:

> It was the error of those splendid theories to regard debility . . . not simply as one of a

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[37] Anthony T. Thomson, *Elements of Materia Medica and Therapeutics*, vol. 1 (London, 1832), p 462.

[38] Thomas Bateman, *A Succinct Account of the Typhus or Contagious Fever of This Country* (London, 1820), pp 7-11.

[39] Boott, *op. cit* (n 7 above), p 40

[40] "All physiology, as far as it is hitherto known, is totally or nearly useless in explaining any thing which happens in fever." George Fordyce, "Of Simple Fever" (London, 1800), *Five Dissertations on Fever* (Boston, 1815), the first dissertation, p. 20.

[41] Boott, *op. cit.* (n 7 above), p 70

[42] Fordyce, *op. cit.* (n. 40 above), pp. 288-289, Welsh, *op cit* (n 31 above), p 116

> concourse of symptoms, but as the cause of the whole series of phenomena which characterize the disease. Evacuations of every kind were therefore much dreaded. . . .[43]

One of Armstrong's followers noted in 1817 that "the cautious methods of the old school" had a "deeply rooted prejudice" in both the public and the medical profession especially with regard to bloodletting. He admitted that "typhus" had been particularly bad that year in Bristol among "the lowest class" and that want of sufficient nutriment would seem to be the most obvious circumstance, but he countered by noting that contributions raised for the relief of the indigent have been "so ample as to make this explanation untenable."[44] Perceptions of the availability of food differed with perceptions as to the nature of debility and the benefits of bloodletting, which from antiquity to the nineteenth century was treatment for dietary excess thought to cause fevers and inflammations despite variations in the supposed physico-chemical properties of the humoral mediators.

Before Armstrong, Henry Clutterbuck of the General Dispensary in London found little therapeutic significance in distinguishing the circumstances of contagion or even contagious from non-contagious.[45] The putrid symptoms of black tongue and excretions as well as petechiae were no longer being seen.[46] He doubted the importance of deficient nutriment in fevers.[47] He saw "general disposition to fever in all ranks" and not just the indigent.[48] Finally, this dispensary physician questioned the advisability of extending fever hospitals, especially in view of their expense.[49] What was the therapeutic consequences of this shift in attitude?

> A great revolution is taking place in the treatment of this disease. Blood-letting, which, but a few years ago, was looked upon with abhorrence in the cure of contagious fever, . . . is proved, by ample testimony, to be not only the most powerful, but the safest of remedies. . . .[50]

Benjamin Welsh, another leader in the new enthusiasm for bloodletting, claimed that the "revolution," a term he also used, was not an actual revolution in the disease itself, i.e. a change in the disease, but rather a "mental revolution" of the practitioners.[51]

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[43] Armstrong, *op cit* (n. 8 above), p. 109.

[44] J C. Prichard, "Cases of typhus fever, with observations on the nature and treatment of that disease," *Edin. Med J.*, 1817, *13:* 413, 414 (ftn).

[45] Henry Clutterbuck, *Observations on the Prevention and Treatment of the Epidemic Fever, at Present Prevailing in the Metropolis and Most Parts of the United Kingdom* (London, 1819), p. 15.

[46] *Ibid* , pp. 23-24

[47] *Ibid* , p 33

[48] *Ibid* , p. 36

[49] *Ibid* , p. 49

[50] *Ibid.*, p. 8.

The strength of the revolution is evident from the weakness of the opposition. Thomas Bateman of the London House of Recovery and the Fever Hospital had been a follower of Robert Willan and had published additions and digests of Willan's works on cutaneous diseases.[52] Bateman had nothing but contempt for Clutterbuck's simple theory that all fevers were due to inflammation of the brain and all required bloodletting.[53] Rather than strictly maintain the specificity of disease, however, Bateman claimed (in contrast to Welsh's mental revolution") that the fever itself had changed due to "our improved and improving system of domestic oeconomy."[54] He defended the role of deficient nutriment but claimed that under the present improved conditions the contagious fevers were now occurring in "milder forms."[55] With that reservation he then all but joined the revolution against the "terror of debility" as resulting from the "prejudices of education," and took up the lancet not only in fever but other diseases as well.[56]

Both the cause and the success of the Bloodletting Revolution which culminated in Armstrong and Clutterbuck can in large measure be found in the earlier demobilization of military surgeons. Benjamin Welsh remarked:

> The reduction of our vast military and naval establishments, at the conclusion of the war, has not been without its use in promoting the doctrine which we attempt to advocate. For medical men, retiring from these services, have settled in almost every corner of the empire, and generally carrying along with them the practice which they have hitherto found so beneficial, remove from their fellow practitioners their prejudicial apprehensions of debility from venesection, by the strongest of all arguments,—ocular demonstrations of its safety.[57]

Their lack of education was considered a virtue. Clutterbuck himself referred to the use of therapeutic bloodletting during the war.[58] Although fevers had been recognized as contagious, the same cannot be said for wound inflammation in the eyes of the British army and navy surgeon who vigorously combatted the excess vascular activity with copious bloodletting, fifty ounces at a time to a total of two hundred![59] One surgeon wrote that while one could not bleed quite so much in private life, "the *principle* is good."[60] It had been argued that bleeding "robust, well fed seamen" cannot justify its application among "the debilitated and poor inhabitants of cities," but this was now contradicted by personal experience.[61] In Gibraltar medical officers were bleeding cases of malignant fever, and claimed it reduced rather than increased debility (obviously only apparent debility).[62] Hydrophobia, long attributed to a specific poison in the dog bite and recognized as unresponsive to therapy, had come to be included with the malignant conditions of great debility. However, a military surgeon of the Bengal Establishment reported in 1813 having cured a case by copious bleeding to the point of fainting.[63] With almost simultaneous discovery a similar successful case was reported by an assistant surgeon of His Majesty's 22d Light Dragoons. He too stressed that the bleeding must be copious, carried to the point of scarcely feeling a pulse in either arm.[64] During the "revolution" opening the temporal artery became a "trivial operation," while other surgeons used the jugular vein.[65]

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[51] Welsh, *op. cit* (n. 31 above), p. 43.

[52] Thomas Bateman, *Delineations of Cutaneous Diseases Exhibiting the Characteristic Appearances of the Principal Genera and Species Comprised in the Classification of the Late Dr Willan* (London, Longman, 1817).

[53] Bateman, *op. cit.* (n. 37 above), p. x.

[54] *Ibid*, p. v.

[55] *Ibid.*, pp. 3-4.

[56] *Ibid*, p. 98

[57] Welsh, *op cit* (n 31 above), p. 141

[58] Clutterbuck, *op. cit.* (n. 45 above), p. 70.

The success of the bloodletting revolution, as is often the case with "successes" in science or medicine, brought about a priority debate. Clutterbuck acknowledged that the army and navy surgeons antedated his work.[66] Among the military it was asserted that Thomas Sutton was "the first who dared to deviate from the routine practice which had resulted from an erroneous hypothesis."[67] In 1806 Sutton did attack the "temerity to draw blood" in the typhus kind of fevers.[68] Sutton himself apparently put forth his claim for precedence in bloodletting at a time when "the merit of originality" still rested with Clutterbuck, but G. A. Borthwick claimed his father, while a surgeon to His Majesty's 14th Light Dragoons, followed a long Irish tradition with "his fearless use of the lancet," in his "Practical Observations on the Treatment of Nervous, Putrid, or Malignant Fever."[69] One author pointed out that anyone acquainted with the medical history of the West Indies would know that bloodletting by the naval medical department there would counter any current "claim to novelty."[70] In the Mediterranean one reason given for the copious bloodletting by military physicians was the tropical climate of their practice.[71] Heat apparently exacerbated inflammation and fever as if physical and physiological qualities rather than contagious agents were the object of therapy. Sutton's *A Practical Account* was qualified by tagging on "in this climate" to the title.[72] In the context of tropical bloodletting it is not surprising that the earlier American literature, such as Robert Jackson's work on Jamaica fevers in 1791, was invoked and Benjamin Rush praised.[73] Jackson had said that "blood-letting has been long employed in the cure of fevers of tropical climates."[74] Rush noted that the debility of warm weather and the Caribbean was "indirect."[75] The Baltimorean Nathaniel Potter, in his edition of Armstrong's work on typhus, noted with pride that Benjamin Rush was the first to refute the groundless hypothesis that contagious fevers will not bear bloodletting.[76] The British press noted the irony in the fact that Rush's work had originally been received there "with horror and incredulity" and now appeared to be ahead of even Sutton by twenty years![77] An anonymous author objected to giving Rush any priority pointing to the long tradition of army and naval hospital practice which Rush himself quotes.[78]

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[59] James Wardrop, *On Bloodletting* (London, 1835), p. 71.

[60] William Gibney, "Case of pneumonia, with observations on the practice of blood-letting in that case," *Edin. Med. J* , 1817, *13*· 188.

[61] John Allan, "Observations on the utility of blood-letting as the principal remedy in continued fever; with some cases of an epidemic in which it was practised with uniform and remarkable success," *Edin. Med. J.*, 1816, *12.* 264.

[62] J. Humphreys, "On the malignant fever which prevailed at Gibraltar in 1813, and its treatment by blood-letting," *Edin Med J.*, 1816, *12* 177, 180, 182.

[63] John Shoolbred, "Case of phydrophobia, successfully treated," *Edin. Med J* , 1813, *9.* 30-49

[64] A. Berry, "Cases of hydrophobia, treated, one of them successfully, by Mr F Tymon, Assistant Surgeon, H M. 22d Light Dragoons," *Edin Med. J* , 1813, *9* 22-30.

[65] H. Perry, "Observations on blood-letting," *London Med. Reps* , 1822, *17*· 113-115.

[66] Clutterbuck, *op cit* (n 45 above), p 74.

[67] Thomas Mills, *An Essay on the Utility of Bloodletting in Fever* (Dublin, 1816), Critical Analysis in *Edin Med. J.*, 1817, *13.* 370.

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[68] Thomas Sutton, *A Practical Account of a Remittent Fever Frequently Occurring Among the Troops in This Climate* (Canterbury, 1806), p. 24

[69] G A. Borthwick, "Bleeding in fever," *Lancet*, 1833-34, *2*. 193.

[70] "On the introduction of the depletory method of cure in tropical fever," *Edin. Med. J* , 1813, *9* 49.

[71] "Observations on the fever prevalent in the Mediterranean, as it occurred on board one of His Majesty's line-of-battle ships on that station," *Edin. Med. J.*, 1813, *9*. 141

[72] Sutton, *op cit* (n. 68 above)

[73] Welsh, *op. cit.* (n. 31 above), pp. 136-137.

[74] *Op. cit.* (n. 70 above), p 50.

[75] Benjamin Rush, *Medical Inquiries and Observations . . A Defence of Blood-Letting as a Remedy for Certain Diseases* (Philadelphia, 1796), pp. 195-196.

[76] John Armstrong, *Practical Illustration of Typhus Fever, of the Common Continued Fever, and of Inflammatory Diseases* (Notes by N. Potter) (Philadelphia, 1821), p. 121.

[77] Boott, *op cit* (n. 7 above), pp. 44-45.

[78] *Op cit.* (n. 70 above), pp 50-51

The historian today must award the coveted "merit of originality" to Thomas Sutton because his appears to be the first of a series of English publications using statistics in bloodletting long before the famous work of Pierre Louis in France. Sutton drew on his practice at the General Military Hospital at Deal. His mathematics involved no more than simple fractions, which he used for clinically sound comparisons. He took the same disease in the same hospital and compared the mortality of three different regimens: (1) the usual treatment for typhus, (2) the same with moderate bloodletting at the onset, and (3) bloodletting as the principal remedy. The mortality rates were 1 in 3 (11/37), 1 in 5 (3/20), and 1 in 20 (never exceeded).[79] Thus the more one used the lancet, the better the results. The following year Clutterbuck's *Inquiry* appeared. It contained no statistics but summarized the current practice in fevers, namely the almost total disuse of bloodletting in England, its extensive use in tropical climates and America, and the difference was due to opinion rather than observation.[80] Thomas Mills read Clutterbuck with interest. Although not a part of the fever hospital establishment, Mills at the same time had the good fortune to get a temporary appointment at the Dublin Fever Hospital. This included a four-month and then an eight-month appointment due to an unforeseen increase in patient load. On his own, Mills instituted the new copious bloodletting treatment for fever. In the name of throwing off "the shackles of school authority" Mills published the mortality ratios of his patients in comparison with those of the regular physicians at the hospital who rarely used bloodletting. His mortality rates were 1/23 in 209 patients and 1/28 in 504 patients during these two periods. He compared them with advantage to the seven-year hospital rate of 1/11. The furious regular hospital physicians responded that the rates vary greatly from year to year and month to month depending on the type of epidemic. Thus Mills' figures were not comparable, i.e. not comparing similar epidemics.[81] Bateman of the London Fever Hospital joined in this criticism of Mills but was still clearly impressed by the figures admitting that Mills' views were "an approximation of the truth, the boundaries of which he may have slightly overstepped."[82] Whereas Clutterbuck and Mills saw essentially only one fever, "a general disease," the contagionists of the Fever Hospitals saw many types which were not comparable. William Stoker and the other regular physicians at the Dublin Fever Hospital showed that comparing mortality rates *at the same time* their rates were actually lower than Mills (1/12 to 1/11).[83] From the registers of the Royal Infirmary and Queensberry House Benjamin Welsh showed a steady drop in mortality following the institution of "free" bloodletting.[84] The response from the Royal Infirmary in Edinburgh in the person of William Brown was to total up all the cases, Stoker's, Mills', his own, those in the literature, etc. establishing an overall mortality in which individual differences were seen as relatively unimportant deviations.[85] Brown's view that a disease's mortality is "a pretty constant ratio" little affected by therapy made, it was said at the time, "very few converts among medical men."[86] Brown made reference to the annuities of insurance companies but his work was more an internal professional response to the Stoker-Mills controversy and hospital book-keeping than it was to extraneous influence of insurance companies on medicine.[87] Like insurance companies, and unlike the contagionist fever hospital physicians, and unlike modern clinicians one might add, Brown found reproducible mortality figures by lumping various fevers together rather than dividing them into types.

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[79] Sutton, *op. cit* (n. 72 above), pp. 16-17

[80] H. Clutterbuck, "Lectures on blood-letting" *Lond. Med Gaz.*, 1838, *ii*: 251-253, 274.

[81] Thomas Mills, *An Essay on the Utility of Blood-Letting in Fever* (London, 1813), Critical Analysis in: *Edin Med. J*, 1813, *9* 461.

[82] Bateman, *op. cit.* (n. 37 above), pp. 78-79, 119.

The tendency during the Bloodletting Revolution to lump rather than divide diseases was reflected in their loose terminology. From Pringle to Willan typhus had been a specific contagious disease with a highly specific cutaneous eruption resulting in a direct debility and occurring in a debilitated population. Bateman noted that typhus had become "the popular appellation of the fever of this country" largely because in his view Armstrong had "extended the definition" to include the inflammatory as well as other varieties of the disease.[88] Armstrong merely thought he had reinterpreted the specific contagious disease as due to inflammation. He was upset at the "abuse of the term" which was being used for any malignant symptoms and the last stage of any acute disease.[89] But Armstrong's "Common Typhus" in effect included intermittent, remittent, and continued, tropical Yellow Fever

[83] Francis Barker et al., "A letter addressed to the managing committee of the Fever Hospital, in Cork Street, by the permanent physicians to that establishment, on the treatment of fever by blood-letting," *Edin Med. J.*, 1814, *10* 358-365; and William Brown, *An Attempt to Estimate the Power of Medicine in Controlling Fever* (Edinburgh, 1818).

[84] Welsh, *op. cit.* (n. 31 above), pp. 169-170

[85] Brown, *op. cit.* (n. 83 above), pp. 45-65.

[86] Welsh, *op cit.* (n. 31 above), p. 150.

[87] Brown, *op cit* (n. 83 above), p. 48.

[88] Bateman, *op cit* (n 37 above), p. 27.

[89] Armstrong, *op cit.* (n. 76 above), p. vi.

and virtually all mal-arias.[90] Clutterbuck was most responsible for using the term *typhus* for a malignant last stage of any fever. He said he preferred to call it a "typhoid state." Clutterbuck shrugged off criticism as a "mere dispute over words" since in the last analysis to him it was all inflammation of the brain anyway.[91] The specificity of disease was so destroyed by the bloodletting revolution that debates of definition usually degenerated into questioning the degree of inflammation and how early a stage any "typhoid tendency" appears. In Houses of the Poor, the typhoid tendency appeared early, whereas in hospitals (which apparently had a somewhat higher class of population and better conditions) this did not occur and early bloodletting was in order. Nonetheless, the hospital patients might have a more severe latter stage.[92] One complication might be "typhoid pneumonia.'[93] Like the unitarian Clutterbuck, Mills thought "fever is of one kind" although he thought the clinical varieties depended on "localization" to the varieties of organs affected and not just the brain.[94] This led to clinical relativism rather than pathological specificity. Thus Welsh after examining the records of the Edinburgh Infirmary found "no greater diversity nor so great, was to be discovered between the two fevers, than we every day meet with between two individual cases of the disease itself."[95] That is why Nathan Smith's clinical specificity appears so "modern" by comparison.

Southwood Smith and the English sanitarians represented the last gasp of the bloodletting revolution, not only in their anti-contagionism but in the open advocacy of abandoning all nosological species, the "only real difference in disease being degree."[96] Even Alexander Tweedie saw "every intermediate gradation" between common fever and typhus. By way of contrast, Mills' opponent Stoker, one of the old regular physicians, had found petechiae in 386 of his 540 cases which suggests that his typhus may have been real typhus and certainly a much more serious disease than common fever.[97] At the very least, it shows a commitment to a specific, numerical clinical methodology before Louis by the old anti-bloodletters.

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[90] Boott, *op cit.* (n. 16 above), I, p 255.

[91] Clutterbuck, *op cit* (n. 45 above), pp. 241-242.

[92] W P. Alison, "Observations on the epidemic fever now prevalent among the lower orders in Edinburgh," *Edin Med & Surg J*, 1827, *28:* 250.

[93] (Review of) John Elliotson, *The Principles and Practice of Medicine* (London, 1838), in *Brit & For. Med Rev*, 1840, *9*· 490

[94] Mills' Critical Analysis, (n 67 above), p. 363.

[95] Welsh, *op. cit* (n 31 above), p. 250

[96] Southwood Smith, *A Treatise on Fever* (Philadelphia, 1831), pp. 41, 201

[97] Alexander Tweedie, "Fever," in *The Cyclopaedia of Practical Medicine* (Philadelphia, 1847), pp 163-164

The English Bloodletting Revolution antedated the Broussaisian revolution in France. As already indicated, it had ties to Rush's bloodletting revolution in America, but as also indicated, both had common roots in military and tropical colonial medicine. In his influential *A Defence of Blood-letting* (1796) Rush in a footnote noted "Dr. Gordon of Scotland and others have lately revived the lancet with success in Great Britain for fevers."[98] The year before, Alexander Gordon had published a treatise on puerperal fever in which he recommended doubling the amount of blood-let.[99] In this regard Gordon acknowledged he had been influenced by George Cleghorn's *Epidemical Diseases of Minorca* (1751). Significantly, Rush then came out with an annotated edition of Cleghorn's work.[100] But there were domestic roots to Gordon's method. Contrary to both his contemporaries and our modern sensibilities, John Leake, a member of the Royal College and physician at the Westminster Lying-In, recommended in his public lectures of 1770 "early and copious bleeding" in puerperal fever—a most debilitating disease in a most debilitated state already compounded by unavoidable blood-loss. Leake rationalized this away with that classic distinction common to Rush, Armstrong and the Bloodletting Revolution and, as we have seen, already present in Fernel. Thus, Leake argued that the apparent post-partem debility was not due to "nature exhausted" but to "nature oppressed,"—oppressed by an excess of arterial activity that required copious blood-letting. "Thus safety of the patient," concluded Leake, "depends upon a true distinction between *real* want of strength and that which is only *apparent*"[101] (my italics). Thomas Denman, physician man-midwife to the Middlesex-Hospital, like most physicians opposed free use of the lancet in puerperal fever, but changed his mind because of the obvious prominence of inflammation.[102] Gordon cited both Leake and Denman and thus was part of definite if obscure tradition of copious puerperal bloodletting, despite Cleghorn's influence. Gordon's method was propelled into the center of the Bloodletting Revolution when in 1815 the surgeon William Hey adopted it as if it were a great discovery.[103] Hey felt obliged to defend his priority after he saw Armstrong's early publication on puerperal fever by pointing out that the epidemic he describes at Leeds occurred before the one described by Armstrong, in Sutherland.[104]

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[98] Rush, *op cit.* (n 75 above), vol. 4, p 252 ftn

[99] Alexander Gordon, *A Treatise on the Epidemic Puerperal Fever of Aberdeen* (London, 1795), reprinted in Charles D Meigs, *The History, Pathology, and Treatment of Puerperal Fever and Crural Phlebitis* (Philadelphia, 1842), p 30.

[100] George Cleghorn, *Observations on the Epidemical Diseases of Minorca, From the Year 1744-1749* (London, 1751), with notes by Benjamin Rush (Philadelphia, 1809).

[101] John Leake, *Practical Observations on the Child-Bed Fever* (1st ed. 1772, 7th ed. London, 1792), II, pp 109, 106. See pp. 12-13 on 1770 lecture.

[102] Thomas Denman, *Essays on the Puerperal Fever, and on Puerperal Convulsions* (London, 1768), *An Introduction to the Practice of Midwifery* (London, 1795), 2 vols. II, pp. 496-497.

Hey's readers summed up the whole Bloodletting Revolution when they concluded from his work that symptoms have "misled the best observers into a notion of typhus."[105] The response to the success of Hey's treatise was twofold. It was admitted that the cautious attitude of earlier physicians had been mistaken in their fears of typhus, malignancy, putridity, debility, etc. in puerperal fever. It was also argued, however, that the disease epidemics themselves had "actually undergone a considerable alteration in their *type* and character"[106] (my italics). The term "change of type" was also used to explain how typhoid and typhus fevers in the British army could in the "sultry and relaxing" atmosphere of Bilboa become inflammatory, thus requiring bloodletting.[107] Hard core revolutionaries like Welsh, it will be recalled, believed the revolution mental, while moderates like Bateman attributed the change to disease type.[108]

The rise of bloodletting was followed by an even more rapid fall. The decline recapitulated the rise only in reverse. Just as the rise had been called a "revolution," so too the decline was called an "important revolution in our art."[109] Even as epidemics had been changing their *type* from putrid and typhoid-like to inflammatory, starting in 1828 fevers now gradually assumed "more and more of the typhoid type over the whole country" with a corresponding decline in benefit gained from bloodletting being noted by practitioners.[110] Whereas disease had been most inflammatory in 1822 when copious bloodletting was most beneficial, ever since that time one observer claimed blood would never spurt so far from the vein and there was more general debility.[111] Just as improved economic conditions had been cited to explain the lack of direct debility and excess of inflammatory activity, now massive unemployment in the woolen manufactories was cited to explain a debilitating destitution among the poor due to dietary insufficiency and predisposing to contagion. A rise in such cases was first noticed in the English towns with extensive manufactories.[112] In 1839 clinical experience at the Royal Infirmary and Poorhouse of Manchester showed a deficient diet leading to a "general debility" that could not tolerate bloodletting.[113] Just as previously "experience" in military hospitals had justified copious bloodletting, now it was noted in an army hospital that "the starved labourer" had his constitution broken by even the first evacuation.[114] And as the specific eruptive fevers had lost much of their significance, and sometimes even their identity during the revolution, now John Forbes singles out these cutaneous diseases in his manifesto against "heroic" medicine.

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[103] William Hey, *A Treatise on the Puerperal Fever, Illustrated by Cases, which Occurred in Leeds and its Vicinity, in the Years 1809-1812* (London, 1815), pp. x, 98-99

[104] *Ibid.*, p. xiii

[105] *Ibid.*, Review in *Edin Med. J.*, 1815, *11*. 505

[106] *Ibid.*, p. 501-502.

[107] Anthony T Thomson, *Elements of Materia Medica and Therapeutics* (London, 1832), I, p. 461.

[108] See notes 51 and 54 above.

[109] Discussion at Liverpool Medical Society, Feb 17, 1848, "Blood-letting as a remedial agent," *Lancet*, 848, *2*. 185.

[110] Christison, *op cit.* (n 11 above), p 233

[111] W Allison "Types of Fever and Bloodletting," *Brit Med J.*, 1864, *1*: 625

> To discontinue all active and powerful medication in the acute exanthemata and fevers of specific type, as small pox, measles, scarlatina, typhus, etc. until we obtain some evidence that the course of these diseases can be beneficially modified by remedies.[115]

Looked at in a broader perspective history was repeating itself, repeating the urban, hospital, military conditions of the pre-revolutionary eighteenth century and the medical doctrine of debility and specific contagion associated with them.

Nathan Smith anticipated John Forbes and modern attitudes because he held on to the older doctrines even while the Bloodletting Revolution was at its peak. This may have been easier to do in New England than old England.

Although the academic bloodletting controversy did not really even get underway until the 1850s, the end of the actual bloodletting revolution occurred in the 1840s and was signaled by Thomas Watson's influential and popular textbook of medicine which first appeared as lectures given at King's College 1836-7. Like Smith to Welch, Watson's text reads "like a fresh breeze" compared to his contemporaries. A "presentist" might even call it the first modern English textbook of medicine. Although bothered by a classification based on symptoms rather than etiology, Watson soberly turned to the sharp delineations of Willan while adding a richness of clinical observations from "the literature."[116] His text was not a system, but it prominently reflected the idea that fevers were due to specific human contagion and debilitating conditions such as nutritional deficiency were predisposing factors. In a gentlemanly fashion he dropped a few passing criticisms of Clutterbuck and Armstrong in such a way that one knew they hardly merited any mention at all in a modern text.[117] Yet Watson could not escape using the *change of type* explanation for the demise of bloodletting in England. Prior to 1831-2 Watson claimed it was necessary to draw blood in fever, but after that time it was necessary to abstain from taking any blood at all![118] Regardless of whether or not diseases actually did change their type in the 1820s and 30s as claimed by various authors, it is most significant that these arguments were stated at the time long before the academic style of "scientific" arguments were amassed against bloodletting in the second half of the nineteenth century, for when the "change of type" argument was mentioned then it was dismissed as the cheapest kind of post-hoc rationalization to avoid the scientific arguments that accounted for medical progress.

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[112] (Review of) W. P Alison, *On the Management of the Poor in Scotland* (Edinburgh, 1840), in *Brit & For. Med Rev.*, 1841, *1* 23-32.

[113] (Review of) Richard Baron, *An Inquiry into the Morbid Effects of Deficiency of Food* (London, 1839) in· *Brit & For Med. Rev* , 1839, *8*. 524-527

[114] Samuel Dickson, *Fallacy of the Art* (Edinburgh, 1836), pp. 62-64.

[115] John Forbes, *Brit & For Med Rev.*, 1846, *21* 263.

Finally, then, we must face the ever-present question as to whether both the rise and fall of bloodletting can be explained at least in part by a change in disease type or whether as Welsh (not Welch) maintained, the revolution took place in men's minds. The "change of type" explanation seems antiquated because our definition of disease entities today is even more rigorous than that of the early contagionists, so it appears "wishy-washy." Yet it is not unreasonable to suppose that drastic changes in urban economic conditions could affect the prevalence of such diseases as real typhus. Since there was a long tradition of not bleeding in these "malignant" eruptive fevers, the degree of bloodletting might well be affected by temporal situations. But the changes in practice were simply too great and too extensive to be accounted for by local epidemics alone.

The term "revolution" itself, which as we have seen was often used in discussion, may be significant. Rush, writing at the time of the French Revolution, felt the world was on the eve of a great "revolution." At the same time he felt English reluctance and French willingness to bleed to be

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[116] Thomas Watson, *Lectures on the Principles and Practice of Physic* (Philadelphia, 1844), pp 823-23ff

[117] *Ibid.*, p 857.

[118] *Ibid.*

"wholly political."[119] Armstrong, who was a great admirer of General Washington,[120] drew a parallel between "reformations in physick" and "reformations in politics." With remarkable insight he noted that both sometimes go too far and perhaps this had happened in abandoning the doctrine of debility.[121] Watson himself noted that Armstrong and other anticontagionists (bloodletters as we have seen) had "liberal opinions in politics and in religion."[122] Many of the bloodletters were derived from the demobilized military establishment, but here practice more than politics was probably the factor. Broussais' liberal opinions paralleled the English but were only a relatively late direct influence. Some insight into liberal medical thought, however, can be had from the Broussaisian L. J. Begin's criticism of "the autocratism of nature." According to this idea there was "a conflict between the morbific causes and the organic powers"—the latter being the autocrat who needs supportive therapy.[123] Begin considered this an absurd "medicine of symptoms," symptoms which were supported and stimulated—the very opposite of bloodletting, which was used to remove the oppressive cause. Thus, the modern pattern of thought that the body needs support in its fight against the contagious agent was apparently alien to liberal thought at that time. This would explain why therapy in general, and bloodletting in particular, and not simply contagionism, were so crucial an aspect of the revolution. The medical liberal was not so much concerned with the real bureaucracy, as has been suggested for French physicians, as he was with making the human body a symbolic autocracy attacking foreign invaders.[124]

Be that as it may, a "modern" pattern of thought about disease with an eighteenth-century background consisting of a specific contagious eruption, a specific natural history or course, with the predisposing role of debility, and the theoretical prejudice against bloodletting was reasserted in the nineteenth century following the Bloodletting Revolution. This thought pattern probably paved the way for the specific achievements of the second half of the nineteenth century. Schölein led the way with the fungal skin disease favus. Indeed, even by 1841 there was the fulfillment of the Koch-Henle postulates with favus by David Gruby, using potato slices as a culture media.[125] The decline of bloodletting *prior* to the specific achievements of modern scientific medicine, however, seems to indicate that the general pattern of thought is decisive in therapeutic judgments. After all, the first statistics showed that copious bloodletting was most effective in treating fevers, but these were quickly forgotten when people again started thinking about fevers in a different way.

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[119] Rush, *op. cit.* (n 75 above), pp 251, 255.

[120] Boott, *op. cit.* (n. 7 above), p 60.

[121] Armstrong, *op. cit.* (n 76 above), p 123.

[122] Watson, *op. cit* (n 116 above), p. 850

[123] L J Begin, *Treatise on Therapeutics*, vol 1 (New York, 1829), p 26 Both Watson and Jacob Henle symbolized the foreign agent by a thorn in the finger at about the same time—see Peter Niebyl, "The Helmontian thorn," *Bull Hist Med* , 1971, *45*. 570-595.

[124] Ackerknecht has suggested that medical liberals were concerned with the real bureaucracy's interference with free trade, see Erwin H Ackerknecht, "Anti-contagionism between 1821 and 1867," *Bull Hist Med.*, 1948, *22*: 562-593. See also Ackerknecht, "Broussais or a forgotten medical revolution," *Bull Hist. Med* , 1953, *27*. 320-343.

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[125] John W. Rippon, *Medical Mycology* (Philadelphia, W B. Saunders, 1974), p 97.

OBSERVATIONS ON THE CHRONOLOGY OF THE GALENIC CORPUS*

DONALD W PETERSON

There are still problems in regard to when and in what order Galen wrote his numerous books. They often stem, not so much from a lack of pertinent data, as from an abundance of seemingly contradictory evidence. Textual errors are certainly to blame; so, too, perhaps, is the author's habit of revising works already completed. Recent gains have been due largely to the prodigious efforts of Johannes Ilberg[1] and to certain modifications of Ilberg's scheme formulated by Kurt Bardong.[2] The present author's interest in the matter arose in connection with his doctoral analysis of Galen's *Therapeutics to Glaucon,*[3] a text that Bardong did not mention, but that Ilberg placed near the middle of a chronological list of books he believed Galen to have written between 169 and 180 A.D.[4] This paper is meant to show that *Therapeutics to Glaucon* can be dated more precisely and that the years currently assigned to a number of other Galenic works are also in need of revision.

How late could *Therapeutics to Glaucon* have been written? Since it is cited in *Crises*[5] and *De libris propriis,*[6] it seems to have been finished by the time these two texts were undertaken. Although Bardong rightly noted that

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[1] Johannes Ilberg, "Ueber die Schriftstellerei des Klaudios Galenos," *Rheinisches Museum für Philologie,* n.s. 1889, *44*:207-239; 1892, *47*· 489-514, 1896, *51*· 165-196; 1897, *52*· 591-623

[2] Kurt Bardong, "Beiträge zur Hippokrates- und Galenforschung," *Nachrichten von der Akademie der Wissenschaften in Gottingen,* Philologisch-historische Klasse, 1942, no. 7, pp 577-640. See also Bardong's introduction to his edition of Galen's *De causis procatarcticis, Corpus medicorum graecorum,* Supplement 2 (Leipzig: B.G. Teubner, 1937).

[3] See asterisked note above.

[4] Ilberg, "Schriftstelleri" (1896).

[5] *ταῦτα μὲν οὖν ἁπάντων ἐστὶ κοινὰ σημεῖα τῶν ἐφημέρων πυρετῶν, ἴδια δ' ἑκάστου ὅπερ εἴρηταί μοι κἀν τῷ πρώτῳ τῶν πρὸς Γλαύκωνα θεραπευτικῶν* *De crisibus* 2 13 (Alexanderson 162.1-3 = Kühn 9· 696.15-17). This remark is followed shortly by a long passage that corresponds almost verbatim to a portion of *Therapeutics to Glaucon* (i.e. Kühn 11: 10.16-14 9).

[6] In *De libris propriis* 4 (*Scripta minora* 2 109 5 f) Galen lists *δύο* . *τῶν πρὸς Γλαύκωνα θεραπευτικῶν* among his therapeutic works.

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Crises was much earlier than *De libris propriis*,[7] his date for the former is otherwise unacceptable.

Galen recalls in a well-known passage in *Prognosis to Epigenes* that he wrote many works, both philosophical and medical, when Marcus Aurelius was away from Rome during the war with the Germans—that is, between 169 A.D. and November 176. When the Emperor returned, Galen gave these books to his friends; he would not have done so, had he known that they would then be given out to the unworthy.[8] The books contained "the whole theory of prognosis by the pulses and by the other prognostic signs." If the reader (i.e. Epigenes) wished to learn from them a certain point, he could do so by reading *Crises*.[9]

It is clear from these remarks that *Crises* was written before Marcus Aurelius returned to Rome in November 176 and that *Prognosis to Epigenes* was undertaken after he came back. According to Bardong, however, Galen elsewhere tells Epigenes that *Crises* was one of three texts he had completed only recently (*ἔναγχος*). Therefore, Bardong argues, *Crises* must have been written just before November 176 and *Prognosis to Epigenes* just after.[10]

Bardong was aware, for the most part, of certain difficulties arising from his conclusion. For example, he noted in the passage summarized above that between November 176 and the writing of *Prognòsis to Epigenes* the "many books" Galen completed during the Emperor's absence were finally released; first they were presented to Galen's friends, and then somehow they were given out (perhaps "published"—*ἐκδοθήσασθαι*) to the unworthy.[11] His problem, therefore, in setting a date for *Prognosis to Epigenes* was that an event earlier than November 176 must still have been "recent" when the text was written, and yet the text must have followed the dissemination of Galen's work, which did not start until after November 176. Bardong concluded that Galen wrote *Prognosis to Epigenes* at the beginning of 177 ("Anfang 177").[12] But whether Galen's numerous books could have found their way to the unworthy in so brief an interval as November 176—Anfang 177 is of course open to question.

A second problem has to do with other chronological data that Galen provided. He told Epigenes that "fifteen years" had passed since he wrote on various matters at the request of a certain Boethos.[13] On the basis of this remark Ilberg concluded, not quite correctly, that *Prognosis to Epigenes* was written in 178.[14] The fifteenth year from the event in question was completed in the last half of 178, and therefore either 178 or early 179 is possible.[15] In defense of Anfang 177 as a date for the text, Bardong argued, with calculations of his own that were slightly forced, that Galen must have exaggerated the interval.[16] If Anfang 177 is correct, it would appear that Galen said fifteen years when in fact not even fourteen had passed.

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[7] For the relatively late date of *De libris propriis* see Kurt Bardong, "Beitrage," especially p 640. The date of *De crisibus* is discussed below.

[8] *De praenotione ad Posthumum* 9 (Kuhn 14 650 15-651-2).

[9] *Ibid* (Kühn 14 651.5-12)

[10] Bardong, "Beitrage," p 609 f

[11] See note 8 above

[12] Bardong, "Beitrage," p. 610

These two difficulties require a close examination of the passage wherein Galen is alleged to tell Epigenes that he recently wrote *Crises*. It appears at the end of a chapter devoted mainly to the illness of the Emperor's son Commodus. Commodus suffered an attack of fever and tonsillitis when he was between eight and fourteen years old. Around the third hour of the third day of his illness, Annia Faustina arrived to find the boy eating in the company of his foster father, Peitholaos, and Galen. Aware that the fever had begun in the eighth hour of the first day, she seems to have found it remarkable that Commodus had been allowed to eat before the eighth hour of the third day had passed. But Peitholaos told her that this, and still another, unorthodox measure had been prescribed by Galen; otherwise, he said (perhaps ironically), they would have waited out the Thessalian three-day period.[17]

In a little while Annia Faustina led Galen away by the hand and began to make fun (*παίζουσα*) of one of the Methodist physicians who had accompanied her on her visit to Commodus. She said in effect that Galen had in many previous instances contested with deeds rather than words the Methodist practice of observing the three-day period, and that now his management of Commodus proved him right. Furthermore, Galen had acted in this instance with the approval of Peitholaos, who had tested Galen beforehand, and who was extremely acute in such matters.[18]

The remainder of the passage must be quoted in the original (for a translation into English see footnote 19 below). *ἡ μὲν τοιαῦτα ἔλεγε,* Galen continues, *διαβαδίζουσα μέχρι τοῦ ὀχήματος· ἐγὼ δὲ μελλούσης αὐτῆς ἐπιβαίνειν αὐτοῦ χωριζόμενος εἶπον· ἐποίησάς με πολὺ μᾶλλον ἢ πρόσθεν ὑπὸ τῶν ἰατρῶν μισεῖσθαι. χωρισθείς τε τῷ Πειθολάῳ διηγούμην αὐτός. καὶ μέντοι καὶ ὅτι διὰ τοὺς τοιούτους ἰατροὺς ἔναγχος ἔγραψα τὰς πραγματείας, μίαν μὲν τὴν περὶ τὰς διαφορὰς τῶν πυρετῶν, ἑτέραν δὲ τὴν περὶ τῶν κρισίμων ἡμερῶν, καὶ τρίτην τὴν περὶ τῶν κρίσεων, ἐπιδεικνὺς ὑφ᾿ Ἱπποκράτους γεγράφθαι τὴν θεωρίαν, ἐφ᾿ ἧς ἄν τις προγινώσκοι τὰ γενησόμενα περὶ τοὺς ἀῤῥώστους. οἱ δ᾿ οὕτως εἰσὶν ἀφυεῖς, ὥστε μηδὲ μετὰ τῆς ἐμῆς ἐξηγήσεως δύνασθαι αὐτὰ μαθεῖν. αὐτὸς γὰρ σὺ, Ἐπίγενες φίλτατε, τὰ γεγραμμένα μοι περὶ τούτων ἁπάντων ὧν ἐθεάσω με προλέγοντα γινώσκεις ἐπιδεδειγμένα πρὸς Ἱπποκράτους εἰρῆσθαι κτλ.*[19]

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[13] *De praenotione ad Posthumum* 5 (Kuhn 14. 630.5-11)

[14] Johannes Ilberg, "Aus Galens Praxis· Ein Kulturbild aus der romischen Kaiserzeit," *Neue Jahrbucher für das klassische Altertum*, 1905, *15* 284-289, including n.2, p. 289.

[15] Bardong ("Beitrage," p 610 n 1) rightly implies that fifteen years must be counted from the summer of 163

[16] Bardong (*ibid* p. 610, n.1) implies that "Anfang 177" lies somewhere in the fifteenth year after the summer of 163, when in fact it is in the fourteenth

[17] *De praenotione ad Posthumum* 12 (Kühn 14: 661 15-663.10). Galen described Annia Faustina as *συγγενὴς οὖσα τῶ αὐτοκράτορι ἔγγιστα*, which suggests Annia Fundiana Faustina, Marcus Aurelius's cousin. See Anthony Birley, *Marcus Aurelius* (Boston-Toronto: Little, Brown and Co., 1966), Appendix 2.

[18] *De praenotione ad Posthumum* 12 (Kühn 14 663.10-664.10).

The question here is, does Galen (as Bardong assumes) tell Epigenes that he recently wrote *Crises*, or is he still merely repeating what he told Peitholaos? As the passage stands, it is difficult to say just where Galen begins to speak directly to Epigenes. But he clearly does not do so until well after the phrase beginning *καὶ μέντοι καὶ ὅτι* ("and furthermore, that . . ."). Such a transition could have been continued with *καὶ μέντοι χαί* ("and furthermore"), but it could not have been initiated with that phrase.[20] Moreover, the subsequent *ὅτι* ("that") cannot be accounted for if Galen is assumed here to begin to address Epigenes directly; and the editor, who seems to have assumed just that, has been obliged to ignore *ὅτι* ("that") in his Latin version.[21] The Latin translation, and the editor's full stop after *αὐτός* ("myself"), have perhaps contributed to the present confusion.

There is other evidence that Galen did not make a transition to Epigenes at the point in question, but was still repeating what he told Peitholaos. In place of the required direct object for the preceding *διηγούμην* ("I told all about") there is only the incongruous *αὐτός* ("I myself"). The lack of a direct object for *διηγούμην* indicates either that *αὐτός* is erroneous[22] or that a lacuna exists just after *αὐτός*. When a lacuna is indicated, the passage reads: "Having parted, I myself(?) told Peitholaos all about . . . and furthermore, that on account of such physicians I recently wrote etc." Evidently, the remark about *Crises* was addressed to Peitholaos, and not directly to Epigenes. One discovers that such an interpretation is not new. A somewhat free Renaissance translation reads: "Me magis abiens dixi quam antea medicis invisum reddidisti. Digressus ad Pitholaum me contuli, cui et hoc idem exposui, et ob huiuscemodi medicorum invidiam, me nuper libros tres composuisse, unum de febrium differentiis, alterum de diebus decretoriis, tertium de crisi."[23]

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[19] Without changing the editor's punctuation or attempting to correct the obvious errors, the passage reads. "She was saying the above while going over to her carriage. As she was about to get on it, I said, upon parting, "You have caused me to be hated by the physicians much more than before " Having parted, I myself *(sic)* told Peitholaos all about *(sic)*. And furthermore, that on account of such physicians I recently wrote a work on the differences of fevers, another on critical days, and a third on crises, showing that the theory by which one would have foreknowledge of what will happen to patients was written down by Hippocrates. But they are so dull as to be incapable of learning such things, even with my own explanation. For you, yourself, dear Epigenes, know what I have written, etc "

[20] For two examples of Galen's use of *καὶ μέντοι καὶ* in the same text see chapter 3 (Kuhn 14: 616.1) and chapter 12 (Kühn 14: 663.8). Numerous others can be found by consulting the appropriate indices of the *Corpus medicorum graecorum*

[21] Which is: . . ac digressus Pitholao ipse narravi. Quinetiam propter hujusmodi medicos opera conscripsi, unum de febrium differentiis, etc.

The proper reading of this passage removes the basis for the date Bardong has assigned to *Prognosis to Epigenes* as well as the difficulties arising from his conclusion. Ilberg was almost right: The text was written in 178 or early 179. The alleged date of *Crises* must also be revised. Since that book was certainly written before November 176, and since Bardong believed that the completion of it was a recent event when Galen wrote *Prognosis to Epigenes,* Bardong assumed, as noted above, that *Crises* ought to be placed shortly before November 176. But Galen could not have told *Peitholaos* that he recently wrote *Crises* later than 19 May 175 when Commodus left Rome to join his father in the north.[24] Bardong to the contrary, *De diebus decretoriis* and *De febrium differentiis* must also be placed before Commodus' departure.[25]

Since 19 May 175 is a *terminus ante quem* of *Crises,* so must *Therapeutics to Glaucon* have been written before that date. There is, however, a remark in the latter which allows the text to be moved back still further. Galen reminds his correspondent that "this year" (*τῆτες*) had been quite hot and dry between the rising of Sirius and Arcturus.[26] Since the rising of Arcturus was then observed, in the latitude of Rome, around 21 September, Galen was evidently writing between late summer and the end of the year.[27] Regardless of whether he followed Roman custom or that of his homeland with respect to when he considered the year to end, he must have been occupied with *Therapeutics to Glaucon* in the autumn.[28] Consequently, the text cannot be dated later than the end of 174.

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[22] One might postulate *αὐτούς* ("them"), referring to the Methodist physicians mentioned before, and just after, the phrase in question.

[23] *Galen· Omnia quae extant opera . . , ex secunda Juntarum editione . . , 4.* 219-220.

[24] The date of Commodus's departure is from Birley, *Marcus Aurelius,* p. 258 Commodus did not return to Rome before his father did in November 176

[25] See note 19 above

[26] *ὅταν γε ἀκριβῶς ᾖ θερμὴ καὶ ξηρὰ* [sc *ἡ τοῦ περιέχοντος ἀέρος κρᾶσις*] *καθάπερ τῆτες ἐν τῷ μεταξὺ κυνὸς ἀνατολῆς καὶ ἀρκτούρου Ad Glauconem de medendi methodo* 1.15 (Kühn 11 44.5).

[27] Elias J. Bickerman, *Chronology of the Ancient World* (London. Thames & Hudson, 1968), p. 185.

To find the earliest possible date for *Therapeutics to Glaucon* it will be necessary to review the citations in the text itself. The works, of which portions, at least, Galen had already completed when he wrote *Therapeutics to Glaucon* include, among others, *De temperamentis*,[29] *De facultatibus naturalibus*,[30] *De anatomicis administrationibus*,[31] and *De simplicium medicamentorum temperamentis ac facultatibus*.[32]

On three occasions Galen also cites *τοῖς περὶ σφυγμῶν*;[33] by that he undoubtedly means some or all of the sixteen books on pulses he elsewhere refers to collectively as *ἡ περὶ τῶν σφυγμῶν πραγματεία*.[34] There is in addition a partial description of "one book . . . on the so-called rheumatic [*sc.* diatheses]."[35] Ilberg is probably correct in assuming that it refers to a now lost *περὶ τῶν ῥευματικῶν διαθέσεων λόγος*.[36] For the identity of a book wherein the recognition of tertian, quartan, quotidian, and continuous fevers had already been discussed "more clearly and at greater length,"[37] Daremberg has suggested *Crises* Book 2 and *De febrium differentiis.*[38] Another likely possibility is *De typis.*[39] Finally, there is a vague reference to a group of books already written which may include *De usu partium.*[40]

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[28] A review of Galen's *In Hippocratis epidemiarum librum I commentaria* 1.1 (*Corpus medicorum graecorum* 5,10,1: 11.14-17.2) will show the danger of assuming, without specific proof, that Galen followed Roman custom in his notion of when the year ended. Of special interest is his reference to solar calendars in use *ἐπί τε ʽΡωμαίων καὶ Μακεδόνων ʼΑσιανῶν τε τῶν ἡμετέρων καὶ πολλῶν ἄλλων ἐθνῶν* (*ibid.* 16.23-25). According to M P. Nilsson ("Neujahr," Pauly-Wissowa 17·151.18 ff.), after the Julian calendar spread to the East in the reign of Augustus, the division of the year was still not uniform. In Asia Minor the year began on 23 September, Augustus's birthday In either case, 23 September or 1 January, the year 175 (by current reckoning) is ruled out.

[29] *ἀλλὰ πάντων τούτων τὰ μὲν ἐν τοῖς περὶ σφυγμῶν, τὰ δὲ ἐν τοῖς περὶ κράσεων διώρισται. Ad Glauconem* 1.1 (Kuhn 11· 5.11).

[30] *ὑπὲρ οὗ λέλεκται καὶ ἐν τοῖς κατὰ φυσικῶν δυνάμεων ὑπομνήμασι. ibid.* 2 12 (Kuhn 11. 139.9).

[31] *ὧν χωρίων τὴν φύσιν ἐν ταῖς ἀνατομικαῖς ἔμαθες ἐγχειρήσεσιν ibid.* 2 8 (Kühn 11. 112 7).

[32] *περὶ δὲ τοῦ . ὡς οἶσθα, ἐν τῇ περὶ τῆς τῶν ἁπλῶν φαρμάκων δυνάμεως ἐπεσκεψάμεθα πραγματείᾳ. ibid.* 2.4 (Kühn 11 99.15). See also *ibid.* 2.6 (Kühn 11. 104 *ult*) and 2.9 (Kühn 11. 118.4), both of which indicate that this work was completed at least through the fifth book

[33] See n 24 above and also *Ad Glauconem* 1.2 (Kühn 11 10.*ult*) and *ibid.* 1 15 (Kühn 11 59.8).

[34] That is, the four books *De pulsuum differentiis*, the four *De dignoscendis pulsibus*, the four *De causis pulsuum*, and the four *De praesagitione ex pulsibus*. All of these are on occasion referred to collectively as *ἡ περὶ τῶν σφυγμῶν πραγματεία, τὰ περὶ σφυγμῶν*, or *τὰ περὶ σφυγμῶν ὑπομνήματα*. See Ilberg, "Schriftstellerei" (1889), p. 219.

[35] *ὥσπερ αἱ ῥευματικαὶ καλούμεναι, περὶ ὧν ἓν ἡμῖν, ὡς οἶσθα, γέγραπται βιβλίον Ad Glauconem* 2.4 (Kühn 11: 100.11).

[36] Ilberg, "Schriftstellerei" (1896), p. 169. Ilberg's conclusion is not without difficulties In *De febrium differentiis* 2.13 (Kühn 7. 380 15) Galen says· *ὅπως δὲ γίνεται τοῦτο, τάχ᾽ ἂν εὕροιμεν, εἰ περὶ γενέσεως φλεγμονῆς καὶ τῶν καλουμένων ῥευματικῶν διαθέσεων ἀναμνησθείημεν, ὅσα δι᾽ ἑτέρων ἡμῖν ἐν τῷ περὶ ἀνωμάλου δυσκρασίας ἀποδέδεικται.* If this passage is not corrupt, it means that Galen's *De inaequali intemperie* (Kühn 7 733-752) is at least partly about the "so-called" rheumatic diatheses, even though the expression itself is not once used therein On the other hand, Galen also says (*De febrium differentiis* 2 14, Kuhn 7 383.1) that he "wrote in particular (*ἰδίᾳ μοι γέγραπται*) about the so-called rheumatic diathesis," and

At the very end of the text Galen tells Glaucon of books he intends to complete, saying: "I think the aforesaid to be right for you, preparing [as you are] for a long stay abroad. If, as I said earlier,[41] I also do the work on drugs according to genus and affected places, you will have that, too, when you return. There is, moreover, another large work, requested by friends, on the whole therapeutic method. If you are kept away for a longer time, I shall not hesitate to send you each commentary I write."[42]

Of the first pair of books mentioned in this passage—*De compositione medicamentorum per genera* and *De compositione medicamentorum secundum locos*—a portion had already been completed.[43] The completed

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later (*Ibid.* 2 18, Kühn 7· 402.2) he refers specifically to *ὁ περὶ τῶν ῥευματικῶν διαθέσεων λόγος.* Furthermore, Galen describes how the book in question begins (*Ad Glauconem* 2.4, Kuhn 11: 100.12), and this description does not fit *De inaequali intemperie.* It does, however, fit very well an opening passage in Oribasius's long chapter *περὶ ῥευματικῆς διαθέσεως* (*Collectionum medicarum* 44.2, *Corpus medicorum graecorum,* vol. 6.2.1: p. 110.6 ff). Ilberg ("Schriftstellerei" (1896), p.169, n.3) has already suggested that the chapter may contain fragments of Galen's lost work of the same name. Both early and modern scholiasts claim to have found a surviving Galenic source for the passage in question, but the correspondence between the two is by no means close (see CMG, vol 6.2.1: p 110 *apparatus fontium,* n.6 and n 6-22)

[37] *Ad Glauconem* 1.4 (Kühn 11. 17. *paen*).

[38] *Oeuvres anatomiques, physiologiques et médicales de Galien,* trans. by Charles Daremberg (Paris Baillière, 1854-1856), vol 2, p.715.

[39] Galen's *περὶ ἰατρικῶν ὀνομάτων* also contains references to a work on fevers. See Galen, *Ueber die medizinischen Namen,* ed and trans Max Meyerhof and Joseph Schacht (Berlin Akademie der Wissenschaften, 1931) p. 6 29 (= p 16.32-34 of the accompanying German translation) and p. 13 16-21 (= p 26.5-16). Of the two such citations, Ilberg ignored one but demonstrated (*ibid.,* p 26 of the German translation, n.1) that the other could be taken to refer to *De typis.* Note that the book in question is called by Galen "our book on fevers"—"kitābunā fī'l-hummayāt." Like *De crisibus* and *De febrium differentiis, De typis* also lists signs of tertian (chapter 4, Kühn 7: 465.11-13 and 466.9-469.*paen.*), quartan (*ibid.,* Kühn 7. 465.14-15 and 469.*paen* -470-14 and *passim* to the end of the work), and quotidian (*ibid.,* Kühn 7: 465.9-11 and 465.*paen.*-466 9 ff.) fevers.

[40] *ἀλλ' ἐμοὶ μὲν δοκεῖ γνώμης τε πάνυ λεπτῆς ἡ τοιαύτη δεῖσθαι διάγνωσις, ἐμπειρίας τε τῆς φύσεως τῶν μορίων, ἣν ἐξ ἀνατομῆς τε ἅμα καὶ ἀκριβοῦς ἐπιστήμης ἐνεργειῶν τε καὶ χρειῶν ποριζόμεθα. περὶ μὲν δὴ τούτων ἐν ἑτέραις εἴρηται πραγματείαις* *Ad Glauconem* 2.1 (Kühn 11. 78 1).

[41] Galen is referring to *Ad Glauconem* 2.9 (Kühn 11: 124 1-10). In the earlier passage he seems to imply that only *De compositione medicamentorum secundum locos* remains to be written. Whether or not that is correct, it is in any case clear that a portion of *De compositione medicamentorum per genera* had already been completed.

[42] *Ibid* 2 13 (Kühn 11: 145 12 ff.).

[43] See reference in note 41 above with special attention to the phrase *ὡς ἐν τοῖς περὶ συνθέσεως φαρμάκων ὑπομνήμασι λέλεκται* (lines 5-6). Cf. *καὶ ἄλλα δὲ μυρία φάρμακα* . . *ἔνεστί σοι συντιθέναι, ὧν τὴν μέθοδον*

section, however, was subsequently destroyed, at least in part,[44] and how much of it, if any, can be seen in the existing work bearing that title is therefore problematical. As for the "large work . . . on the whole therapeutic method," Ilberg has argued, unconvincingly, that it must already have been in progress.[45] It is just as likely that Galen merely had in mind to write *De methodo medendi* at that time.

The few remaining citations are also of a somewhat equivocal nature. When Galen says that a question "is discussed" or "is being discussed" (*λέγεται*) elsewhere, or that he "is defining" or "defines" (*διοριζόμεθα*) a matter in another book, it does not necessarily mean that the other book has been completed.[46] A *περὶ παθῶν λόγος* is cited in this fashion; the title is probably a collective one for Galen's four works on the differences and causes of diseases and symptoms.[47] There are two similar references, one certainly to *De diebus decretoriis*, the other possibly to the same work, or to *Crises*, or to both.[48]

τῆς συνθέσεως ἐν τοῖς περὶ φαρμάκων μεμάθηκας (*ibid* , 2.2, Kuhn 11: 81.7-10) and *ἀλλὰ σύ γε τούτων ἤδη τῶν φαρμάκων, ὧν ἐμνημόνευσα κατὰ τὸ βιβλίον τοῦτο, τὰς συμμετρίας τε καὶ σκευασίας ἔχεις παρ' ἡμῖν λαβών* (*ibid* 2 9, Kühn 11· 124.10). See finally *ibid* 2.7 (Kühn 11:110.16-18) and *ibid*. 2 10 (Kühn 11 133.6). In at least one other work Galen has referred to his books on simple and compound drugs collectively as *ἡ περὶ τῶν φαρμάκων πραγματεία* (see *De methodo medendi* 14.4, Kühn 10: 955.2).

There are also references to *φαρμακίτισι βιβλίοις* (*Ad Glauconem* 2.3, Kühn 11: 87.2, and *ibid*. 2.7, Kühn 11. 108.*paen*.) whereby books by other physicians are probably intended. Cf *De compositione medicamentorum secundum locos* 7.1 (Kühn 13: 3.6) where we find *ὅτι δὲ πολλή τις ἐστιν ἡ διαφορὰ τῶν γεγραμμένων ἐν ταῖς φαρμακίτισι βιβλίοις ὑπὸ τῶν ἰατρῶν.*

[44] Ilberg, "Schriftstellerei" (1889) pp. 226-227, assumes that the early version of *περὶ συνθέσεως φαρμάκων* was two books long and that both were destroyed Galen, however, does not say precisely that At the very beginning of *De compositione medicamentorum per genera* (Kühn 13 362) he notes *Ἤδη μοι καὶ πρόσθεν ἐγέγραπτο πραγματεία, δυοῖν μὲν ἐξ αὐτῆς τῶν πρώτων βιβλίων ἐκδοθέντων, ἐγκαταλειφθέντων δὲ ἐν τῇ κατὰ τὴν ἱερὰν ὁδὸν ἀποθήκῃ μετὰ τῶν ἄλλων, ἡνίκα τὸ τῆς Εἰρήνης τέμενος ὅλον ἐκαύθη κτλ.* That is, Galen once wrote a work on compound drugs, of which *two* books were published, deposited near the Temple of Peace, and then destroyed.

[45] Ilberg, "Schriftstellerei" (1896) p 180, argues that if *De methodo medendi* were not already in progress, but merely anticipated, we should expect to find in the passage just translated above *ἔσται δὲ καὶ ἄλλη . . . πραγματεία* instead of *ἔστι δὲ καὶ ἄλλη . . . πραγματεία.* But *ἔστι* is consistent with either possibility. Galen merely says that his friends have asked him to write another book and implies that Glaucon will eventually have it

[46] See Bardong, "Beitrage," p. 605, and Ilberg, "Schriftstellerei" (1892) p. 502, n.3.

[47] *ὥσπερ γε καὶ τῶν παρὰ φύσιν ἁπάντων ὅσαι κατ' εἴδη τε καὶ γένη διαφοραὶ τυγχάνουσιν οὖσαι, πάσας ἐν τῷ περὶ παθῶν διοριζόμεθα λόγῳ* *Ad Glauconem* 1 1 (Kuhn 11 5 13) In at least one other passage *De symptomatum differentiis* 1, Kühn 7 42) Galen calls all of these works a single *λόγος*.

[48] *ἣν* [*sc*. *φύσιν*] *ἐν τοῖς περὶ κρισίμων sc ἡμερῶν διοριζόμεθα* *Ad Glauconem* 1 16 (Kühn 11 66.7). Galen also says *λέγεται δ' ἐπὶ πλέον μὲν ἑτέρωθι πάντα,* where *πάντα* means how to recognize a *σῶμα ταραττόμενον ὑπὸ φύσεως παρασκευαζομένης ἐπί τε τὰς ἄλλας ἐκκρίσεις καὶ οὐχ ἥκιστα τὰς δι' ἐμέτων τε καὶ αἱμορραγίας, ὧν ἀχώριστόν ἐστι σημεῖον ἡ κεφαλαλγία* (*ibid*. 1 16, Kuhn 11 64.16) Broadly speaking, that is a pervasive theme of *De diebus decretoriis*, especially Books 1 and 2 It is dealt with more specifically in *De crisibus*, especially Book 3.

Since all of the works mentioned were written near the end of 169 or thereafter,[49] that date is a *terminus post quem* for *Therapeutics to Glaucon* as well. But it seems that between 169 and the time he began *Therapeutics to Glaucon* Galen wrote enough to fill some four thousand or more pages in the Kühn edition.[50] Consequently, the text could not have been completed before the early 170s.

There are a few difficulties that remain to be noted in regard to the new dates proposed above. Since Galen evidently had nothing of *De methodo medendi* he was able (or willing) to give Glaucon before the latter's departure, it is quite remarkable that at the same time he should have referred to a *De diebus decretoriis* either completed or anticipated.[51] According to Bardong, there were some nine works or portions thereof written between *De diebus decretoriis* and *De methodo medendi* 1-6 (see table 1).

Here again, however, Bardong proves to be substantially in error. For example, the most recent of the nine works he believes to separate *De diebus decretoriis* and *De methodo medendi* is Galen's *In Hippocratis aphorismos*. Bardong placed the latter before *Crises* (and hence before *De diebus decretoriis*) on the basis of a clearly suspicious passage in the Kühn edition of *Crises* that Bardong reproduced as follows: *ἐπεὶ τοίνυν ἀποδέδειϰται μὲν ἐν τῷ πϱώτῳ λόγῳ τῶν Ἱπποϰϱάτους ἀφοϱισμῶν ἡμῶν ἐξηγουμένων.* (literally, "Since it has been shown in the first discourse of the Aphorisms of Hippocrates we having commented.")[52] Does the statement indeed refer to Galen's formal commentary on the *Aphorisms* of Hippocrates? Or is it a reference to the *Aphorisms* themselves, or to something entirely different? One of several questions that arise is whether the full stop is in the right place. The beginning of the "next sentence," *ὡς ϰἀξ αὐτῆς τοῦ νοσήματος τής φύσεως ϰτλ.* ("That from the nature of the disease itself etc."), is not reassuring. Fortunately, there is in this case no need to speculate further because a critical edition of *Crises* is now available. Alexanderson's version of the passage in question reads: *ἐπεὶ τοίνυν ἀποδέδειϰται μὲν ἐν τῷ πϱώτῳ λόγῳ τὸν Ἱπποϰϱάτους ἀφοϱισμὸν ἡμῶν ἐξηγουμένων ὡς ϰἀξ αὐτῆς τοῦ νοσήματος τῆς φύσεως οὐ σμιϰϱὰν πϱόγνωσιν ἔνεστι λαβεῖν εἰς τὰ*

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[49] Bardong, "Beiträge," especially pp. 633-635

[50] This figure is based on the assumption that Galen had completed no more than five books of *De simplicium medicamentorum temperamentis ac facultatibus* (see above) and that Bardong ("Beitrage," especially pp. 633-635) has correctly identified what Galen wrote before that work, but after 168 A.D

[51] See note 48 above.

[52] "Beitrage," pp 635-637. The passage is from *De crisibus* 3 8 (Kühn 9· 671.11)

TABLE 1

Chronological Order of Certain Galenic Works According to Bardong*

	De methodo medendi 1-6
"etwa Anfang 175"	*De difficultate respirationis*
	De sanitate tuenda 1-3
	De marcore
	De plenitudine
	De sanitate tuenda 4-5
	In Hippocratis librum de fracturis
	In Hippocratis de articulis librum
	In Hippocratis de ulceribus librum
	In Hippocratis de capitis vulneribus librum
	In Hippocratis aphorismos
	De diebus decretoriis
	Crises
	De febrium differentiis
"Nov. 176"**	
	De morborum temporibus
	In Hippocratis epidemiarum libros 1
"Anfang 177"**	*Prognosis to Epigenes*

* Kurt Bardong, "Beitrage zur Hippokrates- und Galenforschung," especially pp. 635-637.

** It was demonstrated above that "Anfang 177" in this table ought to read "178 or early 179" and that "Nov. 176" must be changed to "19 May 175." For additional corrections see below.

μέλλοντα, κτλ. ("Since it was shown in the first discourse, in commenting on the aphorism of Hippocrates, that it is possible to gain no small foreknowledge of future events from the nature of the disease itself, etc.").[53] The remark appears in Book 3 of *Crises,* and the reference to the "first discourse" is of course to Book 1 of *Crises,* where the appropriate passages are easily found to confirm Alexanderson's reading.[54]

Without evidence that Galen's *In Hippocratis aphorismos* was written before *Crises,* there is no good reason to suppose that it was completed before *De diebus decretoriis.* Indeed, it is doubtful whether there is a reference in *De diebus decretoriis* to any formal "Hippocratic" commentary whatsoever.[55]

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[53] *De crisibus* 3.8 (Alexanderson 145.17).

[54] Galen explains *τον Ἱπποκράτους ἀφορισμόν* in the aforesaid fashion in *De crisibus* 1.4 (Alexanderson 75.3-23 = Kühn 9: 560-561). For a subsequent point referred to see ibid. 2.2 (Alexanderson 127 16 = Kühn 9· 642), which begins· *Λεκτέον οὖν ἤδη πῶς ἄν τις κτλ*

[55] In *De diebus decretoriis* 1.1 (Kühn 9: 770.*ult*) we read: *κατὰ μέρος δὲ διά τε τῶν ἀφορισμῶν αὐτῶν καὶ διὰ τοῦ προγνωστικοῦ γράμματος οὐχ ἥκιστα δὲ καὶ διὰ τῶν ἐπιδημιῶν ἐδίδαξε* [*sc* *Ἱπποκράτης*] *τίνα δ'*

When Galen's *In Hippocratis aphorismos* is placed sometime after, instead of before, *Crises*, so must the preceding four "Hippocratic" commentaries (i.e. the commentaries to Hippocrates' *De capitis vulneribus* and *De ulceribus*, the *In Hippocratis de articulis librum*, and *In Hippocratis librum de fracturis*) also be removed from the positions Bardong has assigned them.[56] If Bardong has otherwise argued correctly, all five of these commentaries, and also Galen's *In Hippocratis epidemiarum libros* 1, must still be arranged before *De praenotione ad Posthumum (Prognosis to Epigenes)*.[57] According to table 1 above, it would be unlikely that so many works could have been written between *De praenotione ad Posthumum* and *Crises*. However, when the dates of these two books are corrected as proposed earlier in this paper, there is found to be a good deal more time between them.[58]

The displacement of the five "Hippocratic" commentaries of course

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αὐτῶν τούτω μόνον ἁπλῶς ὑποστρέφει, χωρὶς ὀλέθρου, τίνα δὲ καὶ δυνατὸν ἡμῖν ἐστιν ἐξιᾶσθαί τε καὶ κωλῦσαι παλινδρομῆσαι. περὶ μὲν δὴ τούτων ἐν ἑτέροις ὑπομνήμασιν ἐξηγούμεθα τὴν γνώμην αὐτοῦ· περὶ δὲ τῶν ἅμα τοῖς λυτηρίοις σημασίοις παυσαμένων, ἃ δὴ καὶ κεκρίσθαι λέγεται, νῦν ἡγοῦμαι χρῆναι διελθεῖν. Here Bardong assumes (1) that *ἐξηγούμεθα* is a present, and (2) that the subsequent *ἐν ἑτέροις ὑπομνήμασιν* refers to Galen's commentaries on Hippocrates's *Aphorismi*, *Prognosticon*, and *De morbis popularibus* However, since Bardong also believes that the only one of these three commentaries written before *De crisibus* was *In Hippocratis aphorismos*, he is obliged to interpret the above passage in two different ways. It "zitiert" *In Hippocratis aphorismos*, he says, but merely "ankundigt" the commentaries to *Prognosticon* and *De morbis popularibus* ("Beitrage," pp. 617, 637).

One of several other objections to this whole line of argument is that *ἐν ἑτέροις ὑπομνήμασιν* need not refer here to any of Galen's formal "Hippocratic" commentaries. The plural *ὑπομνήματα* can of course indicate nothing more than a single multi-book work like *De crisibus* (see for example *De crisibus* 1.20, Alexanderson 124 4 = Kühn 9 637 *paen*). More important, Galen does not refer to his remarks on *all* of what Hippocrates discussed in the above works, but merely to a single broad topic (*τίνα δ' αὐτῶν . . ὑποστρέφει κτλ.*) that Hippocrates happens to have taken up in various places. The importance of this distinction is further emphasized by recalling that when Galen finally did undertake to write formal commentaries on the works of Hippocrates, he considered the task "superfluous" (!) because in everything he had written he had "always expounded the opinion of Hippocrates and juxtaposed the most important of his sayings " In his own words *γιγνώσκων δ' ἐμαυτὸν ἐν ἅπασιν οἷς ἐγεγράφειν ἐξηγησάμενον ἀεὶ τὴν Ἱπποκράτους γνώμην, ἅμα τῷ καὶ τὰς ἐπικαιροτάτας αὐτοῦ ῥήσεων παρατεθεῖσθαι, περιττὸν ἡγούμην εἶναι γράφειν ἐξηγήσεις ἐν ὑπομνήμασι καθ' ἑκάστην λέξιν ἀπ' ἀρχῆς ἕως τέλους ἁπάντων αὐτοῦ τῶν βιβλίων. ἐπεὶ δὲ καὶ ταύτας ἔχειν 'ἐδεήθησαν ἔνιοι τῶν ἑταίρων, ἀπὸ τῶν γνησιωτάτων καὶ χρησιμωτάτων Ἱπποκράτους βιβλίων ἠρξάμην κτλ. (In Hippocratis epidermiarum librum 3 comm 2* preface, CMG vol 5 10.2.1· 60.11). Consequently, there is no good reason to assume that Galen refers to formal "Hippocratic" commentaries in *De diebus decretoriis*. And in any case, the work, or works, in question were not necessarily completed.

[56] See Bardong, "Beitrage," pp 617-622 The dependence of the position of the preceding four "Hippocratic" commentaries on that of *In Hippocratis aphorismos* is decisive When the latter is displaced, so must the others follow.

[57] *Ibid*

[58] The correct date of *De praenotione ad Posthumum* is 178 or early 179 The terminus ante quem for *De crisibus* (as well as *De febrium differentiis* and *De diebus decretoriis)* is 19 May 175 See above.

weakens considerably the position of the four books still remaining between *De diebus decretoriis* and *De methodo medendi*. *De difficultate respirationis* is now the only one of the four which can be shown either directly or indirectly to have been written before *De diebus decretoriis*, and it is by no means certain whether the particular edition of this work cited in *De diebus decretoriis* is the same as the one written after *De methodo medendi*.[59] It is still possible that the first six books of *De methodo medendi* came after, or along with, *De diebus decretoriis*.[60] In any case, there remains no inconsistency of any magnitude in regard to the citations found in *Therapeutics to Glaucon* or to the conclusions drawn from them above.[61]

In summary, changes have been proposed in the dates or relative order of Galen's *Therapeutics to Glaucon, Prognosis to Epigenes, Crises, De diebus decretoriis, De febrium differentiis*, and the "Hippocratic" commentaries. Problems in regard to the date of *De difficultate respirationis* have also been noted.

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[59] The position Bardong has assigned to the first edition of *De difficultate respirationis* would be somewhat stronger if it were not necessary to remove the five "Hippocratic" commentaries which Bardong has placed shortly after it. Once this alteration is made, a number of difficulties already disturbing become still more so:

(1) *De difficultate respirationis* was anticipated as early as the time of *De respirationis usu*, and it was cited in at least four works which Bardong has put before *De methodo medendi* ("Beitrage," pp. 634-635). To make good his claim that *De difficultate respirationis* was first written after *De methodo medendi*, Bardong assumed that the four early works citing it were all re-edited. Bardong's resolution of this and similar problems is based on the quite arbitrary belief that later additions to a text cannot have been substantial (see, in the present instance, "Beiträge," pp. 606-607). Obviously, the possibility remains that one edition of *De difficultate respirationis* appeared before *De methodo medendi*.

(2) Bardong claims repeatedly that, when dealing with a revised book, it is possible, merely by looking at a citation therein, to tell whether it was also present in the (now lost) first edition, or only added in the (existing) second edition. If Bardong is not allowed this somewhat doubtful claim, the question of an edition of *De difficultate respirationis* between *De methodo medendi* and *De diebus decretoriis* does not arise. If he is allowed it, it proves in this case to have a double edge. When the five "Hippocratic" commentaries are removed, the only citations remaining to suggest an edition of *De difficultate respirationis* during this interval are those in *De marcore* 5 (Kühn 7: 685.10), and *De diebus decretoriis* 2.12 (Kühn 9: 890.3), and each of these meets precisely Bardong's criteria for the recognition of "Zusätze," or later additions.

[60] There is no reference to the first six books of *De methodo medendi* in *De diebus decretoriis*

[61] There is also positive evidence of sorts that *Ad Glauconem* was never revised. The books Galen wrote between 169 and November 176 are, as a rule, very difficult to arrange in chronological order. Perhaps that is due to the fact that Galen held on to them until the end of that period (see above) and thus had the opportunity to revise. *Ad Glauconem* is an exception. It was written for a man who was going abroad and whose departure was so close at hand that Galen was apparently obliged to omit material he would otherwise have included (see chapter two of the present author's doctoral dissertation, "Galen's Therapeutics to Glaucon and its Early Commentaries"). The potential usefulness of *Ad Glauconem* for the purpose of dating the other texts mentioned therein has not been fully utilized above.

A POX ON THE ILEUM: TYPHOID FEVER AMONG THE EXANTHEMATA

LLOYD G. STEVENSON

Early in the nineteenth century, at a time when the famous Paris school was taking shape in the great metropolitan hospitals, it could be said that there were, by and large, two categories of disease—the large, vague, shapeless disorders of "physiological medicine" and the neat, circumscribed, well-defined entities which were seen by their heralds as biological realities and by the proponents of "physiology" as mere artifacts, the creations of those who described them. At the center of the so-called physiological medicine was the famous and ebullient Broussais, who derided what he felt to be the creations of the ontologists, the believers in the existence of specific diseases.

Professor Erwin H. Ackerknecht, author of a brilliant and devastating early essay on Broussais, has never failed to do justice to that old soldier and prominent liberal. "Through Broussais essentialism was buried, and localism was made the law. The medicine of symptoms was transformed into a medicine of lesions." For a medicine of lesions there was plentiful and striking evidence. "Influenced by his numerous dissections of cases of typhoid fever, he was led to limit his pathology more and more to lesions of the gastro-intestinal tract."[1]

During a certain period, Paris may be viewed as a field of contention (if not actually a battleground) for Broussais and Laennec, and this is a story that bristles with paradox. It is surely a paradox that localism should have been made the law by a man who did not believe in specific disease entities. And yet, like his other great innovation, it is not hard to explain, for Roupell was able to say of him: "he extends his theory to all those fevers called essential, which, with him, are nothing but gastro-enteritis."[2] On the other hand, Laennec, though he called pathological anatomy the "torch of medicine," believed in the primacy of generalizing humoral pathology and is described by Ackerknecht as an innovator in spite of himself. "It was, after

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[1] Erwin H Ackerknecht, *A Short History of Medicine* (New York. Ronald Press, 1955), p. 136. Cf. *idem*, "Broussais, or a forgotten medical revolution," *Bull Hist. Med.*, 1953. *37:* 320-34.

[2] George Leith Roupell, *A Short Treatise on Typhus Fever* (Philadelphia, 1840), p 38 The theory, of course, was that inflammation of the stomach was the primary event.

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all, he who erected the foundations for a new localism and solidism, and he who invented the instrument which made localistic pathology dominant."[3] Another fact about Laennec which ought not to be forgotten is that he was the pupil of Dupuytren, eminent surgeon.[4]

Laennec summed up the aim of his research, as Temkin points out, in these words: "I have tried, with regard to diagnosis, to put the internal organic lesions on the same level as the surgical diseases."[5] It is in the same essay that Temkin reminds us of the broad range of the surgeon's work outside major operative surgery, work which encompassed all external disorders, as surgical diseases were sometimes called. "Let us not forget the everyday cure of wounds, inflammations, ulcers, dislocations, and fractures; the removal of foreign bodies; catheterization, as well as scurvy, diseases of the eyes and ears, skin diseases, and venereal diseases, the treatment of which the surgeon shared with the physician."[6] And a few pages later, in this brief but weighty essay on "The Role of Surgery in the Rise of Modern Medical Thought," Temkin, too, sums up: "The internist dealt with symptoms which he wished to explain. This he could do by reference to humors, acrimonies, increased irritability or other concepts of a largely speculative general pathology. Or, with Morgagni and his followers, he could look for the proximate causes of disease in the body. These causes, upon dissection, revealed themselves as effusions, hemorrhages, tumors, abscesses, ulcers, inflammations—in short he found within the body what the surgeon found on its surface."[7]

To the catalogue of the types of lesions which were to be found on dissection, it is relevant to our present purpose to add one more—exanthemata. William Budd came to a point in his great book, *Typhoid Fever,* at which he felt that the true significance of the morbid process was no longer doubtful. "When we reflect that it occupies part of a structure which, physiologically speaking, is as much the surface of the body as the skin itself; that the morbid changes in which it consists are scattered widely

[3] Erwin H. Ackerknecht, *Medicine at the Paris Hospital 1794-1848* (Baltimore The Johns Hopkins Press, 1967), p 98.

[4] "At fourteen, he became a military surgeon third class at Nantes. These surgical beginnings certainly contributed to his later preoccupation with pathological anatomy. He himself and his teacher Bichat both emphasized the fact that *pathological anatomy was imported into medicine by surgeons.*" *Ibid.*, p 89. Cf. Temkin, "The role of surgery . . " (n. 5 below)

[5] Owsei Temkin, "The role of surgery in the rise of modern medical thought" (*Bull. Hist. Med.*, 1951, *25*· 248-59) in Temkin, *The Double Face of Janus and Other Essays in the History of Medicine* (Baltimore and London: The Johns Hopkins University Press, 1977), p. 494.

[6] *Ibid* , p 490.

[7] *Ibid* , p. 494

over this surface, with spaces of healthy structure between; that, in their origin, these changes are confined to a single anatomical element; that they are attended *by the formation of a special product, the maturation and casting forth of which appear to be their natural climax;* and, finally, that they are peculiar to the disease before us, *and that disease a contagious fever,*—it is impossible not to see that the analogy already hinted at, as subsisting between this affection and the eruption of small-pox applies to more essential points than any yet mentioned, and that this disease of the intestinal follicles, is in fact, a true *exanthema* of the bowel."[8]

Very soon afterward Budd went on to discuss the illustrations he had provided. "I do not know what impression these illustrations may make on others," he wrote, "but to me it seems impossible to look at them without the idea of an eruption at once arising in the mind."[9] For ten years, so he tells us in a note, he taught this doctrine in lectures he delivered in the Bristol Medical School, and not until 1857 did he find out that Bretonneau had put forward the same view "so long ago as July 7, 1829." Something not unlike a smallpox of the intestinal mucosa had, therefore, appeared to more than one observer. "It is obvious enough . . . that the idea that the intestinal affection is a true eruption had been very clearly apprehended by him, so that whatever credit may be supposed to attach to it belongs to him. At the same time he does not appear to have perceived its all-important bearing on the mode in which this fever is disseminated. Neither in his own papers nor in those of Gendron de l'Eure, who in the matter of typhoid fever may be looked upon as his disciple, is there the slightest hint either of the part which the discharges play in the work of propagation, or of the need of measures to disarm these discharges of their contagious power. Cruveilhier speaks of the intestinal affection having been compared to an eruption, but refers to the notion as being altogether fanciful and unworthy of serious notice."[10] But i Bretonneau did not read from the pocky ileum what Budd was to read them at a later time, he nevertheless found it a revealing scroll. One of the first to delineate typhoid as an entity, he saw the disease as a member of that cohor of specific diseases which he, despite the "physiologists," would proclaim.

Of the eleven numbered conclusions in Budd's summary in his fina chapter, the first proclaims "that typhoid fever is, in its essence, a contagious, or self-propagating fever, and is a member of the great natural

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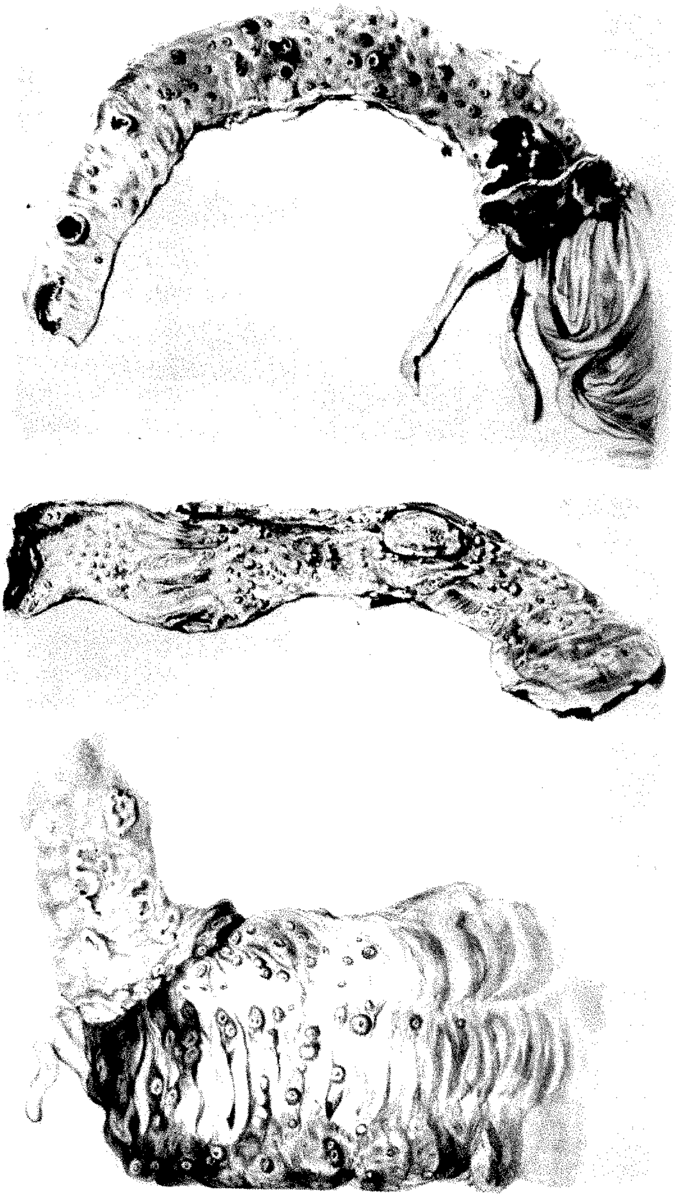
[8] William Budd, *Typhoid Fever. Its Nature, Mode of Spreading, and Prevention* (London, 1873, New York, 1931) pp. 45-46 [italics in the original].

[9] *Ibid.*, p 47

[10] *Ibid.*, pp. 47-48, n 1



Intestinal lesions in typhoid fever.

Top: Twelfth day. Typical ulcerations in the lower end of small intestine.
Center: Lower end of small intestine. The characteristic follicular disease in an early stage.
Bottom: Advanced state. Typical ulcerations, lower end of small intestine as well as in colon.
Illustrations from William Budd, *Typhoid Fever* (London, 1873; New York, 1931), facing p. 46.

family of contagious fevers, of which small-pox may be taken to be the type.''[11] But in the disease situation, in what we may designate the pathology context, of the nineteenth century there were three great prototypes rather than one—and the three were smallpox, tuberculosis and typhoid. Budd explained typhoid by the analogy of smallpox. ''By every title . . . this disease of the intestine is as much a specific character of the fever as a peculiar pustular eruption on the skin is a specific character of smallpox, or as tubercle—to borrow Louis' own illustration—is of phthisis.''[12]

To see typhoid fever in the manner Budd saw it, as ''a true *exanthema* of the bowel,'' was not altogether new. The observation and the idea go back, in point of fact, much farther than to Bretonneau. Murchison points out that in a work of the year 1659 Willis ''alluded particularly to a dysenteric form of fever accompanied by pustules and ulcers in the small intestines, which he compared to the external pustules of variola, an idea which French writers long afterwards claimed the credit of originating.''[13] In the eighteenth century it was reported that smallpox pustules were found in the intestines of persons who had died of smallpox itself. ''How far this may have occasionally taken place,'' wrote Baillie judiciously, ''I will not pretend to say, but late dissections, upon the best authority, have not confirmed this fact.''[14] In 1813 appeared the since famous treatise on typhoid of Petit and Serres, of which Murchison writes: ''These observers pointed out that the lesions were limited to the lower portion of the ileum, and that hence the disease differed from ordinary enteritis. They were the first to regard it as specific. They expressed the opinion that the morbid appearances in the intestine resulted from the introduction of a poison into the system, and that they were of an eruptive nature, like the pustules of variola.''[15] Of

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[11] *Ibid.*, p. 171. Cf. ''Notice sur la contagion de la dothinentérie lue à l'Académie Royale de Médecine, le 7 juillet 1829, par M. Bretonneau,'' *Arch. Gén. de Méd.*, 1829, *21:* 57-78. ''C'est une maladie accompagnée d'éruption intestinale, et non une maladie causée par cette éruption'' (p. 58). Cf. p. 77: ''l'exanthème pustuleux du canal digestif.'' Cf. also *Traités de la dothinentérie et de la spécificité de Pierre-Fidèle Bretonneau* (Paris. 1922).

[12] Budd, *op. cit.* (n. 8 above), p. 44. Cf. p. 43, where the heading of the third chapter exhibits the following epigraph: ''Il faut considérer cette lésion non-seulement comme propre à l'affection typhoïde, mais comme en formant le caractère anatomique, ainsi, que les tubercules forment celui de la phthisie''—Louis.

[13] Charles Murchison, *A Treatise on the Continued Fevers of Great Britain* (London, 1862), p. 389. The work was *De febribus* (1659) but the reference is to *Opera omnia*, ed. Blasius (1682), p. 86.

[14] Matthew Baillie, *The Morbid Anatomy of Some of the Most Important Parts of the Human Body*, 3rd ed. (London, 1807), p. 196. As so often elsewhere, Baillie is citing Lieutaud.

[15] Murchison, *op. cit.* (n. 13 above), p. 395, citing Petit et Serres, *Traité de la fièvre entero-mésentérique* (Paris, 1813), pp. 159, 165, and intro., pp. 20, 39. According to Neuburger, Rosenblum at a later date conceived, just as did Schoenlein and Jahn, of an ''anathem'' corresponding to the exanthem, that is, a disease of the internal mucous membranes and organs, and they were known as convinced supporters of the

Broussais, Murchison declares: "He was aware, that the ulcerations found in fever frequently had their seat in the intestinal glands, but he thought it useless to distinguish between this form and inflammations of other portions of the intestine. So little did Broussais appreciate the nature of the lesions . . . that, in describing this disease as the type of his 'gastro-enterite,' he maintained, that in variola, measles, and scarlet fever, death was due to the same 'gastro-enterite.' " After this, says Murchison, "Cruveilhier, Lerminier, and Andral described the intestinal lesion as an internal exanthem, the ulcerations as preceded by *'pustules'* and the larger patches as an *'anthrax de la membrane muqueuse.'* " According to William Budd's report of the matter, as we have seen, "Cruveilhier . . . refers to the notion as being altogether fanciful and unworthy of serious notice." In much the same manner, although with greater elaboration, Budd dismisses Bretonneau's description of the intestinal eruption, for "he does not appear to have perceived its all-important bearing on the mode in which this fever is disseminated."[17] It is not least in this dispositon of Bretonneau that Budd reveals, and very clearly, the important part which he felt was played by his own contemplation of the pocky ileum in his discovery of the means by which typhoid is spread.

Jacob Bigelow, author of the famous essay "On Self-Limited Diseases," was also concerned with typhoid fever among the exanthemata, but not once did he refer to smallpox. He remarked, in 1835, that "typhoid fever has, in many respects, a marked affinity with the class of eruptive fevers, which are supposed to depend on a specific morbid poison, and which no one pretends to intercept after the body has become infected with them."[18] Scarlet fever and measles have a fixed course: "so also does typhoid." All three diseases generally, but not always, confer life-long immunity. Comparing typhoid with scarlet fever specifically, he observed that the eruption of scarlet fever

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doctrine of "repression" of skin eruptions. Cf. Max Neuburger, *The Doctrine of the Healing Power of Nature throughout the Course of Time*, tr. by Linn J. Boyd (New York, n d.), p. 178, n 1.

[16] Murchison, *loc. cit*, citing Cruveilhier, *Essai sur l'anat. path.* (Paris, 1816); Lerminier et Andral, *Clinique médicale* (Paris, 1823), tome 1, Fièvres, 403. On the same page of Murchison we read "It was reserved for Bretonneau, of Tours, to prove that the disease was always localized in the solitary and agminated glands of the ileum. He also was the first to maintain that it depended on the action of a poison, which was communicable from the sick to the healthy." On the next page (396). "Bretonneau's views were made known in Paris in 1820, but were first published by his pupils Landini and Trousseau, in 1826, and by himself in 1829."

[17] Budd, *op cit* (n. 8 above), pp 47-48, n. 1

[18] Jacob Bigelow, "On Self-Limited Diseases," in *Modern Inquiries: Classical, Professional, and Miscellaneous* (Boston, 1867), p. 155. The celebrated discourse was delivered before the Massachusetts Medical Society, 27 May 1835.

is matched in typhoid by "one of its most constant symptoms . . . a few scattered rose-colored pimples." "In scarlet fever, moreover, certain portions of the mucous membrane undergo morbid alterations, particularly on the tonsils and other parts of the fauces; and these frequently degenerate into ulcers, affecting the subjacent textures. In like manner, in typhoid fever, the mucous membrane of the granular patches in the small intestine, which have been named after the anatomist Peyer, undergo morbid changes; and these changes are followed by ulcerations, and sometimes perforations, of the intestine."[19]

Without reference to smallpox, or to pox on the ileum in typhoid (not at least in so many words), Bigelow almost succeeded in ranging typhoid fever among the exanthemata; and in doing so, he was very little dependent on the rose-colored spots.

In the middle of the nineteenth century there arose a small group, mostly French, of ingenious and slightly quixotic theorists of disease whose doctrine seems to have won a measure of approbation and even international notice for a time but was ultimately quashed by the French Academy. It was a special example of the doctrine of transformation of essential fevers. A contemporary, Charles Murchison, summarized the story tidily: "Several recent French writers, opposed to vaccination, have endeavoured to show that the practice has effected no reduction, but only a 'displacement of mortality,' and that, although smallpox has been arrested, it has been replaced by enteric fever, which, according to them, is nothing more than an internal variola, the eruption being developed in the intestines, instead of on the skin. It has even been proposed to the French Academy, to prevent pythogenic fever [Murchison's name for typhoid] by vaccinating some portion of the mucous membrane. The subject was investigated with the utmost care by the French Academy, and the result was a complete refutation of M. Carnot's doctrine."[20] That it had been, indeed, M. Carnot's doctrine is suggested by the date, 1849, when he—first of all men apparently—had revealed it to a fascinated world;[21] it was 1851 (February) before Ancelon[22] and Bayard[23] (separately) joined the crusade, and 1855 before Gressot conveyed it all the way to Edinburgh;[24] by 1856 Carnot himself was again prepared to joust.[25] But it must have been about this time that the Academy closed its ranks and moved down on him.

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[19] *Ibid.*, p. 156.

[20] Murchison, *op. cit.* (n. 13 above), p. 420.

[21] Carnot, *Essai de mortalité avant et depuis de la vaccine en France* (Autun, 1849).

[22] Ançelon, "Des transformations des fièvres essentielles, dont le cow-pox est la cause," *Compt. Rend. de l'Acad. des Sci.*, Fév. 10, 1851

[23] Bayard, "La fiévre typhoide n'est qu'une variole interne," *Compt. Rend. de l'Acad. des Sci.*, Fév. 10, 1851

In a variety of different ways, but chiefly by contemplating pox on the ileum, doctors since the seventeenth century had been able to range typhoid fever among the exanthemata. What had they learned by so doing? William Budd had learned a great deal but he had not been alone.

It was not merely the pocky gut of the typhoid victim that caused typhoid fever to be seen as a specific disease; but the smallpox analogy once brought to mind, the indications of "specificity" could perhaps be recognized more easily. There were good and sufficient reasons to consider typhoid a contagious disease, but for a very long time many denied that this it was. Case histories, family histories and community histories pointed to the fact that typhoid is spread by the dissemination of feces in water. And yet decades passed and these two important lessons were not learned until a parallel with eruptive fevers was recognized as pointing to contagion and until the content of the pocky gut was suddenly appreciated as the vehicle by which contagion operates. Typhoid was by no means the only disease to be apprehended as self-limiting and self-limited; and yet, when "a marked affinity with the class of eruptive fevers" had been made out—fevers "which no one pretends to intercept after the body has become infected with them"—it was presently apparent that remedies did not work for typhoid either, and that for typhoid as for the exanthemata there is a time to be sick and a time to be well. If the student of typhoid had been alerted by a clear sign or an unmistakable signal—and the pocky gut could serve that purpose well—if he was really attentive, typhoid fever was not unwilling to communicate. Typhoid, in fact, was ready to tell its own tale. Typhoid would proclaim itself a specific disease; typhoid would confess itself a contagious disease; typhoid readily disclosed that it was spread by the mediation of water; typhoid, if given a chance and not subjected to heroic treatment, would protest itself self-limiting. All of these things typhoid would affirm and reaffirm, if only the student's attention could be fixed. It was fortunate that the pocky gut seemed plain as the proverbial pikestaff, that a fine, ostensible eruption demanded to be seen, that a raucous sort of rash was clamoring for attention. This was certainly not an exanthem, so to speak, but an exanthem, so to shout. But what was the message? That was less obvious, certainly, and the clamorous gut seemed to offer various revelations to various students. To Andral it said one thing, to William Budd another; to the French antivaccinationists its message was to stop the use of smallpox vaccine, but very soon their successors came forward with a reinterpretation of the message and a use for the old vaccine that was altogether new. It is not, perhaps, surprising that the communicative ileum spoke wisdom to the wise and to fools only folly. More was required than to fix the student's attention; also needed was a student whose attention was worth fixing. Such students, however, there actually were.

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[24] Gressot, "On the prevention of abdominal typhus by vaccination," *Edin. Med J.*, July 1855.

[25] Carnot, *Revue Méd.*, 1856. This and the last four references are taken from Murchison, *op cit.* (n. 13 above), pp. 624, 625, 626.

Four important conclusions about typhoid fever have been listed above which were suggested in part by the analogy of smallpox and other exanthemata, the concept imprinted on the minds of not a few of these doctors by what appeared, at autopsy, as *une variole interne*. Sometimes the message seemed plain, sometimes very mysterious. There was not necessarily any clear logic in all this. There was not, in fact, ordinarily any message that could be relied on beyond a call to investigate. And yet the message was read, the call was heard. More often than not, perhaps, the genuine conclusions were reached at last by a somewhat different route than the one indicated; and yet *une variole interne* was so often somewhere near the starting point. A pox on the ileum showed the way.

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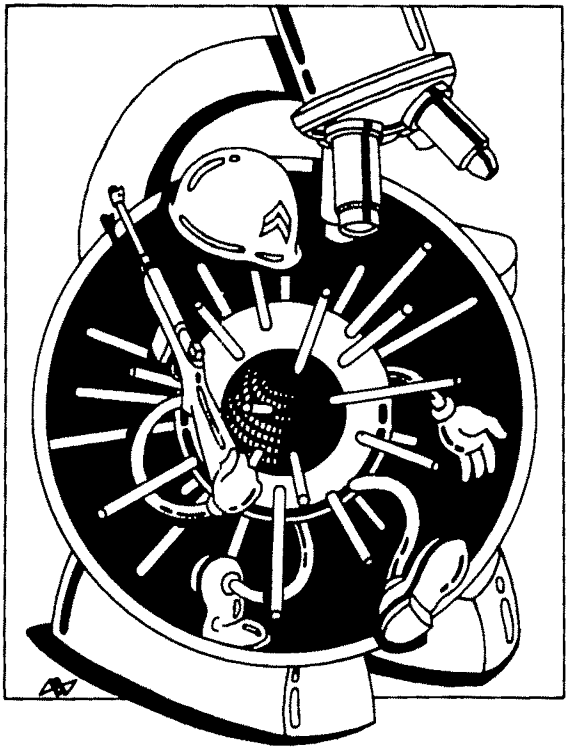

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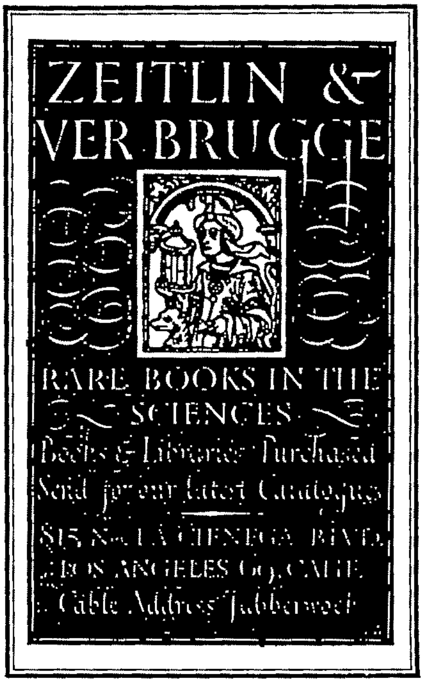

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EDWARD MELLANBY AND THE ANTIRACHITIC FACTOR

JOHN PARASCANDOLA AND AARON J. IHDE*

Introduction

One of us (A.J.I.) has discussed elsewhere the work of the British physiologist and pharmacologist Edward Mellanby (1884-1955) on rickets.[1] While Mellanby's original suggestion that Vitamin A might be the rickets preventing factor, as well as his later "toxamin" theory, proved to be incorrect, his studies were important in leading to the eventual recognition of Vitamin D as the antirachitic agent because they (1) established rickets to be a deficiency disease responsive to a trace nutrient present in certain fats and (2) introduced a technique for creating the disease in an experimental animal.

Recently one of us (J. P.) discovered some relevant correspondence between Mellanby and another distinguished British physiologist-pharmacologist, Henry Dale (1875-1968), while examining the collection of Dale manuscripts at the Royal Society (London) Library. Since these letters shed light on the manner in which Mellanby came to recognize the significance of diet in the production of rickets, it seemed in order to publish them along with explanatory comments. Written in the period 1946-1950, they contain the recollections of the two scientists on Mellanby's early work on rickets during the First World War and provide significantly

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* The authors wish to thank the National Science Foundation for support of this research under grants GS-41462 and GS-27505 Appreciation is gratefully expressed to the Library of the Royal Society of London, to Lady Alison S Todd, and to Mrs J L. Kingdon (on behalf of Lady May Mellanby) for permission to reproduce the letters Part of the research for this paper was carried out by one of the authors (J P) while he was a visiting worker at the National Institute for Medical Research in London, England

[1] A. J. Ihde, "Studies in the history of rickets I Recognition of rickets as a deficiency disease," *Pharmacy in History*, 1974, *16* 83-88

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more insight into Mellanby's thinking at the time that these studies were carried out than do his published accounts.[2]

Edward Mellanby[3] studied physiology at Cambridge University, where he was also exposed to the newly emerging science of biochemistry under the guidance of F. Gowland Hopkins. After completing his medical studies and serving for a brief period as demonstrator in the Department of Physiology at St. Thomas's Hospital in London, he accepted appointment to the chair of physiology at King's College for Women, University of London, in 1913. Mellanby had already become involved in experimental research on the metabolism and excretion of creatine and creatinine while at Cambridge and at St. Thomas's Hospital, and he was soon given an opportunity to pursue further researches along biochemical lines.

Early in 1914, the Medical Research Committee (later the Medical Research Council), then newly appointed to administer government funds for the support of medical research, held a meeting to discuss plans for such expenditures. Henry Dale, who had recently been appointed as the first head of the Department of Biochemistry and Pharmacology at the research institute (later the National Institute for Medical Research) which the M. R. C. had founded, was one of those present at the meeting. Dale later recalled, in a letter (see below) and in his obituary of Mellanby,[4] that the question of the nature and cause of rickets was deemed by the Committee to be an important subject for investigation. Various possible causes of the disease were mentioned, and Hopkins, also a committee member, seemed particularly interested in having someone explore the possibility of dietary deficiency as a factor in rickets. Hopkins' interest in diet is not surprising, for he was himself engaged in important studies on nutrition. As early as 1906 he had expressed his belief that there is a dietetic factor involved in rickets and had alluded to the need for certain trace nutrients (which he termed "accessory factors" in 1912) in the diet to support proper growth.[5]

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[2] In his first publication on the rickets work ("An experimental investigation on rickets," *Lancet*, 1918, *1:* 407-412), Mellanby noted (p 407) that "a considerable number of experiments were first made in an attempt to see whether the aetiology of rickets was to be sought along non-dietetic lines" and that it was later found that diet was the primary factor He did not, however, make any specific mention of the oxidation studies described in the letters, which eventually led him to investigate the effect of diet Later the results of his studies were published in detail in *Experimental Rickets*, Medical Research Council Special Report No. 61 (London His Majesty's Stationery Office, 1921) The account given herein of the early work on rickets (pp 4, 15) is misleading in that it implies that oxidation was only one of many factors being investigated by Mellanby and that the work on diet was undertaken completely independently of the oxidation studies Years later, in his *Nutrition and Disease The Interaction of Clinical and Experimental Work* (Edinburgh Oliver and Boyd, 1934), Mellanby wrote (pp. 1-2) that the problem assigned to him by the Medical Research Council was the calcium and phosphorous balance in rickets, and neglected to mention the studies on oxidation In his last published account of his early studies on rickets, in *A Story of Nutritional Research The Effects of Some Dietary Factors on Bone and the Nervous System* (Baltimore Williams and Wilkins, 1950), Mellanby did finally clearly indicate (pp. 2-3) that it was his investigation of the effect of oxidation which ultimately led him into his research on diet The letters, however, provide a much more vivid and more detailed picture of how Mellanby came to recognize the importance of the dietary factor in rickets than does the published version. The fact that the 1914-1915 Report of the Medical Research Council (as noted below) clearly indicated that Mellanby was studying "experimental Rickets and its relation to conditions of oxidation" provides strong evidence for the validity of the account given in the two Mellanby letters (written four years apart, but completely consistent with each other and with his last published version, which was written during the same time period) Certainly Mellanby would have had no reason to intentionally distort the story to state, as he did in the letters, that he was on the "wrong track" in his rickets research for two years It is understandable, however, that in his early published accounts of his work (in 1918 and 1921) he was more concerned with conveying positive results to his fellow scientists than with giving a detailed historical account of his studies, and so did not emphasize the work on oxidation It is not clear, however, why his 1934 book stressed the problem of calcium and phosphorous balance rather than oxidation processes, unless perhaps in his early work Mellanby saw the two as being closely related.

[3] For biographical data on Mellanby, see H H Dale, "Edward Mellanby, 1844-1955," *Biog Mem. Fellows Roy. Soc*, 1955, *1.* 193-222.

Hopkins' suggestion, which Dale stated may only have been made to him as a "whispered aside" at the M. R. C. meeting,[4] was apparently not transmitted to Mellanby, who was invited by the Committee (at the recommendation of Hopkins) to undertake experimental research on rickets. Mellanby's later recollection (see below) is that he was asked by the Committee to study the relation between rickets and conditions of oxidation. It is not clear, however, who first suggested that his research should be pursued along these lines. In Mellanby's letter of September 29, 1914 (referred to below), to Walter Fletcher, then Secretary of the M. R. C., he proposed that his project be called "An investigation into the various methods of producing experimental rickets, with the object of demonstrating a common factor in all," suggesting that he may not have had any specific causative agent in mind at that time. In the 1914-1915 report of the Committee,[6] it was stated, however, that Mellanby was studying "experimental Rickets and its relation to conditions of oxidation." It seems fairly clear, in any case, that the M. R. C. did not instruct Mellanby to investigate dietary factors, and that the Committee at least approved, and perhaps suggested, the study of the relationship between rickets and conditions of oxidation.

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[4] *Ibid*, p 201

[5] F G Hopkins, "The analyst and the medical man," *Analyst*, 1906, *31* 385-397, F G Hopkins, "Feeding experiments illustrating the importance of accessory factors in normal dietaries," *J Physiol*, 1912, *44* 425-460

[6] *First Annual Report of the Medical Research Committee, 1914-1915* (London His Majesty's Stationery Office, 1915), p. 26. Other approaches to the rickets problem by other investigators are described on pp 25-26 It was not until the *Third Annual Report of the Medical Research Council, 1916-1917* (London His Majesty's Stationery Office, 1917) that it was stated (p 28) that Mellanby had discovered that the primary cause of rickets was a deficiency of an accessory food factor For a general history of the M R C, see A Landsborough-Thomson, *Half a Century of Medical Research*, Vol. I, *Origins and Policy of the Medical Research Council (UK)* (London· Her Majesty's Stationery Office, 1973)

The Committee actually decided to support several projects on rickets at that time, involving the investigation of various factors that might play a role in the disease. For example, studies were funded on the effects of thymusectomy and its relation to experimental rickets, on the adsorption of cholesterol and its distribution in the body in rickets, and on social and economic factors in the causation of rickets (a field study carried out in the city of Glasgow). Also among the researches supported by the M. R. C. in this period was a study in Hopkins' laboratory of the unknown factors (vitamins) in diet which determine growth (see below), although this study appears to have involved a more general investigation of "growth factors" rather than a specific study of rickets.[6]

Mellanby apparently spent about two years attempting to produce rickets in dogs by manipulating conditions of oxidation (e.g., by the use of drugs such as quinine) as he discusses in the letters reproduced here. It seemed possible that reduced oxidation in the animal's body might play a part in the development of the disease (and, if so, then presumably increased oxidation should help to prevent or cure the disease). During the course of this work, he was accidentally led onto the right track with the discovery that rickets was caused by a dietary deficiency. In one series of experiments, he gave puppies large quantities of meat. This step was taken, as Mellanby explains, *not* to test the effects of dietary constituents, but to increase oxidation by means of the specific dynamic action of the meat.

Specific dynamic action had been discussed by Max Rubner in his work on the influence of food materials on metabolism.[7] He observed an increase in heat production in dogs in the hours following ingestion of food, a heat production in excess of that occurring under conditions of basal metabolism. Rubner observed that meat equivalent to 100 calories brought about a metabolic increase of 30 calories. With fats and sugars the increased metabolism was less dramatic (4-6 cal.) but nevertheless consistently observed. Later research revealed that the effect of certain amino acids such as phenylalanine, tyrosine, and leucine is particularly noticeable.[8] Since specific dynamic action suggested an increase in rate of oxidation, Mellanby seized on the meat diet as one which would favor the oxidative conditions he desired in his study of the cause of rickets.

Mellanby found that the high meat diet, contrary to his expectations, produced rickets. This fact, Mellanby claimed, turned his attention towards problems of nutrition and led him to experiment with different diets. Since he noted a certain parallelism between rickets preventive ability and vitamin A content (e.g., cod liver oil and to a lesser degree, butter were effective in preventing rickets), he suggested at first that vitamin A and the rickets preventing factor might be identical. His own later studies, however, failed to confirm this hypothesis,[9] and eventually E. V. McCollum established the existence of a separate antirachitic agent which he named Vitamin D.[10]

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[7] M Rubner, *Die Gesetze des Energieverbrauchs bei der Ernahrung* (Leipzig· Franz Deuticke, 1902), pp. 70-82

[8] C M. Wilhelmj, "The specific dynamic action of foods," *Physiol Rev* , 1935, *15:* 202-220

It is interesting to note that Mellanby pointed out in this correspondence that one of the stumbling blocks to his acceptance of the concept that rickets might be a vitamin deficiency disease was the fact that accessory food factors were often called "growth factors" (i.e., they promoted growth). It was difficult for him to appreciate that a deficiency of something essential for growth could be responsible for a disease (rickets) which only occurred during growth.

It was also during this course of his efforts to produce rickets by interfering with conditions of oxidation that Mellanby discovered, to his surprise, that large doses of thyroid produced rickets. It was later shown by Aub and coworkers that the ingestion of excessive doses of thyroid extract led to increased calcium excretion and possible bone deformation.[11]

Dale's mention (see below) of the fact that the lion cubs in the London Zoo were cured of rickets by the use of cod liver oil reminds one of the long standing use of this fish oil in folk medicine. The specific therapeutic value of cod liver oil in rickets was being seriously questioned by many medical scientists at that time, however, although it was ultimately exonerated when it was shown to be an excellent source of vitamin D.[12] Mellanby's studies provided evidence for the efficacy of cod liver oil as an antirachitic agent, and he warned that vegetable oils should not be used as substitutes for it as they may not have the desired effect. His criticism of the proprietary product "Marylebone Cream" (below) was based on the fact that it contained linseed oil as the supposed active ingredient, but his studies had shown that this oil had no ability to prevent or cure rickets.

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[9] E Mellanby, *Experimental Rickets The Effects of Cereals and their Interaction with Other Factors of Diet and Environment in Producing Rickets*, Medical Research Council Special Report No 93 (London, His Majesty's Stationery Office, 1925)

[10] E V McCollum, N. Simmonds, J E Becker, and P G Shipley, "An experimental demonstration of the existence of a vitamin which promotes calcium deposition," *J. Biol. Chem.*, 1922, *53* 293-312

[11] J Aub, W Bauer, C Heath, and M. Ropes, "Studies of calcium and phosphorous metabolism. III The effects of the thyroid hormone and thyroid disease," *J Clin Invest.*, 1929, *7* 97-137

[12] A. J. Ihde, "Studies in the history of rickets II The roles of cod liver oil and light," *Pharmacy in History*, 1975, *17* 13-20

The Mellanby-Dale Letters

Two letters from Mellanby to Dale and one from Dale to Mellanby (with a portion not relevant to the present subject deleted) are reproduced below.

Edward Mellanby to Henry Dale April 8, 1946

I was interested in the story that you told to the meeting of the sub-committee of the Council last Friday about how I started to work at rickets. Somehow or other it did not seem to fit in with my recollections in one respect and after the meeting I looked the matter up. Of course I have no doubt at all that your story of what happened at the committee is correct but, as I was unfortunately not present at the meeting, I was outside its influence.

What I do know, however, is that I was asked by the committee *to study experimental rickets and its relation to conditions of oxidation.* Since there was no known method of producing experimental rickets, this meant that I had to produce the condition experimentally by altering oxidation processes. This explains what I had completely forgotten, namely, why I wasted a year or probably two years doing all kinds of funny things in order to produce rickets in dogs by interfering with conditions of oxidation. These included reducing the blood supply to the limbs and removing the lymph glands and lacteals of one or two limbs and then trying to stop the whole of the lymphatic system functioning. Other things I remember doing were trying to interfere with oxidation by means of drugs, including quinine. Later I tried the effect of thyroid and found it made rickets much worse; (that was the first time it was shown that thyroid interfered with bone calcification). All this seems crazy now, but it represented my attempt to follow precisely the invitation of the committee. Even the experiment that I referred to the other day in the discussions at the Physiological Society, when you were present, of including in the diet of puppies a large amount of meat was not done so much in order to test the effect of dietary constitutents but to raise oxidation by means of the specific dynamic action of the meat. It was that experiment which turned my mind away from problems of oxidation to those of nutrition because as you already know, I found that very bad bone deformities were produced by this diet. I have heard or seen it suggested on a number of occasions, that rickets was an easy problem because it was half solved when I started it, that I want to put on record exactly what did happen and that far from getting helpful advice from the Medical Research Committee at that time, I was actually put on to a wrong track. This is not meant to be a complaint or groan in the slightest degree. I was lucky to get the chance of working on rickets.

Henry Dale to Edward Mellanby April 9, 1946

I am really grateful for your letter of April 8th, giving me the correct history about the M.R.C.'s early dealings with rickets. Of course, my only data are those of memory; but I believe that my memory of things which happened in those days, when I was comparatively young, is fairly accurate.

What I remember is a meeting of the original Medical Research Committee some time in 1914, after the original members of the research staff had been appointed, but before they took up their appointments. The Committee asked them to be present at this discussion, of subjects selected as suitable for coordinated attack by research, apart from what the research staff might start on their own initiative Rickets was one, and is particularly prominent in my memory. The discussion ranged over all the well-worn possibilities—defective fat absorption, para-thyroid deficiency, infection, and some kind of dietary deficiency. Nobody, as I remember the discussion, thought there was any evidence pointing to the last of these possibilities, although, as you know, there was a recurrent but ineffective tradition about the value of cod liver oil.

I remember, when I was at Bart's, hearing that all the lion cubs in the Zoo had suffered from rickets, until the surgeon, Tom Smith, had told the Superintendent to give them cod liver oil. I do not think that anybody had, at that time, any such possibilities in mind; but I do remember that Hopkins thought that somebody ought to be asked to explore the question of dietary deficiency. After that meeting, the Committee's activities and plans disappeared completely from my purview, and I never heard who had been asked to do what. When I learned, some years later, that you were producing rickets in puppies by a dietary deficiency, and finding that the deficiency could be replaced by the fat-soluble vitamin of those days, I suppose I assumed that you had been invited by Hopkins, on behalf of the Committee, to explore the possibility of such a deficiency being the cause. I should not like you to imagine, however, that that in the slightest degree weakened my admiration of what you had done. I do not think that I ever make the mistake of supposing that a suggestion from any source, that a particular possibility might be worth exploration, has any value of title to credit, in any way comparable with that of the production of the experimental evidence which settles the matter one way or the other. What you tell me, however, about being asked to study the relation of experimental rickets to oxidation, is entirely new to me, and I am very glad to have the story right; not because I think that it is of great importance, but because I take the interest of an incorrigible gossip in the way in which important discoveries have been made . .

Thanks, again, for giving me the true story of the relation of your *rickets* work to the M.R.C. scheme.

Edward Mellanby to Henry Dale September 27, 1950

Sorry for the long delay in replying about my early rickets research. I failed to find anything of interest here and then appealed to Thomson to see if there was anything of interest at the M.R.C. He has sent me a few references, copies of which I enclose.

Exhibit 1 (July 31, 1914) is the letter of invitation to me to join in the research from Fletcher. No plan of work suggested here.

Exhibit 2 (Sept. 29, 1914) is a letter stating my general object in the work. You will note that, although I was at the London Hospital (as a part-time demonstrator in Pharmacology) with unlimited access to ricketty babies and children, my sole object was to work on experimental rickets in *animals*—as you know, I chose puppies. There is no reference here to accessory food factors.

Exhibit 3 is an extract of the First Annual Report of the Medical Research Committee for 1914-1915. Here you see my first attempt to define a 'common factor' in rickets production, namely 'conditions of oxidation'. It is interesting to see that Hopkins with Winfield's assistance had tackled the 'growth factor' side of the problem at this time.

I spent two years on this oxidation business—giving drugs to raise and lower metabolism—and curiously enough I was the first to show that giving large doses of thyroid actually produced rickets—a result which knocked me over at the time, but opened up a big field of investigation by others. However, this work ultimately led me to the right track, because in one series of experiments I gave large quantities of meat to raise the specific dynamic action of the diet and got awful rickets in the puppies (plus a good deal of osteoporosis). I then saw the importance of the diet and proceeded to devise a simple diet which would always produce rickets in puppies. The simplest diet compatible with reasonable health was bread and milk where the milk was limited to about 200 ccs. daily. I then found if I removed the milk fat I got rickets more quickly. The next step was due to my discovery that in London clinics for children a concoction called Marylebone Cream was being distributed largely for the treatment of rickets. I added it to my standard diets and found it was without effect on the development of rickets. Marylebone Cream consisted of emulsified linseed oil. I then tried other fats, including cod-liver oil, butter, lard, suet and vegetable oils. All this work was done under dreadful conditions as the war was on and I could not find a place to keep puppies until I moved the work to the Field Laboratory at Cambridge, and this involved visiting Cambridge every Friday night till Sunday night.

Exhibit 4: You will see that by this time I was well away with the work at Cambridge. This letter is undated, but I think it must have been written in 1917. You will see with interest that I had also now got going with the alcohol work, for which I kept the dogs at the Brown Institution.

Altogether there is good evidence not only that I had no instructions to work on vitamins in rickets, but that I only came to this aspect of the subject very slowly and with great labour. One of my great intellectual difficulties with this particular line of investigation was that accessory food factors were called '*growth factors*' and I observed early on that rickets was a disease of growth—no growth, no rickets. It was difficult to believe that a deficiency of something essential for growth could be responsible for rickets.

It was necessary, therefore, for me to break away from the 'growth' tradition and show that the question of the relation of these substances to growth was only incidental and due to a common technique used at that time, namely the rate of growth of young rats.

Is this information any use to you? I could talk at much greater length, especially about the difficulties of this work. It looks such a simple problem now, but every experiment took 3 to 6 months and it was heart-breaking on many occasions.

(A handwritten postscript added by Mellanby to the above typed letter reads: "I enclose the first two papers on (a) teeth by my wife and (b) rickets. They certainly demand the forgiving eye for perusal." The first paper referred to is probably May Mellanby's paper on the influence of diet on teeth formation in *The Lancet* in 1918, and the second Mellanby's initial paper on rickets from the same journal and year).

Note: The four items described above (Exhibits 1-4) were attached to the letter. They are adequately summarized for our present purposes in

Mellanby's letter (with 2 and 3 also being discussed in the introductory comments above) and so they are not reproduced here. These four documents were supplied to Mellanby by A. Landsborough-Thomson, then Secretary of the Medical Research Council.

HISTORY OF TISSUE CULTURE AT JOHNS HOPKINS*

FREDERICK B. BANG

It is healthy in this day of massive and often specifically oriented research to examine the origin and history of research in tissue culture at Johns Hopkins. In this review I will not only discuss some of the outstanding personalities who did the work, but will attempt to fit together the ideas, drives and circumstances under which the work was done. Other historic data on three of the main participants may be found in Dr. A. M. Harvey's recent article.[1] In this presentation, we will be concerned with six people: Ross Harrison, Montrose Burrows, Warren and Margaret Lewis, and George and Margaret Gey. My close association in the laboratory with George Gey over a period of 33 years, and that of my wife and myself with George and Margaret Gey at the personal level for some forty years, means that the account will have a personal bias. Perhaps a touch of that will vitalize the dry bones of the published records of scientific history. The research was exciting and demands for credit or priority were not absent from two of the protagonists: Burrows and Carrel.

This first successful use of an in vitro (on glass) method for the solution of a specific biological question about the growth of cells was that of Ross Harrison when he was working in the Department of Anatomy at the Johns Hopkins School of Medicine.[2] I have carefully defined my terms so that the statement is accurate. There were, however, a number of predecessors in the use of tissue culture itself who made varying contributions in ideas and techniques. Leo Loeb, a comparative pathologist par excellence, studied Limulus cells as they spread out on glass but did not grow.[3] Roux took

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* Based on a paper presented at a symposium on Tissue Culture at Hopkins, a centennial meeting honoring Ross G Harrison, Warren H. Lewis, Margaret R. Lewis, George O Gey, Margaret K. Gey, organized by The Johns Hopkins Medical History Club, Baltimore, 22 March 1976

[1] A M. Harvey, "Johns Hopkins—The birthplace of tissue culture: The story of Ross G. Harrison, Warren H Lewis and George O Gey," *Johns Hopkins Med. J* , 1975, *136*: 142-149.

[2] R G Harrison, "Observations on the living developing nerve fiber," *Proc. Soc Exp Biol Med.*, 1907, *4*· 140-143; L. Loeb, "On the blood lymph cells and inflammatory processes in Limulus," *J Med Res.*, 1902, *7*: 145-158

[3] Loeb also outlined in a "reprint" published in Chicago, but written in German as early as 1902, how he thought growth of cells could be carried out outside of the body He states that much of the work was done in

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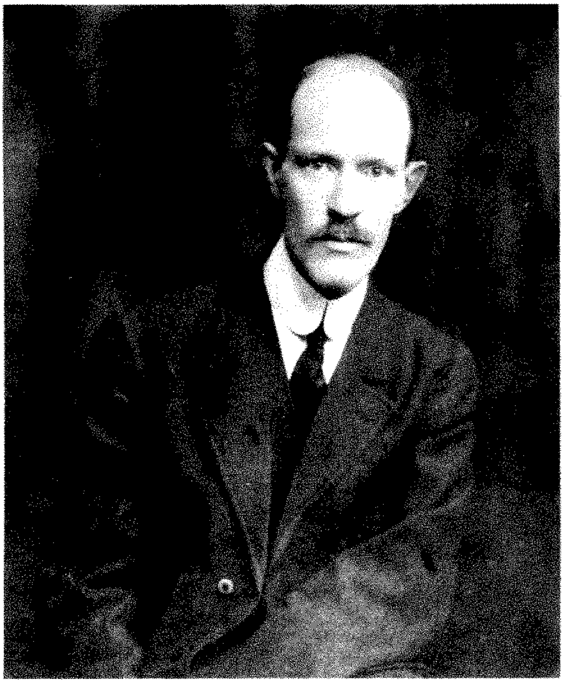
embryos apart. Haberlandt studied plant cells and theorized about growth.[4] But it was the mind of Ross Harrison which demanded a specific answer to a specific question[5] concerning nerve muscle growth that made him take a bit of medullary tube, place it on a coverslip in a drop of freshly drawn frog lymph sac fluid, allow it to clot, then invert it over a hollow ground slide and look at the growth of nerve cells therefrom. It was really very simple. In his own words, delivered to the 23rd meeting of the Society for Experimental Biology and Medicine in New York on May 22, 1907:[6]



Ross G. Harrison

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the Department of Pathology at Johns Hopkins [L. Loeb, "*Ueber die Entstehung von Bindegewebe, Leukocyten und roten Blutkörperchen aus Epithel und über eine Methode, isolierte Gewebsteile zu züchten.* (Chicago: Stern, 1897); and "Ueber das Wachsthum des Epithels," *Roux' Archive für Enwickelungsmechanik der Organismen*, 1902, *13*: 487-506].

[4] E. N. Willmer, *Cells and Tissues in Culture. Methods, Biology and Physiology*, vols. 1, 2, 3 (New York: Academic Press, 1965, 1966).

[5] J. M. Oppenheimer, "Historical relationships between tissue culture and transplantation experiments," *Trans. Studies Coll. Phys. Philadelphia*, 1971, *39* (4th ser.): 26-33.

[6] Harrison, *op. cit.* (n. 2 above), p. 141-142.

> The immediate object of the following experiments was to obtain a method by which the end of a growing nerve could be brought under direct observation while alive, in order that a correct conception might be had regarding what takes place as the fiber extends during embryonic development from the nerve center out to the periphery.

In retrospect, the same or a similar technique would surely have been invented by someone else under other circumstances. Indeed, in the Hopkins story, one almost gets the idea that it was invented twice. The procedure was simple and had no new components; lymph had been used by Harrison's teacher, Born, and the procedure of looking at material in a hanging drop on a coverslip was standard bacteriologic practice. However, the drive to make this simple step was a beautifully logical series of experiments which illustrates Harrison's approach.[7] Harrison, as a young man brought to the Department of Anatomy at Johns Hopkins Medical School some years after his graduation with a Ph.D. under W. K. Brooks at the Department of Biology of the University, and later an M. D. in Bonn, Germany,[8] was caught up in the current controversy of nerve and muscle relationship. One of his early articles[9] starts:

> The influence of the nervous system on the regeneration of lost parts has formed the subject of several experimental studies. . . . The first questions I had in mind in beginning the present investigation were the comparatively simple ones, whether a stimulus from the nervous system is necessary in order to start the differentiation of striated muscle fibers.

To test this hypothesis, Harrison determined on frog embryos the stage at which there was histological differentiation of muscle. He then removed the spinal cord from these embryos, leaving the underlying notochord intact. Such nerveless embryos developed normally despite the lack of peripheral nerves, but were retarded as far as most of their external form was concerned. However, the muscle fibers were normal. He concluded that the nerve elements normally innervating a muscle play no part in its morphogenesis (1904).[10]

The complementary question, then, was how the outgrowth of nerves into the muscle was influenced by the muscle mass into which it grew. The reigning theory, propounded by Hensen, was that "the nerve centers and their peripheral end-organs are connected from the beginning of embryonic life by means of protoplasmic bridges and that the development of the nerve fibers consists merely in the differentiation of these pre-existing connections under the stimulus of functional activity."[11] Harrison's experiments of 1907[12] supported the opposite view, i.e., that the nerve fiber is the outgrowth of the ganglion cell. In a series of four experiments, limb buds were taken from nerveless or "aneurogenic" larvae and transplanted to normal individuals, and the growth and differentiation of these "nerveless" and normal limbs was compared. The donor nerveless larva serving as the source of the limb was sectioned and it was demonstrated that the original operation was successful, i.e., there were no nerves in this animal.

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[7] J. M. Oppenheimer, "Ross Harrison's contributions to experimental embryology," *Bull. Hist. Med.*, 1966, *15*: 525-543; J. M. Oppenheimer, "Cells and organizers," *Am. Zoologist*, 1970, *10*: 75-88.

[8] J. S. Nicholas, "Ross Granville Harrison, January, 1870—September 30, 1959," *Biogr. Memoirs Natl. Acad. Sci.*, 1961, *35*: 131-162 (New York: Columbia University Press).

[9] R. G. Harrison, "An experimental study of the relation of the nervous system to the developing musculature in the embryo of the frog," *Am. J. Anat.*, 1904, *3*: 197-220.

[10] *Ibid.*, p. 216.

The recipient tadpole burdened with the additional limbs (one normal and the other nerveless) was kept alive for another month, by which time each of the transplanted buds had given rise to a pair of legs. While alive, this larva had shown slight movements in the aneurgenic limb. The recipient embryo with its two extra limbs was serially sectioned and the internal anatomy of the extremities reconstructed. The relationship of the plexus of the nerves from the spinal cord to this aneurogenic limb was determined.

The succeeding experiments varied as to species and the presence or absence of a normal limb transplant. All but one of the transplanted limbs contained nerves. These experiments led to the critical experiments of in vitro growth.

Looking back on this work 28 years later, Harrison[13] wrote:

> I had a definite embryological problem to solve which it seemed to me could only be done by a radical experiment in isolation and that is what the tissue culture experiments were par excellence. . . . Technically the method is an adaptation of one that has been for years familiar to the bacteriologist—the hanging drop culture. Any originality, therefore, that may be claimed for this work is due to a combination of ideas, rather than the introduction of any particular device.

When Harrison decided, in the same year as the presentation of his fundamental discovery, to move to Yale University, work on tissue culture at Johns Hopkins ceased. Even though Harrison had used the newly invented method as a specific answer to a specific question, he did not fail to recognize the potential of the methodology. In the subsequent few years, there was an intriguing and inadequately described sequence of events in which Montrose T. Burrows, who graduated from Johns Hopkins Medical

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[11] Harrison, *op cit.* (n 2 above)

[12] *Ibid*

[13] R G Harrison, "On the status and significance of tissue culture," *Arch exp Zellforsch.*, 1928, *6*, 4-27

School in 1909, played a very significant part. He must have been exposed during his first two years (1905-1907) to the influence of the Department of Anatomy, but there is no record of this. In some ways, his story seems that of a visionary man who wanted to conquer all; immediately after graduation, he went to the Rockefeller Institute and to Cornell, became disillusioned and returned to the Department of Pathology of Johns Hopkins in 1915-1917 as Associate and Resident Pathologist. Later still, he went to Barnes Hospital in St. Louis.[14] He seems to have been the real connecting link between the fundamental studies of Harrison and the wild, popular, intensely exciting early claims of Carrel and his disciples at the Rockefeller Institute, where the growth of chick heart cells for a few weeks was equated with immortality, and pure strains of cells for the growth of viruses were spoken of thirty years before they became available.

In a letter to a New York acquaintance years afterward,[15] Burrows said that he came to Johns Hopkins in 1905 because his sponsor, Hertzler, had convinced him that Mall was the greatest man in American medicine. In his own words of 1942:

> I had collected a large number of insect galls. I was convinced that if one was to solve the formation of these galls, the growth of cells in cancer and the body, one must find a means to study the reaction of body cells *in vitro*. . . . Again I was convinced that cellular reactions in the body are not the work of cellular, but rather of body reactions. The idea was fantastic to the ordinary college professor at that time, so in Kansas University I kept away from McClung. . . .

Whether he had seen Carrel before is not known. Carrel had, however, lectured in 1906 at Johns Hopkins on surgery of the blood vessels, etc.[16] Burrows apparently went to work, following Flexner's request, with Harrison at Yale for a few months. He goes on with incisive descriptions:

> The problem before me was to reproduce the exudate as it occurs in a wound. I must prepare blood plasma first. Then I must add tissue extracts which occur in the wound exudate. Harrison did not uphold my ideas, but finally consented to let me repeat his nerve experiment, using a warmblooded animal. Chickens were used because it was easy to obtain chick embryos. . . . When I showed the growing and dividing cells to Harrison, he was astonished. Flexner was overjoyed. . . . The discovery also pleased the Rockefellers.

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[14] M T Burrows' biography in *American Men of Science* of 1921

[15] M T. Burrows, Copy of 26 January 1942 letter to Dr F. M Allen, New York City, R G Harrison Papers, Archives and Manuscript Department, Sterling Library, Yale University, New Haven, Conn

[16] A Carrel, "Surgery of the blood-vessels and its application to the changes of circulation and transplantation of organs," *Johns Hopkins Hosp Bull*, 1906, *17*· 236-237.

The arrangement for Burrows to learn tissue culture from Harrison at Yale, was made by Flexner.[17]

In this work with Carrel at the Rockefeller Institute in 1910, excitement was so great that four separate papers were published within the course of three months in the *Journal of the American Medical Association.*[18] It was as if a bulletin had to be put out on each few weeks of "growth" of cells. Indeed, in the correspondence of Burrows with Harrison at Yale, there is discussion as to how, if Harrison did not accept certain words, they would be able to telegraph corrections to the editor. Apparently Harrison decided not to publish with Burrows. Flexner, in a letter of apology to Harrison on the rapid publication of results, spoke of the "exalted state of mind" of Carrel and Burrows. (Burrows and Carrel had published before Harrison,[19] who was the mentor, had seen the results.)

However distrustful Burrows was of the cell theory, he had made a basic contribution: the growth of warmblooded vertebrate cells in chicken plasma clots. This allowed one to see more rapid growth, to watch mitosis of cells, and to follow migration of cells from the original tissue. The publicity was excessive. No wonder Burrows characterized Carrel as a man "who knew mass psychology and was the newspaper favorite of the Institute."[20]

Burrows' imaginative contributions to tissue culture included searches for new substrates.[21] He grew chick cells on spider webs.[22] Later he studied the interference of amino acids with each other during the growth of chick cells.[23] He was interested in their metabolism, the effect of oxygen, etc. But despite all this (or was it because of this), in the *Bulletin of the Johns Hopkins Hospital* of August 1917, he writes:[24]

The question in cancer is whether the cancer cell is a normal cell responding to a

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[17] R. G. Harrison, Copy of 20 October 1928 letter to Dr. Wm. Seifriz, Philadelphia, R. G. Harrison Papers, Archives and Manuscript Department, Sterling Library, Yale University, New Haven, Conn.

[18] A. Carrel and M. T. Burrows, "Cultivation of adult tissues and organs outside of the body," *J.A.M.A.*, 1910, *55*: 1379-1380, M. T. Burrows, "The cultivation of tissues of the chick-embryo outside the body," *J.A.M.A.*, 1910, *55*: 2057-2058; A. Carrel and M. T. Burrows, "Cultivation of sarcoma outside of the body," *J.A.M.A.*, 1910, *55*: 1554 and 1732.

[19] Harrison, *op. cit.* (n. 17 above)

[20] Burrows, *op. cit.* (n. 14 above)

[21] M. T. Burrows, "The growth of tissues of the chick embryo outside the animal body, with special reference to the nervous system," *J. Exp. Zool.*, 1911, *10*: 63-84.

[22] A. Carrel and M. T. Burrows, "Cultivation in vitro of the thyroid gland," *J. Exp. Med.*, 1911, *13*: 416-421.

[23] M. T. Burrows and C. A. Neymann, "The toxicity of alpha-amino-acids for embryonic chicken cells," *Bull. Johns Hopkins Hosp.*, 1917, *28*: 180-181.

[24] M. T. Burrows, "The tissue culture in cancer," *Bull. Johns Hopkins Hosp.*, 1917, *28*: 269.

> peculiar environment or whether it is a cell whose metabolism is such that it can grow in an environment where other cells cannot show these changes.

Pretty good and incisive! But then he goes on:

> Cells of higher animals are not highly organized, but fluid-like systems. Their various manifestations of life such as movement, growth, etc., are differential surface tension phenomena under the control of a specifically organized environment.

His imaginative approach continued. With Suzuki at Washington University Medical School, he showed that tissue culture may be used to study neutralization of diphtheria toxin.[25]

Strange as it may seem, spontaneous interest in tissue culture occurred twice within the Department of Anatomy at Johns Hopkins. Mall,[26] the teacher who could not lecture and who taught by induction (he made you think), had brought into his new department a second embryologist, Warren Lewis. He studied the development of the eye in amphibia, and also by using embryological methods, regeneration and differentiation of the central nervous system. Lewis has also studied nerve-muscle relationships.[27]

As far as I can tell, Warren Lewis really had no interest in tissue culture as such until he married Margaret Reed. Since she was the one who was the first author of seven of their eleven contributions from 1908 to 1917, I will start with a quote from their first paper, entitled "The growth of embroyonic chick tissues in artificial media, agar and bouillon," published in the *Bulletin of the Johns Hopkins Hospital* from "The Anatomical Laboratory, The Johns Hopkins University":[28]

> In 1908, working under Dr. Max Hartman at the Kgl. Institut fur Infektionskrankheiten in Berlin, one of us (MRL)[29] found that bone-marrow from the guinea-pig formed a membrane-like growth with mitotic figures on the surface of nutrient agar. This agar was a modification of one used at the Institute for the cultivation of amoeba.[30] In the place of distilled water a modification of Locke's modification of Ringer's solution was substituted.

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[25] M. T. Burrows and Y. Suzuki, "The study of problems of immunity by the tissue culture method. II. The tissue culture as a means of quantitatively estimating toxin and antitoxin and determining the distribution of antitoxin in passively immunized animals," *J. Immunol*, 1918, *3*· 219-232

[26] F. R Sabin, *Franklin Paine Mall The Story of a Mind* (Baltimore· The Johns Hopkins Press, 1934), 342 pp.

[27] W. H. Corner, "Warren Harmon Lewis, June 17, 1870—July 3, 1964," *Biogr Memoirs Natl Acad Sci*, 1967, *39*. 322-358 (New York Columbia University Press).

[28] M. R. Lewis and W H. Lewis, "The Growth of embryonic chick tissues in artificial media, agar and bouillon," *Bull Johns Hopkins Hosp*, 1910, *22*. 126-127

[29] Not in original text

[30] This was apparently developed by Rhoda Erdmann, who also later went into tissue culture, wrote a text

> Last fall we began the cultivation of embryonic pig bone-marrow in the blood plasma of older pig embryos. We utilized very much the same methods as were described by Burrows and Carrel, but without very conclusive results. We attempted to start our cultures from a very few cells instead of small pieces of tissue.
>
> With the appearance of the important publications by Burrows and Carrel on the cultivation of the tissues of warmblooded animals in clotted plasma adapted by Burrows from Harrison's brilliant discoveries on the growth of amphibian tissues, especially nerve axons in clotted lymph, we began similar studies on embryo chicks of various ages and were able to obtain by the method of Burrows and Carrel very wonderful growths from the various organs of the embryo.

This first paper was the beginning of a series of attempts to determine the growth requirements of cells, and it led to the work of a number of others on the definition of the specific requirements of cells in tissue culture. This was and is one of the major areas for investigation, both here in Baltimore and elsewhere, but I will not attempt to trace it now.

Looking back now with hindsight, it seems to me that, in addition to the constant help that Margaret Lewis gave to many who later came to the Carnegie Institution from the Departments of Pathology and Anatomy (I ran across twenty joint papers in the *Johns Hopkins Hospital Bulletin*), there were two major contributions. The first was the establishment of pathology of cells in tissue culture as a specific area of study. The second was the use of tissue culture in immunology. Although there had been a few instances of growing viruses in tissue culture before her studies, these were almost all on crude preparations of minced tissue kept in flasks in one or another medium where there was no attempt to examine the tissue and see what was happening. In 1920, Margaret Lewis wrote a paper[31] which I still find remarkable today. Someone should surely re-examine it in terms of the present-day interest in the effect of various gram-negative endotoxins on cells in vitro. She showed that, when cultures of typhoid bacteria were added to chick embryo intestinal cultures, she obtained a vacuolation of the endodermal cells which grew out from the explant. This was especially striking in that the cells became foamy in appearance. Some of the cells of course also showed phagocytosis and the bacilli were often found bouncing around within these vacuolated cells. This paper introduced the principle of studying specific cell destruction, specific in terms of both the type of tissue used and in terms of the kind of cytopathology. I am unaware of similar

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on this, and worked with Harrison [R. Erdmann, *Praktikum der Gewebepflege oder Explantation besonders der Gewebezuchtung* (Berlin Springer Verlag, 1922)].

[31] M R Lewis, "The formation of vacuoles due to bacillus typhosis in the cells of tissue cultures of the intestine of the chick embryo," *J Exp Med*, 1920, *31* 293-313.

studies on typhoid. Subsequently, she did several other interesting studies on the effect of pathogens on cells, collaborated later with Andervont of the new and first Department of Filterable Viruses of the School of Hygiene. In the Department of Pathology, where Ella Oppenheimer, working with MacCallum, had partially purified vaccinia virus by differential centrifugation, this suspension of virus was placed on outgrowths of rabbit cornea and cell destruction was observed.[32]

However, the historically most important discovery of Margaret Lewis was that in the field of cellular immunology when she worked with Arnold Rich. I will here skip ahead and give a diagram of the present status of our knowledge of cellular immunity. To explain how we got here, we need to go back to our days as students in the Department of Pathology when Rich was proclaiming the doctrine that allergy and immunity to tuberculosis are two separable events following infection. The nature of this allergic reaction facinated everyone. In 1932, Rich and Margaret Lewis published "The Nature of Allergy in Tuberculosis as Revealed by Tissue Culture Studies."[33] Rich separated those reactions in which local damage is produced yielding acute inflammation (tuberculin reaction) from those in which smooth muscle is involved (anaphylaxis). The reaction to tuberculin was studied:

> For this purpose the method of tissue culture seemed particularly suitable, for cells under the conditions of tissue culture are delicate indicators of the presence of cytotoxic substances, they are free from all complicating nervous and circulatory influences.

And so guinea pigs were made tuberculin-sensitive by the inoculation of avirulent human tubercle bacilli, and cells were taken from the buffy coat of normal and sensitized animals. The buffy coat was cut into small pieces and cultured. Spleen was also used; a small piece was removed directly from the animal. This was also cut into small pieces. The plasma was saved. They studied the effect of tuberculin on normal and tuberculin-sensitive cells in plasma of the normal and sensitive animals. When tuberculin of graded doses was added, striking results were obtained:

> The damage done to allergic cells by tuberculoprotein is marked and easy to observe. It is evidenced by a decided inhibition of migration of the allergic cells from the explant into the surrounding tuberculin-containing plasma and by the fact that the relatively few cells which do wander out die in a few hours in contrast to the

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[32] E C. Craciun and E. H. Oppenheimer, "Vaccinia-virus in tissue cultures," *Bull Johns Hopkins Hosp*, 1925, *37* 428–429

[33] A R Rich and M R Lewis, "The nature of allergy in tuberculosis as revealed by tissue culture studies," *Bull Johns Hopkins Hosp.*, 1932, *50*: 115-131

> swarming migration of normal cells into the same medium, most of which remain alive and multiply for several days or more. The results of the experiments, which comprise nearly two thousand individuals cultures . . . make it clear that washed cells are hypersensitive.

This was 1932. Yet, today, we think of cellular immunology, MIF, lymphokines and the like, as fairly recent phenomena. I suspect there were a series of reasons for the great delay. On the one hand, perhaps too much emphasis in the original work was put on the death of the cells, and not enough on the inhibition of the migration of the macrophages. Attempts to show actual specific cell sensitivity to the tuberculin or other proteins failed because later cultures were too pure. A study in Dr. Rich's own laboratory failed to show that liver cells were susceptible, etc.[34] However, thirty years later, in 1962, George and Vaughan[35] in Rochester placed the cells from the allergic animal in a small capillary tube and showed that the macrophages from allergic animals were inhibited from migrating from the tube in the presence of tuberculin.[36] Then Bloom,[37] and independently David,[38] showed that the real target cell in the original experiments was the sensitized lymphocyte and that it responded in the presence of antigen with the production of lymphokines which inhibit migration of macrophages. Thus today we accept the original experiments of Lewis and Rich, since we now have a satisfactory theoretical framework to explain them.

One other collaborative effort between Rich, Mrs. Lewis and Wintrobe[39] needs mentioning. Again, let's go back to the state of knowledge of immunology in 1939. At that time, splenomegaly of infection was well recognized and of course lymphocytes themselves were highly suspect as the source of antibodies. However, the acute splenic tumor of infection contained within it great numbers of large mononuclear cells with rounded nuclei and nongranular cytoplasm. The nature of these cells was unknown. So the three set about deciding the nature of these cells by characterizing

[34] J. J. Buckley, S. M. Buckley, and M. K. Gey, "Tissue culture studies on liver cells of anaphylactically (arthus) sensitized animals in the presence of the sensitizing antigen," *Bull. Johns Hopkins Hosp.*, 1949, *84*: 195-203.

[35] M. George and J. H. Vaughan, "In vitro cell migration as a model for delayed hypersensitivity," *Proc. Soc. Exp. Biol. Med.*, 1962, *111*: 514-521.

[36] *Ibid.*

[37] B. R. Bloom, "In vitro approaches to the mechanisms of cell-mediated immune reactions," *Adv. Immunol.*, 1971, *13*: 101-208.

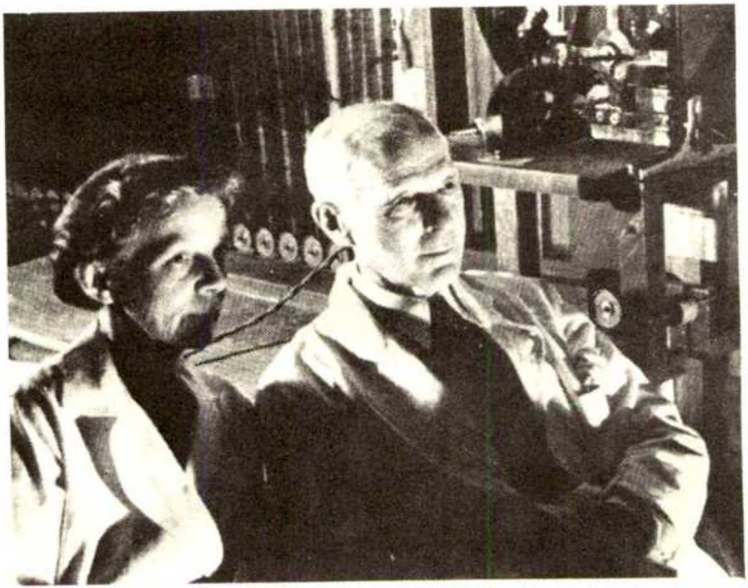
[38] J. R. David, "Mediators produced by sensitized lymphocytes," *Fed. Proc.*, 1971, *30*: 1730-1735.

[39] A. R. Rich, M. R. Lewis, and M. M. Wintrobe, "The activity of the lymphocyte in the body's reaction to foreign protein, as established by the identification of the acute splenic tumor cell," *Bull. Johns Hopkins Hosp.*, 1939, *65*: 311-326.

them with motion pictures. The acute splenic tumor cell had the locomotion and hand-mirror form in culture of a lymphoblast and was totally different from the myeloblasts or fibrocytes. Therefore, there was a massive multiplication of lymphoid cells in the spleen following a variety of antigenic stimuli. Thus another piece of the immunological picture was filled in.

Mrs. Lewis' interest in the growth of pathogenic agents in tissue culture never ceased. During an attempt to grow the common cold virus, she asked Margaret Gey to inoculate her intranasally with fluid containing the virus. Margaret Gey refused, whereupon Mrs. Lewis promptly did it herself, spraying the apparently infectious material into both nostrils. A rather severe illness resulted, but the results have apparently not been recorded.[40]

Warren and Margaret Lewis worked together and worked separately over the years. The flow of their work and ideas was almost like a braided river with its separation and rejoining. Thus, like the river analogy, it is impossible at any particular stage to assign responsibility for work. However, I have chosen three advances in knowledge of cells derived from tissue culture which seemed to be prominently related to Warren Lewis. These are: (i) the determination that monocytes are identical with fixed phagocytes and the



Warren and Margaret Lewis

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[40] M. K. Gey, Personal communication, 1976.

epithelioid cells of tuberculosis;[41] (ii) the phenomenon of pinocytosis;[42] and (iii) the successful continuous growth of normal and malignant cells in culture.[43]

If we turn back to our present-day diagram of the relationship of the immune cells and the macrophages, it is difficult to realize that the clasmatocyte or macrophage was ever considered to be different from the blood monocyte. Yet they really do appear to be very different cells when they are observed within a blood smear and in tissues. Finally, there are the epithelioid cells, which are so designated because the large areas of clear cytoplasm are so reminiscent of epithelial cells.

Tissue cultures of the three types of cells in the Carnegie Institution were done against a background of intense interest in the vital staining of cells initiated by Dr. Florence Sabin and Dr. H. M. Evans in the Department of Anatomy. This work has made it possible to recognize supravitally stained tissue macrophages by the rosette which the granules and the short mitochondria formed. Cultures of the macrophages from a large variety of animals, including cunners, toads, frogs and lizards, had shown that the phagocytic cells had a standard morphology when in culture.

Culture of chicken blood, especially of the buffy coat, by Mrs. Lewis had shown the transformation of the blood monocytes into active macrophages, indistinguishable from the tissue macrophages.[44] These cells ingested the red cells within the culture, grew in size, and developed granules. All stages of change could be found. The third cell type, the epithelioid cell, which becomes a giant cell of the Langerhans type, was identified as derived from macrophages in the same way, i.e., by cultivation of the cells from experimentally infected rabbit lung and tuberculous lymph nodes from human patients. By following individual cells, the transition to macrophages was apparent; again identification was made on the basis of inclusions, neutral red staining and Janus green staining of mitochondria.

As Lewis himself said: "The idea that these cells come from monocytes was of course not new, . . . but the methods used allowed a clear elimination of alternative explanations."[45] The description of pinocytosis by Lewis is so clear that I prefer to quote from his short article:[46]

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[41] W. H. Lewis, "The transformation of mononuclear blood cells into macrophages, epithelioid cells, and giant cells," *The Harvey Lectures*, 1926, *21*: 77-112.

[42] W. H. Lewis, "Pinocytosis," *Bull. Johns Hopkins Hosp.*, 1931, *49*: 17-26.

[43] W. H. Lewis, "Malignant cells," *The Harvey Lectures*, 1936, *31*: 214-234.

[44] Lewis, *op. cit.* (n. 41 above).

[45] *Ibid.*

[46] Lewis, *op. cit.* (n. 42 above).

> The fluid globules which are taken up at the periphery of the wavy veil-like membranous pseudopodia seem to get caught or trapped in the folds of the ruffle, which probably then fuse around and completely enclose them. This process takes only a few seconds. The globules are at first irregular in outline but they soon become circular and probably spherical in form, presumably after they have become pinched off from the surface layer and lie free in the cytoplasm of the pseudopod but surrounded, however, by a thin surface film which was originally a part of the surface membrane of the cell. One cannot see exactly how the folds enclose and fuse about a globule of fluid, but there can be no doubt about the fact that globules do actually get into the cell and then move rapidly towards the center. . . . As soon as the globules or vacuoles of fluid become enclosed within the pseudopod, they move toward the center of the cell with gradually increasing speed and within one to five minutes reach the periphery of the zone of fat globules and granules which surround the central area and nucleus. The globules usually penetrate into the granular zone and then rapidly decrease in size so that in the course of a few minutes (1 to 5) they become reduced to granules which are lost among the other granules and fat globules.

The word "pinocytosis" is now often subsumed under the larger word "endocytosis," but the phenomenon of fluid ingestion is recognized as playing a major role in the physiology of cells.

From the very first days of excitement in tissue culture, the idea of determining the difference between normal and malignant cells had been uppermost in the minds of the pathologists. MacCallum, at that time at Columbia University, had started Lambert off by having him trained in Harrison's laboratory at Yale.[47] Burrows theorized about it. Carrel publicized it, but did not succeed in growing any cells except for the infectious Rous sarcoma for short periods of time. Warren Lewis started by growing rat tumors obtained mostly from Dr. George Walker, who at that time was following the transplants from one animal to another.[48] Growth of tumors in tissue culture was usually associated with presence of a great number of other cells. Often the first outgrowth contained a large number of macrophages[49] or even normal fibroblasts. Lewis, in his second Harvey Lecture in 1936,[50] reported that he had "slowly learned to distinguish the malignant cells of each tumor from such normal cells as happened to grow out in the cultures or were obtained in cultures from normal tissues." By selection and persistence tumor cell lines were established and Lewis concluded:

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[47] Harrison, *op. cit.* (n. 17 above).

[48] Lewis, *op. cit.* (n. 43 above).

[49] W. H. Lewis and G. O. Gey, "Clasmatocytes and tumor cells in cultures of mouse sarcoma," *Bull. Johns Hopkins Hosp.*, 1923, *34*: 369-371.

[50] Lewis, *op. cit.* (n. 43 above).

> The cultivation of pure colonies of malignant cells *in vitro* for month after month and year after year without appreciable change in the visible cytological characteristics and malignancy of the cells, especially in media that contain no trace of serum or plasma or juice of the species of animal in which the tumor originated certainly supports the idea that malignant cells are permanently altered cells.

George Gey was the direct intellectual descendant of Warren Lewis. He worked with him from the time he was in medical school until the time of Lewis' retirement in 1937. He established three unique laboratories at Johns Hopkins. The first was in a former janitor's room in the Carnegie Institution; the second was in an abandoned histological laboratory. These two he almost literally built with his own hands. The third was built for him in the Department of Surgery at the specific insistence of Dr. Alfred Blalock. In these different laboratories, where he and Margaret Gey worked almost continuously for more than forty years, a variety of human and rat normal and malignant cells were established and maintained for many years.

It is not possible, as in the case of the Lewises, to separate the contributions of George and Margaret Gey. He stimulated new studies,

George and Margaret Gey

developed new ideas and techniques, she maintained the laboratory on an even keel. He ranged far into new areas, such as the effect of electron sterilization of biological media, or the necessity for a new substrate, while she, as a chief surgical nurse and trained registered technician, established procedures and routines necessary for long-term maintenance of cells.[51] This third part of the story will then speak of a very clearly combined effort.

George Gey was never interested in short-term experiments. They must ask large questions, and therefore the experiments went on for years. It was like having the end of a ball of yarn, one that continued to unwind. If one met a tangle of impossible knots in the yarn, it was a stimulus to invent a new method of untying the knots and picking up the thread. I do not remember an experiment in the laboratory that was ever finished, and of course no one ever does get the final answer. So George's colleagues would have to cheat a little and conspire to have him present a paper somewhere so that he would have to face up to finishing one or another phase of an experiment in order to say something conclusive. Of course knowledge is not discontinuous and it was we who were confused, not George! The eagerness of the chase after some new thing led him on until the last day.

George and Margaret Gey's major contribution jointly to Johns Hopkins was the education of numerous scientists in what could be done in tissue culture. It is shocking to know that until the present day of HeLa popularity George Gey was better known abroad than at Johns Hopkins. There were good reasons for this. He was the first to demonstrate in vitro carcinogenesis,[52] but he never wrote an article on the subject. He made the first phase microscope movies of mitochondrial behavior in normal and malignant cells, but published primarily the movies themselves. He cultivated human malignant cells in tissue culture, long before HeLa. Then he cultivated this famous cell, but his first publications on HeLa were an abstract co-authored by Ward Coffman and Mary Kubicek,[53] and an article on the susceptibility of HeLa to poliomyelitis which was written as a sort of addendum to Syverton and Scherer's paper on this subject.[54] This was done on Syverton's insistence, since there had been no prior publication on HeLa.

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[51] G. O. Gey and M. K. Gey, "The maintenance of human normal cells and tumor cells in continuous culture. I. Preliminary report: Cultivation of mesoblastic tumors and normal tissue and notes on methods of cultivation," *Amer. J. Cancer*, 1936, *27*: 45-46.

[52] G. O. Gey, "Cytological and cultural observations on transplantable rat sarcomata produced by the inoculation of altered normal cells maintained in continuous culture," *Cancer Research*, 1941, *1*: 747.

[53] G. O. Gey, W. D. Coffman, and M. T. Kubicek, "Tissue culture studies of the proliferative capacity of cervical carcinoma and normal epithelium," *Cancer Research*, 1952, *12*: 264-265.

[54] W. F. Scherer, J. T. Syverton, and G. O. Gey, "Studies on the propagation in vitro of poliomyelitis

In 1938, while following the growth of normal rat fibroblasts in roller tubes derived from the subcutaneous areolar tissue of a rat, he observed the first "spontaneous" change of normal cells into malignant cells in tissue culture. Since he was closely familiar with the differential growth of normal and malignant cells, the occurrence of this change in cell appearance and behavior was readily recognized and inoculations of these altered cells into the same inbred strain of rats from which the cells had been taken showed that the cells were malignant. This change to malignancy was not, however, an abrupt and unique event. It occurred on glass in different containers and at various times. The sequence of these various events was followed very closely by Gey, and he was able at any time to produce the sequence for you out of his head. Indeed, there are napkins and pieces of wrapping paper scattered all over the world detailing this sequence of events. But none of the napkins was ever transferred into a published diagram for a published record of the first recognition of the conditions under which normal cells changed into malignant cells. Entirely independently, Wilton Earle at the National Institutes of Health had for years been interested in chemical carcinogenesis and had set up cells in vitro to see if the process could be followed in culture.[55] He too found that spontaneous conversion of mouse cells, or transformation as it is now designated, was in no way rare. The two men cordially exchanged ideas, long debating the meaning of it all. Earle and his associates described the changes in some detail.[56] Gey was so deeply involved in what was happening and in building such carefully developed machines to test one or another hypothesis, that much of the basic knowledge remained on napkins and scrap paper. Years after, with the help of Mrs. Gey, a diagram of sorts was reconstructed, so that those of us who used the cells might delineate the events.

We return to the fascination of the cells themselves. The difference in these various lines of cells, as seen by phase movies, was documented in many reels of film, made with a movie machine built, with aid from the Crown Cork and Seal Company of Baltimore, in the basement of the Department of Pathology. These became the record of cells themselves. Phase optics was applied very early to the study, with the direct help of Zernike, the Dutch Nobel laureate in physics who developed phase

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viruses. IV. Viral multiplication in a stable strain of human malignant epithelial cells (strain HeLa) derived from an epidermoid carcinoma of the cervix," *J. Exp Med.*, 1953, *97*. 695-710.

[55] K. K Sanford, "Malignant transformation of cells *in vitro*," *Internatl. Rev Cytol.*, 1965, *18*: 249-311

[56] W. R Earle, "Production of malignancy in vitro; mouse fibroblast cultures and changes seen in living cells," *J. Natl. Cancer Inst* , 1943, *4*: 165-212

microscopy, with visits and constant consultation. This tool was used to study pinocytosis. The activities of fluid droplets from their first phase-negative state to a phase-positive denser form could be directly observed, studied and restudied. Excitement was high as essential movie records followed the web-like interaction of mitochondria. The movie data of less experienced workers was sometimes confusing. I well remember a visitior who said that he had a beautiful movie of "reverse pinocytosis"; Gey asked him if it was possible that he had run the movie backwards. We never heard any more of that particular example of exocytosis. At this time Dr. Firor was "boss." He had been responsible for getting the Geys to move to the Dispensary Building and build their laboratory there as part of the Department of Surgery, following the retirement of Dr. and Mrs. Lewis from the Carnegie Institution. He remained an enthusiastic backer of the work to the end. However, sometimes frustrated in his desire to see full recognition of the work, he was driven to take the movies to a surgical or cancer congress himself, and then arrange to have the work published. Thus one of the first descriptions of in vitro carcinogenesis was that of Firor and Gey in the *Annals of Surgery* in 1947.[57]

We still do not know the cause or causes of in vitro carcinogenesis. It is so common that it has been suggested that a cultured cell must either change towards malignancy or eventually die in culture. Yet, 14pf, a cell line that seems normal in appearance but which has changed its chromosome number in adjustment to life outside the original rats, lives on and is used for all sorts of studies, including the growth of rat leprosy bacilli in tissue culture.[58]

An elaborate lead-lined chamber with a roller tube apparatus inside was made to test the question of whether carcinogenesis in vitro was in some way related to irradiation. This did not yield useful data. Several students in the Gey laboratory later showed that transformation from normal to malignant cells would often occur within six months of setting up the culture on glass. It was never possible to grow these cells on a completely artificial medium, so the role of viruses was never excluded. Some forty years later the phenomenon of in vitro transformation is still not understood. However, a comparison of the susceptibility of the cell lines to a virulent virus, eastern equine encephalomyelitis, did show a startling difference in the different cell derivatives to this virus. The standard tumor, T-333, was rapidly destroyed by the virus, whereas the normal cells in the same medium showed little or no susceptibility.[59] Some of the intermediate (ANSAT, or altered normal subcutaneous areolar tissue) lines were intermediate in susceptibility. This led to new experiments in which infected cultures were transferred to new medium and new roller tubes. This led to the recognition that after cells had apparently been completely destroyed by virus, new cells could grow out and these new cells were not at first susceptible to virus. Thus chronic infections were established. This study of the cyclical destructive effect of one cell line by one virus with an intervening recovery phase (or self-cure) helped Isaacs[60] in England some years later to search for an interfering factor in other culture systems and, with Lindemann, to discover interferon. Though not as famous as HeLa, these rat cell lines are still highly useful. Their differential behavior in vitro and in plastico are currently under study by Won Young Lee for his doctoral thesis in our department.

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[57] W M Firor and G O. Gey, "The study of malignant cells with phase difference microscopy," *Annals Surgery*, 1947, *125* 604-607

[58] E W. Garbutt, R. J. W. Rees, and Y. M. Barr, "Growth of Mycobacterium lepraemurium maintained in cultures of rat fibroblasts," *J. Gen Microbiol*, 1962, *27*. 259-268

Like the laboratory of the Lewises, the Gey laboratory became host for a variety of studies which cannot be covered here. Roller tube cultures of a four-month old strain of human fibroblasts were made available to me while I was a medical student in 1935-39, and these cells developed beautiful vesicles in which the "virus" of lymphopathia grew for seven months. This was the first long-term growth in tissue culture of this agent and it established the feasibility of such studies.[61] A student in the School of Hygiene, Robert Lewert, grew the cells of chicken pia mater,[62] so that the poorly understood exo-erythrocytic form of bird malaria could be studied.[63] After World War II, John Hanks, desirous of growing human leprosy in tissue culture, developed new methods of growing cells at lower temperatures.[64] Honor Fell came visiting from England, lectured on her wonderful story of cell modulation by vitamin A, and demonstrated the wide horizons of organ cultures. I was then able to work abroad using her techniques, thus introducing organ cultures of human embryonic tissues into the study of virology.[65] Finally, Renato Dulbecco came to learn tissue

[59] F. B Bang and G O Gey, "Comparative susceptibility of cultured cell strains to the virus of Eastern equine encephalomyelitis," *Bull. Johns Hopkins Hosp.*, 1952, *91*: 427-461

[60] A. Isaacs, "The role of interferon," *Endeavor*, 1963, *22*. 96

[61] G O. Gey and F. B Bang, "Experimental studies on the cultural behavior and the infectivity of lymphopathia venerea virus maintained in tissue culture," *Bull Johns Hopkins Hosp.*, 1939, *65*. 393-417

[62] R M. Lewert, "Alerations in the cycle of *Plasmodium gallinaceum* following passage through tissue culture I Tissue culture studies," *Amer. J Hyg.*, 1950, *51*: 155-177.

[63] *Ibid.*

[64] G O. Gey, J H. Hanks, and R. Barrett, "Retardation of growth and metabolism of normal and malignant cells during continuous cultivation," *Growth*, 1948, *12*: 69-105.

[65] F B. Bang and J S. Niven, "A study of infection in organized tissue cultures," *Brit. J exp Pathol.*, 1958, *39*: 317-322.

culture, learned about the plaques produced by eastern equine encephalitis,[66] and adapted these methods for the basic molecular virology that is standard the world over.

Cancer of course has intense personal meaning to human beings and sparks the eternal drive to study the human disease. George Gey chose to work in the Department of Surgery because it was through direct association with surgeons that he had access to human material and could follow the development of discrete but spreading tumors. The capability of determining the basis of the behavioral difference between normal and malignant tissue from the same individual was a goal that was always there.

The laboratory had established two human tumor lines in cultures before the advent of HeLa. This particular cell line, which has become almost as standard as the laboratory mouse, was established during a fairly routine study of normal and malignant cells in culture. Mary Kubicek, a recently trained technician in the laboratory, was given the job of growing normal and malignant cells in chicken plasma clots, using the biopsy material from the human cervix. Dr. Richard TeLinde, at that time head of the Department of Gynecology, urged such studies and arranged for a continuing supply of material. Many cultures were made of cases of early and late cancer. The famous biopsy material was brought in casually one day at noon hour, lunch was interrupted, and somewhat reluctantly the cultures were set up.[67] Methodologically there was no deviation from the previous procedures or from those of subsequent tests. Yet, in this case, good cells were growing out in 4 days, and unlike other cultures, the cells continued to grow at an uninterrupted rate, no pause, no change in activity, just continued to grow in the original plasma clot medium. Now, of course, HeLa cells can be grown by almost anyone capable of trypsinizing cells and transferring them from one tube to another. Indeed they grow so well that in inexperienced hands they have become laboratory contaminants of other cells, and have not infrequently had to be identified by their particular enzymes[68] when a new cell line is being established. They never became a problem of contamination in the Gey laboratory, not only because careful technical checks on mix-ups had been firmly instituted in the laboratory many years before, but also perhaps because they were never transferred in that laboratory by trypsinization.

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[66] G. O. Gey and F. B. Bang, "Viruses and cells—A study in tissue culture applications I. Cells involved—Availability and susceptibility," *Trans. N. Y Acad. Sci.*, 1951, *14*: 15-24

[67] M Kubicek, Personal communication, 1976

[68] S H. Hsu, B Z Schacter, N. L. Delaney, T. B. Miller, V A McKusick, R. H Kenneth, J. G. Bodmer, D. Young, and W F. Bodmer, "Genetic characteristics of the HeLa cell," *Science*, 1976, *191*. 392-394

HeLa was and is different. Subsequent attempts to grow about ten other carcinomas of the cervix established two cells lines which could be coaxed into continued growth on a collagen substrate. Normal cells, or even carcinoma in situ, studied by Raafat under the Gey tutelage, never produced such a continuous growth, even though initial growth was usually achieved.[69] The nature of the uniqueness of HeLa remains unknown. The immune system of the lady, who provided the original cells which are now spread so widely around the world, was unable to check the progressive growth of her own malignant cells. When she came to post mortem later, there were attempts to grow the cells from a variety of tissues, but whether because of the terminal uremia from which she suffered, or for other reasons, this time the cultures did not succeed.

Glass is a most unsatisfactory substance for the growth of cells. It is a substance to which cells react. Macrophages become angry on it, spread out, and attempt to phagocytize it. Chick cells die within a few generations of exposure to it. Rat cells become malignant on it. Plastic is not much better as a substrate on which one can expect cells to behave perfectly normally. After all, all sorts of plastic are carcinogenic in vivo. Yet it is transparent, so we may observe the cells through it. Many of the early tissue culture workers were able to keep their cells in a fairly contact-free situation by growing them in chicken plasma clots. But chicken plasma clots are digested by the growing cells, and the clots must be reconsituted by adding new plasma-containing embryo extract in order to provide a new structure within which the cells will grow.

Gey sought and found a new substrate for the growth of cells: collagen reconstituted from rat tails.[70] In a laboratory where rats were kept in constant supply for testing the malignancy of the rat cell lines and for carrying established rat tumors, what could be more convenient? With the advice of Jerry Gross of MIT, and the help of Robert Ehrmann, the tendons of the rat tails were teased free, dissolved in acetic acid, dialyzed, and the sticky fluid reconstituted on the inside of a roller tube, so that it was stable, transparent, and could be washed and made ready for cell growth. I believe that this technical advance will eventually become as important a contribution as any made in the Gey laboratory.

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[69] M. Raafat (Haisha), "Evaluation of some growth potential of carefully defined carcinoma-in-situ of the human uterine cervix in tissue culture," Thesis for the Doctor of Science degree, 1965 Baltimore, Md.: The Johns Hopkins University School of Hygiene and Public Health

[70] R L. Ehrmann, and G. O. Gey, "The growth of cells on a transparent gel of reconstituted rat-tail collagen," *J. Natl. Cancer Inst* , 1956, *16*. 1375-1403.

First, it made it possible to grow in tissue culture a number of human tumors that apparently resisted growth on even the cleanest of glass.[71] Secondly, differentiated cells, such as liver cells, grew well on this surface and maintained their liver-like morphology.[72] Thirdly, chick fibroblasts, which died when placed as small explants directly on glass, lived and grew and spread over the surface of the collagen, stopping when they came to glass. Thus in the last piece of published work to come from the Gey laboratory,[73] it was shown that chick cells will survive for five years or more on a collagen substrate, even though they have not yet taken on the claimed immortality of Carrel's heart fibroblasts.

Finally, the use of this natural substrate allows one to study the capacity of different cells to digest collagen, a subject of central interest in the study of chronic collagen vascular disease.

One of George Gey's greatest attributes was his contagious enthusiasm for examining the marvelous machinery of the cell. This he carried with him on television, into the shops of Crown, Cork and Seal, where all sorts of marvelous apparatus was made, and finally to convince potential donors for the scientific study of the cell. Mrs. Milton Edgerton, wife of the professor of plastic surgery, worked in the Gey Laboratory and shared in the enthusiasm. Her mother, Mrs. W. Alton Jones, then donated 5.2 million dollars to the Tissue Culture Association, of which George Gey was the first president. The money was used for the establishment of the W. Alton Jones Cell Science Center at Lake Placid, New York. This Center now sets standards in tissue culture for the entire country, and carries on the spirit of teaching and exposition, so much a part of the life of the Gey Laboratory.

My review of the story of tissue culture at Johns Hopkins has involved a number of sections of the university and has been incomplete and biased by close personal acquaintance with some of the people. But it does, I believe, allow of some generalizations. The major source of original ideas and of impetus surely derived from the Department of Anatomy, for here it was that Harrison and Lewis, rather independently, started out as experimental embryologists. In the Carnegie Institution's Department of Embryology—a creation of Dr. Mall, the first Professor of Anatomy—the work of the

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[71] Raafat, *op cit* (n. 69 above)

[72] W. D Hillis and F B. Bang, "Cultivation of embryonic and adult liver cells on a collagen substrate," *Exp. Cell Res* , 1959, *17.* 557-560

[73] G O. Gey, M Svotelis, M. Foard, and F. B Bang, "Long-term growth of chicken fibroblasts on a collagen substrate," *Exp Cell Res.*, 1974, *84* 63-71

Lewises continued, but supported by an outside agency. And finally it was here that the Gey laboratory started. In all cases, university support for the research was minimal. During most of her career, Margaret Lewis, who contributed so much independently to the use of tissue culture for the study of pathological processes and the development of knowledge of how cells react with pathogens, and who worked with and trained people within the medical environment, was an employee of the Carnegie laboratory. Also Warren Lewis. The Geys received little or no pay directly from the university or the medical school. At one time they were directly supported by the old U.S. Public Health Service, but after a few years George said there were too many papers to fill out. Indeed, at his death, George Gey was considering a request to found a fourth laboratory at the Good Samaritan Hospital.

What does all this add up to? First, that much fundamental work on cell pathology and cell-virus relationships was possible only because the tissue culture base was there. Second, I believe it re-emphasizes what we all know: good fundamental research, here as elsewhere, must be free of constraints. There must be time to think about what you are doing and time to do it. I believe that each of the people in this series of stories had that in common. In their hands, the potentials of tissue culture expanded within the enormously different approaches of Harrison, who started it all, and Gey, who created three successively important laboratories in the Johns Hopkins Medical Institutions.

Acknowledgments

I am indebted to Mrs. G. O. Gey for much of the material concerning both the Lewis Laboratory and the several Gey laboratories.

Miss Sally Wilens of the Department of Biology at Yale University was extremely helpful in making available the original papers of Ross Harrison, and in this regard I am also indebted to the Sterling Memorial Library of Yale University.

Dr. Jessica Lewis helped in the description of her parents' activities in the laboratory. Dr. Jane Oppenheimer contributed from her wealth of knowledge of the development of Ross Harrison's ideas, and Dr. Virginia Evans furnished data on the relationship between George Gey and Wilton Earle.

Both Dr. George Corner and Dr. John Hanks reviewed the draft of the manuscript and made several helpful comments.

THE FÜNFBILDERSERIE—A BRIDGE TO THE UNKNOWN*

YNEZ VIOLÉ O'NEILL

The Pontifex Maximus of the Fünfbilderserie bridge was unquestionably Karl Sudhoff. Some seventy years ago he, the then professor of the history of medicine in the University of Leipzig, discovered five illustrations and their accompanying texts derived from a medieval anatomical treatise. Terming the work, *Fünfbilderserie,* or the five picture series, Sudhoff published reproductions of his discoveries in 1907.[1] Since then, scholarly attention has focused primarily upon the drawings and somewhat less upon the text of the treatise that Sudhoff had found in two Bavarian monastic manuscripts.[2]

Still, many puzzles concerning both aspects of this curious composition and its variant versions remain. Certainly no attempt to unravel the entire tangled web will be made here, but certain new ideas concerning the earliest known illustrated anatomical treatise to have appeared in the Latin West will be advanced. If, at the outset, it may seem rash to venture new thoughts about a subject at least 800 years old, one may consider the remark of Mademoiselle Rose Bertin, Marie Antoinette's milliner, who reputedly observed that nothing is new except what has been forgotten. In keeping with her maxim, it is the purpose of this paper to review some of what is known about the *Fünfbilderserie,* and to suggest means by which some of what has been forgotten about the so-called five picture series may be recalled.

Ostensibly, Sudhoff was extraordinarily lucky in his research on this

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[1] Karl Sudhoff, "Anatomische Zeichnungen (Schemata) aus dem 12 und 13. Jahrhundert und eine Skelettzeichnung des 14. Jahrhunderts," *Studien zur Geschichte der Medizin, Heft I: Tradition und Naturbeobachtung in den Illustrationen medizinischer Handschriften und Fruhdrucke vornehmlich des 15 Jahrhunderts,* (Leipzig Barth, 1907), pp 51-59, plates 13 and 14 (hereafter cited as "Zeichnungen").

[2] Listings of some of these studies are in Otto Kurz's "The medical illustrations in the Wellcome manuscripts," Appendix 2, *J. Warburg and Courtauld Institutes,* 1942, *5* 137-142, and more recently in Robert Herrlinger's *History of Medical Illustration,* English Language Translation (New York Editions Medicina Rara, 1970), pp 10-14.

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topic. Unlike those of us who seem always to fall into the middle of things, he believed he had found the earliest examplar first, and began with characteristic Teutonic efficiency to consider it and its accompanying drawings. His initial study centered on a twelfth-century copy which was written in the Benedictine monastery of Prüfening, and is now part of a codex preserved among the treasures of the Bavarian State Library at Munich.[3]

In another prize of that collection, Sudhoff uncovered a thirteenth-century version of the treatise and of its illustrations. This manuscript was written in the Bavarian Benedictine monastery of Scheyern around 1240 by a monk named Conrad. We are certain of the scribe's identity as he not only signed his work, but inserted a picture he had drawn of himself into it. Thus, in the tableau of the enthronement of the Virgin, we find black robed little Conrad interposed modestly between Saint Elizabeth and Saint Anne, kneeling as a dutiful vassal who petitions his heavenly queen.[4]

That Conrad used the Prüfening manuscript as his model is uncontested. The similarity between the drawings in the two codices is unmistakable.[5] In both, a series of five schematic figures, posed in a squatting posture, depicting the venous, arterial, osseous, nervous, and muscular systems is presented, and in both, the text describing the systems is written around the figures.

Subsequent to his preliminary study, Sudhoff launched a concerted effort to identify other examples of the medieval treatise. Within little more than three years after his initial publication on this subject, he had assembled texts from five manuscripts and had edited a collated recension of the treatise and of the captions to the figures.[6] Beginning with the two versions he had discussed in his earlier study of the texts in the Munich manuscripts,[7] he compared them with texts he had discovered in several additional sources.

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[3] Cod. lat. Monacensis 13002 A scholarly account of the contents of this codex is in Albert Boeckler's *Die Regensburg-Prüfeninger Buchmalerei des XII und XIII Jahrhunderts* (Munich: A Reusch, 1924), pp 20-29

[4] Cod lat Monacensis 17403, f *7r*

[5] Gerhard Baader, "Mittelalterliche Medizin in bayerischen Klöstern," *Sudhoffs Arch* , 1973, *57* 282-283, Josef Hemmerle "Die Benediktinerklöster in Bayern," *Germania Benedictina*, II (Augsburg Winfried-Werk, 1970), p 274, Hans Swarzenski, "Die lateinischen illuminierten Handschriften des XIII Jahrhunderts in den Ländern an Rhein, Main und Donau," *Denkmaler deutscher Kunst Die deutsche Buchmalerei des 13 Jahrhunderts* (Berlin, Deutscher Verein für Weissenschaft, 1936), p 2

[6] Karl Sudhoff, "Abermals eine neue Handschrift der anatomischen Fünfbilderserie, *Arch f Gesch d Med* , 1910, *3* 361-366 (hereafter cited as "Abermals")

[7] Sudhoff, "Zeichnungen," pp 55-59

One of these versions he had found in a small fourteenth-century medical anthology now in the Saxon State Library at Dresden.[8] Written in many different hands, the Dresden codex contained an incomplete text of the treatise, as the section dealing with the muscular system was missing, and only one figure, a skeleton which, though placed in the typical squatting attitude of the Munich drawings, represents a more naturalistic tradition.[9]

Reproductions of still another copy of the illustrated treatise must have reached Sudhoff in or before 1909, as he also included in his recension the four sections of the text which had been found in *Ashmolean Codex 399* of the Bodleian Library at Oxford. Though it is doubtful that Sudhoff ever thoroughly examined the Ashmolean codex, he tells us somewhat later, in his memorial to Joseph Frank Payne, that he had had a rotograph of the complete manuscript and of its illustrations for a number of years.[10] In any event, as the version in the Ashmolean codex lacks the introduction and the first sections of the text, Sudhoff was fortunate in discovering another manuscript copy whose text he believed to represent an improvement over those in the two Bavarian cloister codices.

This version, which he found in a late fourteenth-century compendium of medical writings belonging to Prince von Lobkowitz, Duke of Raudnitz, is now preserved in the manuscript collection of the University Library at Prague.[11] Though it was transcribed at a much later date, the text of the treatise preserved in the Prague codex is not unlike those found in the cloister codices, but does display certain distinctive characteristics and textual variants, many of which Sudhoff preferred over corresponding words and phrases he had found in the texts he had studied previously.[12]

The figures in both the Prague and the Ashmolean manuals certainly bear kinship to the illustrations in the Munich examplars, if the squatting postures in these later versions be less pronounced. The captions are entirely missing from the figures of the Ashmolean series, but while some of those in the Prague manuscript are similar to the titles written on the figures of the Munich codices, others reveal either new knowledge or additional sources.[13] A conspicuous pictorial innovation in these later versions is the unusual and striking use of color to differentiate the various structures.

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[8] Karl Sudhoff, "Der Text der anatomischen Bilder aus Prüfening (Prüfling) und Scheyern in weiterer handschriftlicher Uberlieferung," *Studien zur Geschichte der Medizin*, Heft IV. *Ein Beitrag zur Geschichte der Anatomie im Mittelalter speziell der anatomischen Graphik nach Handschriften des 9. bis 15. Jahrhunderts* (Leipzig; Barth, 1908), pp. 5-10 (hereafter cited as "Text").

[9] I examined Ms Dres. C. 310 in 1974, and though the drawing of the skeleton is still quite clear, the text which Sudhoff transcribed is, for all practical purposes, lost. The pages on which it was written are water stained, and the script illegible. An earlier but scholarly description of the codex is in Franz Schnorr's *Katalog der Handschriften der Kongl. Oeffentlichen Bibliothek zu Dresden*, I (Leipzig: Barth, 1882), p. 235.

[10] The extent of Sudhoff's knowledge concerning this manuscript, and his relationship with Payne in this matter is unclear. Some of Sudhoff's statements on these subjects are in "Abermals," p. 354, "Text," pp. 8-9, and note 1, "Drei noch unveroffentlichte Kindslagenserien des Soranos-Musio aus Oxford und London," *Arch. f. Gesch. d. Med.*, 1911, *4*: 112-116 (hereafter cited as "Kindslagenserien"), and "Die Oxforder anatomische Funfbilderserie des Cod. Ashmol. 399," *Arch. f. Gesch. d. Med.*, 1914, *7*: 363 (hereafter cited as "Oxforder").

[11] Klementinum MS R. VI. Fc. 29.

[12] The number and various forms appear in Sudhoff's collated text, "Abermals," pp. 361-366.

The cross-eyed frogmen of the Ashmolean series were drawn very likely in England toward the end of the thirteenth century, and then, it would appear, the colors and the background text which does not refer to the figure or to anatomy, were both added at a later date.[14] Though the chromatic effects on the figures of the Prague series are most remarkable, I can only describe them for you as facilities for producing colored slides were not available at the University Library in Prague when I studied this manuscript during September 1974. The colors chosen for use in tinting the arterial figure, for example, are red, rose, turquoise, and black. The rete, or the network of arteries in the brain, like most of the cardiac region is painted a vivid crimson, as are the lips. The peripheral arteries are rose, and the windpipe and the five-lobed liver are both turquoise.

The striking and distinctive coloration of the figures in the Prague and Ashmolean codices did not deter Sudhoff from hypothesizing a proximate connection between the drawings in these later manuscripts and those he had discovered earlier. He found also a resemblance between the first figure of the Ashmolean series, and the rude sketches which appear on the verso of a sheet depicting a medieval cautery series which Sudhoff was studying concurrently.[15] Though the rough outlines on this terminal page of what is now *Codex e Museo 19* of the Bodeleian Library are unaccompanied by explanatory text, they were included in Sudhoff's list of manuscripts containing the so-called five picture series with which he began his collated edition of the text.[16]

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[13] The captions in the Prufening and Scheyern manuscripts were set forth by Sudhoff in "Zeichnungen," pp 55-59, and those in the Prague manuscript in "Abermals," pp 356-360 Further on in the same article, Sudhoff compared the three sets of captions, *ibid.*, pp 366-368.

[14] Evidence that the color and the text were added after the figures were sketched is the fact that the feet of the men in this series overlap the decorative borders which were obviously then painted around them The text is also carefully written around the figures which do not illustrate or highlight material in the written portion of the page, but are completely independent of it

[15] Sudhoff, "Abermals," p 355, n 1 The drawings on f 165*v*. of Cod. e Museo 19 had been published previously to illustrate E Seidel's and Karl Sudhoff's "Drei weitere anatomische Fünfbilderserie aus Abendland und Morganland," *Arch F Gesch d Med*, 1909, *3* 165-187, and plate 3 The cautery series was discussed and the drawings published in Sudhoff's "Tabellen, Bild-und Merkschemata zur Kauterienanwendung bein Erkrankungen," *Studien zur Geschichte der Medizin*, Heft X· *Beitrage zur Geschichte der Chirurgie im Mittelalter* (Leipzig Barth, 1914), pp 81-90, and plate 34

As he had noticed in his first examination of the series, and is obvious to even the casual reader, the introduction to the illustrated treatise propounds a paradox. Its first words read,

> In the name of the Father, and of the Son, and of the Holy Spirit, here begins the account of incision as Galen, the most skillful of physicians, related it vein according to vein, bone according to bone, muscle according to muscle, nerve according to nerve, and he described them as they are, and separated each one from the other, so that the observer need not err, but might understand and see them in this fashion. Therefore, the first description is of the arteries; the second, of the veins; the third, of the position of the bones; the fourth, of the nerves; the fifth, of the muscles; the sixth, of the genitals; the seventh, of the stomach, liver and belly; the eighth, of the womb; the ninth, of the brain and the eyes.[17]

Thus, the introduction manifests its author's intention of describing nine systems of the body according to Galen. Curiously, however, throughout his investigations, Sudhoff was able to identify texts and drawings for only the first five systems listed. Sudhoff explained this anomaly by intimating that the seventh category was dealt with in the known drawings, and that the organs named, and even the intestines which were not mentioned, appeared in the thoracic cavities of the vein and artery men. He held further that as the medieval author had enumerated the blood vessels, the bones, the muscles, and the nerves in his initial statement, the unknown creator of the text and the drawings probably felt that only those systems deserved major consideration.[18]

This gossamer explanation seems to have quelled Sudhoff's curiosity. Even after publishing other medieval depictions of internal organs,[19] Sudhoff steadfastly maintained that they bore no direct relationship to the *Funfbilderserie* but opined that they might have derived from a similar source. Sudhoff believed that the so-called five picture series originated in Alexandria, that its prototype was a short anatomical textbook composed in

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[16] Sudhoff, "Abermals," p 361.

[17] "*In nomine patris et filii et spiritus sancti incipit historia incisionis sicut dicit Galienus peritissimus medicorum venam secundum venam, os secundum os, lacertum secundum lacertum, nervum secundum nervum, et descripsit ea, secundum quod sunt, et separavit unumquodque ab alio, ne forte erret inspector eorum, sed agnoscat ea ita ut videt Est ergo prima descriptio arteriarum, secunda venarum, tertia positionis ossuum, quarta nervorum, quinta lacertorum, sexta veretri, septima stomachi, epatis et ventris, octava matricis, nona cerebri et oculorum* Cod. lat Monacensis 13002, f 2v

[18] Sudhoff, "Zeichnungen," p. 54, and *cf.* "Text," p 8.

[19] Karl Sudhoff, "Graphische Darstellungen innerer Körperorgane," *Arch f Gesch d. Med*, 1914, 7. 367-372 (hereafter cited as "Darstellungen").

Greek during the third century, B.C., and that the text and its illustrations must have been transmitted from antiquity via Byzantium to the Bavarian monastery of Prüfening where he thought he had found its earliest copy.[20] According to his theory, the depictions of various organs found on a sheet in Pisa, and those scattered through the Ashmolean codex containing the gorgeous if garish five anatomical drawings must also have originated near the Nile delta.[21]

The organs depicted on the Pisan leaf are the stomach with four capsules, each bearing the name of a humor, the gall bladder, the spleen, two sketches of the heart, a five-lobed liver, the trachea, an enlarged drawing of the liver, this one with six lobes, the lungs, a diagram of only the nose and the eyes (Fig. 1), the intestines, and finally, a composite drawing of the reproductive organs (Fig. 2).[22]

Though roughly similar to the Pisan sketches, the comparable drawings in the Ashmolean codex lack almost all explanatory inscriptions, a confusing situation compounded by the fact that the texts written around the illustrations do not refer to them. Considerably more elaborate than their Pisan counterparts, they are not jumbled together all on one page, but are dispersed over ten leaves of the composite work.

Thus, we see on folio 13 verso a full page drawing of the female reproductive organs. (Fig. 3)[23] These are followed some ten pages later with a diagram of the eyes, the nose, and the brain (Fig. 4).[24] Subsequently, we find designs representing the stomach with the four humoral capsules, the liver with a candle-like object representing the gall bladder superimposed on it, and the kidneys.[25] The largest object on the succeeding leaf is also a five-lobed liver which is shown as overlapping the stomach. Directly above

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[20] Karl Sudhoff, "Die fünf anatomischen Abbildungen der Baseler provenzalischen Handscrift," *Studien zur Geschichte der Medizin*, Heft IV, *op. cit.* (*supra*, n. 8), p. 28, and "Abermals," p. 353.

[21] Sudhoff, "Darstellungen," p. 372.

[22] Pisa, Biblioteca Universitaria, Ms. 735 (*olim* Ms. Roncioni 99), f. 2*r*.

[23] Oxford, Bodleian Library, Ashmole Ms. 399, f. 13*v*. Photographic reproductions of this sheet were also published by Christoph Ferckel, "Diagramme der Sexualorgane in mittelalterlichen Handschriften," *Arch. f. Gesch. d. Med.*, 1917, *10:* 255-263, and plate 13; and by Charles Singer, "A thirteenth century drawing of the anatomy of the uterus and adnexa," *Proc. Roy. Soc. Med.*, Hist. Med. Sect., 1916, *9:* 43-47. Although Singer's publication was probably unknown to Ferckel, who was serving as a medical officer in the German army when his article appeared in print, Singer cited Sudhoff's discussion of the text and the diagrams of the fetus in utero which follow it, see Sudhoff, "Kindslagenserien," pp. 112-114. Obviously, at the time he published the article cited above, Singer was convinced it was part of a treatise comprising the text and the diagrams which immediately follow it.

[24] *Ashmole Ms. 399*, f. 22*v*.

[25] *Ibid.*, f. 23*r*.

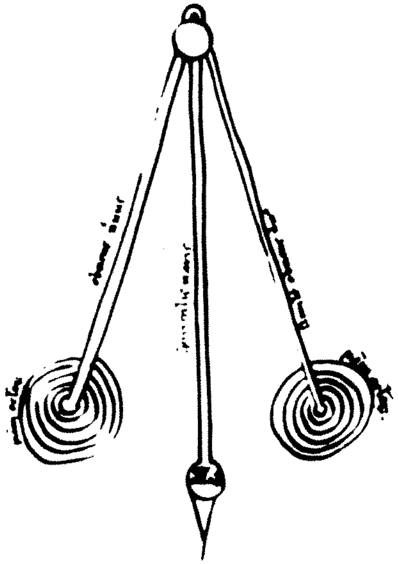

Fig. 1. A truncated sketch of the nose and the eyes. Pisa. Biblioteca Universitaria MS 735, f. 2*r*.

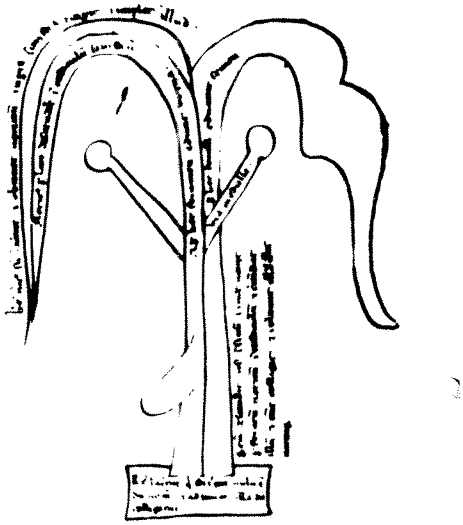

Fig. 2. An amalgam sketch of male and female genitalia. Pisa. Biblioteca Universitaria MS 735, f. 2*r*.

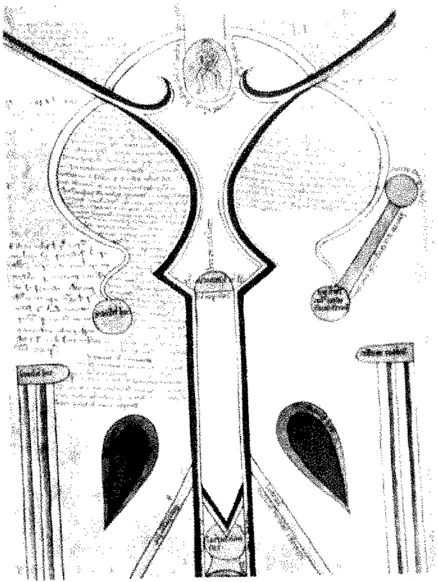

Fig. 3. The matrix and its adnexa. Ashmole MS 399, f. 13*v*.

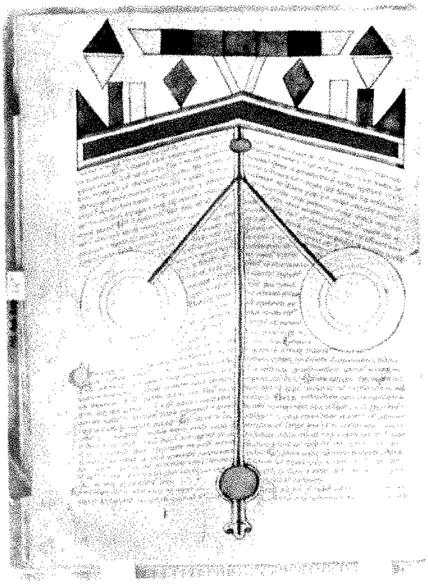

Fig. 4. A diagram of the brain and the eyes. Ashmole MS 399, f. 22*v*.

these two organs is a drawing of the heart half obscured by integumentary lungs. A separate diagramatic depiction of the windpipe running into the lungs points downward toward a large green gall bladder which presides majestically over a sole-shaped spleen.[26] It has been suggested that the blue pyramidal form may be a medieval depiction of the greater omentum,[27] which along with a coiled-cobra-like small intestine complete the illustrations of the next page.[28] Finally, the series ends on the verso of that sheet with a full-page diagram of the male genitalia.[29]

Having considered these drawings, Sudhoff concluded that the Pisan and the Ashmolean series of depictions of various corporeal organs constituted an independent mass of traditional material, that they were neither copied from the *Fünfbilderserie* nor supplementary to it, but though the two series probably shared a common origin, they were transmitted to the West separately.[30] These assertions, which in justice Sudhoff surmized but did not canonize, have become dogma for most scholars who have written concerning the Ashmolean and the Pisan drawings.[31]

Now, the time has come for a change, and I propose to challenge this conjecture by showing that the Ashmolean and the Pisan sketches represent contents from the four missing sections, never identified by Sudhoff, but announced by its author in the preface of the anatomical treatise we have been examining. The basis for my demonstration will be a copy of the treatise containing not five, but nine descriptions of bodily systems which appear precisely in the order in which they were enumerated in the previously cited introductory passages that Sudhoff found in the Prüfening, Scheyern, Dresden, and Prague codices.[32]

The nine picture series appears on five leaves of a small late twelfth or early thirteenth-century codex now in the manuscript collection of Gonville

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[26] *Ibid.*, f. 23*v*.

[27] Cf. Boyd H. Hill, Jr., *The Fünfbilderserie and Medieval Anatomy*, an unpublished Ph.D. dissertation, The University of North Carolina at Chapel Hill, (1963), pp. 148-150, where Mr. Hill suggests that a similar sketch appearing in *Wellcome Ms. 5000* may represent the greater omentum.

[28] *Ashmole Ms. 399*, f. 24*r*.

[29] *Ibid.*, f. 24*v*. Ferckel in *loc. cit. (supra*, n. 8), p. 263 also discussed this figure almost as an afterthought. He published a reproduction of it, *ibid.*, plate 13. Singer's discussion and reproduction of this figure was admittedly an afterthought, see Charles Singer, "Note on a thirteenth century diagram of the Male Genitalia," *Proc. Roy. Soc. Med.*, Hist. Med. Sect., 1916, *9:* 212-214.

[30] Sudhoff, "Darstellungen," p. 372.

[31] Ludwig Choulant, *History and Bibliography of Anatomic Illustration*. Translated and annotated by Mortimer Frank, revised edition (New York: Schuman's, 1945), pp. 52-54; Herrlinger, *op. cit. (supra*, n. 2), p. 13, and Ferckel, *loc. cit. (supra*, n. 8), p. 261.

[32] Sudhoff, "Abermals," p. 361.

& Caius College, Cambridge.[33] Described in 1907—that is the same year that Sudhoff published his first account of the *Fünfbilderserie*—by Montague Rhodes James,[34] the intriguing if excentric antiquary, who seems to have divided his time between cataloguing most of the manuscript collections at Cambridge and writing ghost stories,[35] the manuscript is enclosed in a limp cover of thick skin along with a fifteenth-century copy of several of John Arderne's medical writings. Though Sudhoff discovered what he considered to be a variant version of the five-picture series in a copy of John Arderne's works now at Stockholm,[36] he seems never to have examined the Gonville & Caius treatise, which perhaps in consequence, has been virtually ignored by most subsequent students of medieval anatomy.[37]

To remedy that neglect, let us look at the first figure of the Gonville & Caius series. Designed like its Prüfening, Scheyern, Prague and Ashmolean counterparts to depict the venous system, the drawing is erroneously titled, *historia arteriarum*.[38] Its general composition evokes reminiscences of the Prüfening vein man, for here again, the text is written around the highly captioned figure. Distinctive features may also be noted. An instance is the inscription running laterally down the anterior abdominal cavity, which reads, "the liver clasping the stomach."[39]

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[33] Cambridge, Gonville & Caius College, MS 190/223.

[34] Montague Rhodes James, *A Descriptive Catalogue of the Manuscripts in the Library of Gonville & Caius College*, 1 (Cambridge University Press, 1907), p. 218, and compare with Sudhoff, "Zeichnungen," pp. 51-59.

[35] For a listing of James' works, see *British Museum General Catalogue of Printed Books*, 114 (London: Trustees of the British Museum, 1962), cols. 708-718, where James' catalogues of the Cambridge manuscripts in the following college libraries are cited: Magdalene, Corpus Christi, Gonville & Caius, Jesus, Pembroke, Peterhouse, Sidney Sussex, St. Catherine's, St. John's, Trinity, King's, Christ's, Queen's, Clare, and Emmanuel. Also included in this bibliographical notice are James' *Ghost Stories of an Antiquary* (London: Edward Arnold, 1904); *More Ghost Stories of an Antiquary* (London: Edward Arnold, 1911); and *A Warning to the Curious, and other Ghost stories* (London: Edward Arnold, 1925).

[36] Karl Sudhoff, "Die graphische Weiterbildung der anatomischen Fünfbilderserie aus Alexandrinerzeit und eine anatomische Serie aus Stockholm," *Arch. f. Gesch. d. Med.*, 1915, *8:* 129-139, and plates 3 and 4.

[37] According to Kurz, *loc. cit. (supra*, n 2), p. 138, apart from the description of the manuscript in James' catalogue, only two works consider the Gonville & Caius figures. The first is George Washington Corner's contribution to the *Clio medica* series, *Anatomy* (New York: Paul B. Hoeber, 1930), where the "vein man" of the Gonville & Caius series appears on the frontispiece. I have been unable to find mention of the Cambridge codex in Dr. Corner's text, however, though on pp. 17-18: there is a brief comment concerning Sudhoff's work on the *Fünfbilderserie*. J. G. De Lint in his *Atlas of the History of Medicine* (London: H. K. Lewis, 1926), p. 24 and pp. 28-32, figs. 17 and 22-28, reproduced six pages of the Gonville & Caius manuscript, and commented upon them briefly. As De Lint credited his knowledge of the figures to Charles Singer, it may be presumed that Dr. Singer was acquainted with the Gonville & Caius manuscript, and perhaps even grasped its significance. It is regrettable, therefore, that Kurz's hope, expressed *loc. cit. (supra*, n. 2), p. 138, n. 1, that Dr. Singer would soon publish his discoveries relating to the history of these anatomical illustrations was to remain unfulfilled. Professor Hill, though he included the Gonville & Caius manuscript in his listing of *Fünfbilderserie* versions, *op. cit. (supra*, n. 27), p. 260, erred in citing the manuscript's number. As he makes no mention of the Cambridge codex in his text, and seemed to believe that the missing four pictures consisted merely of insets, *ibid.*, p. 3, it may be assumed that he had never seen the Gonville & Caius example of the nine-picture series.

Vessels radiate from the heart in the figure depicting the arterial system, and terminate like the veins in feathered endings. A blue trachea passes directly to the black heart, and although the liver and the spleen are represented, the stomach is not.[40] The narrative explaining the osseous system is placed between the legs of the skeleton, whose artist may have mistaken a posterior view for an anterior one, replacing the back of the head with a more decorative face.[41]

The sketches depicting the nervous and muscular systems conform to a similar format. They are ladened with descriptive titles, but the bulk of anatomical data on these sheets is conveyed to the reader in the succinct account straddled by the nerve and muscle men.[42] A departure from this convention, as well as evidence that the archetypal treatise comprised nine sections, is found on the subsequent manuscript leaves.

A diagram of the male genitalia fills the verso of the page devoted to the myological system (Fig. 5).[43] All commentary concerning form and function in this graphic representation is proffered in the extensive captions written along the structures outlined. Drawings of various internal organs appear on the following page. These include, precisely as announced in the preface that Sudhoff collated, the stomach, the liver, and the organs of the belly (Fig. 6).[44] Each organ is identified by a descriptive word or phrase, and occasionally these indications contain functional data.[45] The affinity of the figures on this page with their equivalents in the Pisan and Ashmolean series seems incontestable.

Apparently allied also to those sequences are the two subsequent illuminations of the Cambridge codex. The cognation of the eighth Gonville and Caius design[46] with the illustration representing the matrix and its annexia in the Ashmolean manuscript is obvious at first glance (Fig. 7),[47] as

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[38] Gonville & Caius College, MS 190/223, f. 2*r*.

[39] "*epar comprehends stomachum,*" *ibid.* cf. Sudhoff, "Zeichnungen," p. 55 and plate 13 where the corresponding caption in the Prüfening series reads "*epare comprehenditur monoculi.*"

[40] Gonville & Caius College, MS 190/223, f. 2*v*.

[41] Gonville & Caius College, MS 190/223, f. 3*r*.

[42] Gonville & Caius College, MS 190/223, f. 3*v*-4*r*.

[43] Gonville and Caius College, MS 190/223, f. 4*v*.

[44] Gonville and Caius College, MS 190/223, f. 5*r*.

[45] As, for example, the captions "*fauces unde spiramus,*" and "*in stomachum adiuvat eum operationem et ab ipsis v calefit stomachum.*"

[46] Gonville and Caius College, MS 190/223, f. 5*v*.

[47] *Ashmole* MS 395, f. 13*v*.

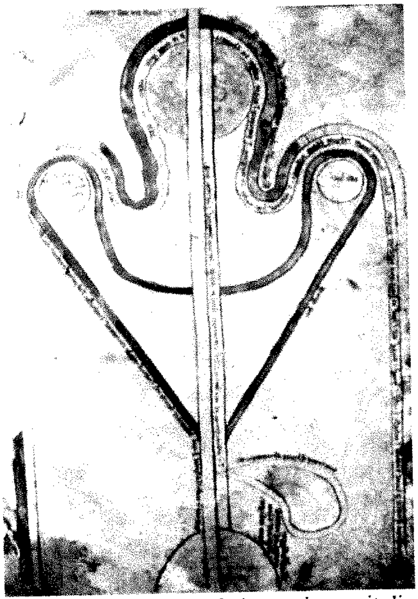

Fig. 5. A diagram of the male genitalia. Gonville and Caius MS 190/223. f. 4*v*.

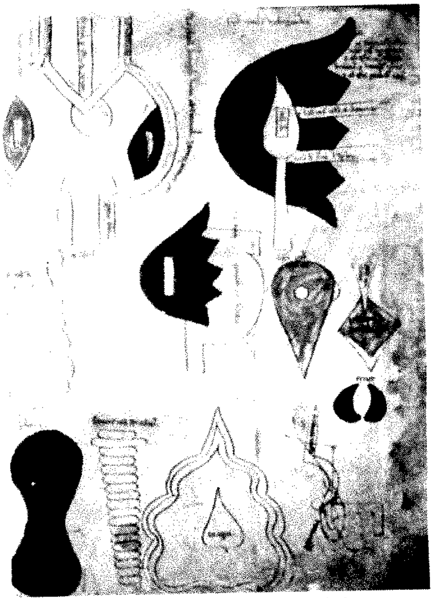

Fig. 6. The stomach, the liver and the organs of the belly. Gonville and Caius MS 190/223, f. 5*r*.

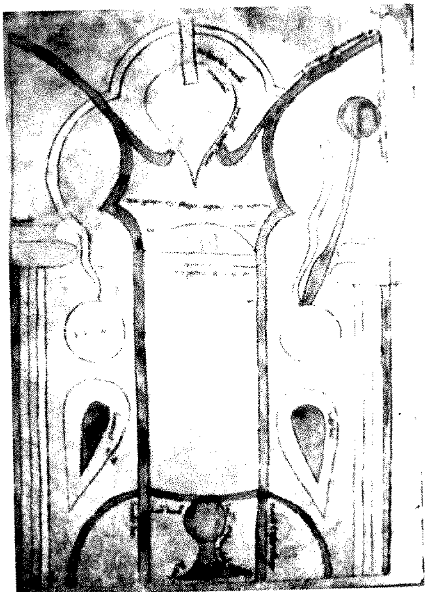

Fig. 7. The matrix and its adnexa. Gonville and Caius MS 190/223, f. 5*v*.

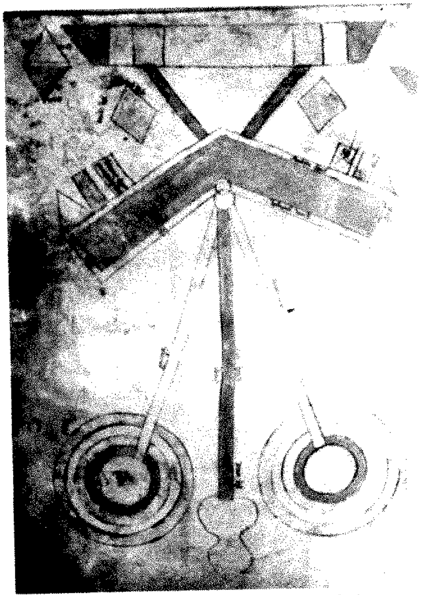

Fig. 8. A diagram of the brain and the eyes. Gonville and Caius MS 190/223, f. 6*r*.

is the relationship of the diagram demonstrating the brain and the eyes in the Cambridge examplar (Fig. 8)[48] with its uncaptioned counterpart in the Ashmolean series,[49] and even with the truncated sketch of the same subject on the Pisan leaf.[50]

The nine integrally preserved drawings of the Gonville & Caius codex, then, constitute an exceptional document for the further study of medieval anatomy. While linking the sketches in the Ashmolean and Pisan exemplars to what traditionally has been termed the *Fünfbilderserie,*" they provide the solution to an enigma posed by the treatise's preface, and puzzling to scholars since Sudhoff's original investigations.[51] By enabling us to pass over that *pons asinorum,* the Gonville & Caius series affords the expectation, like Whittier's bridge, of a covered way that opens into light.

Acknowledgment

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[48] Gonville and Caius College, MS 160/223, f. 6*v*.

[49] Ashmole MS 399, f. 22*v*

[50] MS 735 (*olim* MS Roncioni 99), f. 2*r*

[51] Sudhoff, "Text," p 8; Frank, *op cit* (*supra*, n 31), p. 54, and Hill, *op. cit* (*supra*, n 27), pp 3-4

THE CHANGING RELATIONSHIP BETWEEN MIDWIVES AND PHYSICIANS DURING THE RENAISSANCE.*

THOMAS G. BENEDEK

The quality of the practice of midwifery during the Renaissance may have been generally inferior to the better class of Greek practice some 1300 years earlier, if we assume that the qualifications of a midwife which were recommended by Soranus of Ephesus early in the 2nd century were commonly adhered to. Concern over the practice of midwifery began to be manifested in the latter part of the 15th century by the subjection of midwives to licensing procedures. The specifications in the licenses and the related oaths of office gradually became more detailed, both in their references to ethical behavior and technical knowledge. However, few Renaissance ordinances were so bold as Soranus, who considered an important prerequisite to becoming a midwife to be that "she must be literate in order to be able to comprehend the art through theory too. . . ." Another of Soranus' considerations which became of great concern during the Renaissance was that the best midwives ". . . will be free from superstition so as not to overlook salutary measures on account of a dream or omen or some customary rite or vulgar superstition."[1]

Reasons for the advent of the licensure of midwives are more apparent than is the slowness of the dissemination of the practice from southern Germany, where it originated. Perhaps the most important reason that licensure began during the Renaissance is a quality which defined this epoch—the beginning of a change in attitude from Medieval fatalism to a more investigative, scientific approach to nature. To some extent the growth of governmental concern about the public health was a part of this changing philosophy, which also had the consequence of the institution of licensing procedures for physicians and surgeons. Another consequence was that the inevitability of the high maternal and neonatal mortality rates came to be questioned. If these were susceptible to improvement by intervention beyond prayer it followed that licenses would be required as a medium in

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* Presented in part at the meeting of the American Association for the History of Medicine, Galveston, Texas, May 13, 1976

[1] O. Temkin (transl.), *Soranus' Gynecology* (Baltimore: Johns Hopkins Press, 1956), pp. 5, 7

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which standards of care could be promulgated and as a basis for their enforcement.

A comparison of the conception of Soranus of a good midwife and of the concept that developed during the Renaissance reveals one crucial difference. According to Soranus

> . . . we call a person the best midwife if she is trained in all branches of therapy (for some cases must be treated by diet, others by surgery, while still others must be cured by drugs). . . .[2]

He did not state when the midwife should consult physicians or surgeons. Contrariwise, regulations for the licensure of midwives began in the 16th century to include stipulations that if difficulties are encountered in the management of a delivery the midwife must call "the learned physician" for advice or assistance. Herein lies the paradox which it is the main purpose of this paper to explore. The prevailing mores inhibited physicians and surgeons, since they almost invariably were men, in the examination of women, and prohibited them from examining their genitalia. The midwives possessed a certain amount of practical knowledge in the restricted area of the signs and symptoms of pregnancy, labor and its complications which physicians and surgeons still generally lacked in the 16th century. Nevertheless, improvement of obstetrical care was perceived to require practitioners with practical knowledge but little prestige to consult practitioners without practical and little relevant theoretical knowledge, but greater prestige.

The paradox clearly resulted not only from degrees of occupational prestige, but inextricably also from the relative prestige of men and women in the social order. A study of this relationship is handicapped by the virtual absence of contemporary writings by women. Furthermore, we hardly have even biased descriptions by men of the activities of midwives. They simply were ignored in medical writings until the end of the 16th century. The most candid statement in this regard was made by the French surgeon, Guy de Chauliac (@1300-1368), who wrote that he is unwilling to dwell at length upon midwifery, since that as a rule it is practised by women.[3] Therefore, the defense, unsuccessful though it was, which Jacoba Felicie, a female physician of Paris, advanced in 1322 at her trial for malpractice is of particular interest. She stated that:

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[2] *Ibid.*, p. 6

[3] H Fasbender, *Geschichte der Geburtshilfe* (Jena: G Fischer, 1906; reprint Hildesheim. G Olms, 1964), p. 98.

> It is better and more seemly that a wise woman learned in the art should visit a sick woman and inquire into the secrets of her nature and her hidden parts, than that a man should do so, for whom it is not lawful to see and seek out the aforesaid parts, nor to feel with his hands the breasts, belly and feet, etc., of women; a man should ever avoid and flee as much as he can the secrets of women and of their societies. And a woman before now would allow herself to die rather than to reveal the secrets of her infirmity to a man, on account of the honor of the female sex and of the shame which she would feel. And for these reasons many women and also men have perished of their infirmities, not being willing to have doctors, lest these should see their secret parts. . . .[4]

The argument of Jacoba was that in the existing moral climate female physicians should be available to give women medical care, especially in regard to their reproductive organs.

An anonymous 14th-century Englishman made a similar argument in the introduction to his gynecologic text:

> . . . and whereas women are more feeble and cold by nature than men and have great trouble in child bearing, there often fall to them more diverse sicknesses than to men and, namely, to those who are begetting. Therefore, in the worship of Our Lady and of all Saints I think to do my best to draw from Latin into English diverse causes of their maladies, the signs whereby they shall know them and the cures to help them, after the treatises of diverse masters who have translated them out of Greek into Latin. And because women of our tongue read and understand this language better than any other, and every literate woman can read it to other unlettered ones and help and counsel them in their maladies without showing their disease to a man, I have drawn and written this in English. And if it befalls any man to read it, I pray him and charge him in Our Lady that he read it not to spite or disgrace any women or for any cause but to heal and help them, dreading that vengeance might fall to him as it has done to others that have spied their privities and disgraced them. . . .[5]

Gynecologic texts, as well as the predominantly obstetrical texts which began to proliferate in the 16th century, were written in the vernacular more frequently than texts on other medical subjects because they were intended primarily for a readership which generally was ignorant of Latin, that is, women. Some two centuries after the above quotation a similar opinion about the illiteracy of midwives and the way they may be helped by texts despite this handicap was expressed by Dr. Thomas Reynalde in his prologue to the second edition of *The Byrthe of Mankinde,* the first printed English book on obstetrics, which was published in 1545.

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[4] P Kibre, "The faculty of medicine at Paris, charlatanism and unlicensed medical practices in the late Middle Ages," *Bull Hist. Med*, 1953, *27* 1-20

[5] C. Singer, "Thirteenth century miniatures illustrating medical practice," *Proc Roy. Soc Med*, 1915, *9* (2): 29-47, p 37 (modernized by present author)

> If by my pains this little treatise were made to speak English, as it has been long since taught to speak Dutch, French, Spanish, and diverse other languages in countries where there are few women who can read, they will always have one of these books in readiness, where also this and other such books are sold as commonly at every stationer's shop as any other book. . . . So that to those who diligently will refer and give heed to the instructions of this little book, it may supply the room and place of a good midwife. . . . There have been since the first setting forth of this book right many honorable ladies and other worshipful gentlewomen who have not disdained by the occasion of this book the oftener to frequent and attend women in their labors, carrying with them this book in their hands and causing such part of it as chiefly concerns the purpose to be read before the midwife and the rest of the women then being present. . . .[6]

I mainly want to consider the evolution of the relationship between midwives and male medical practitioners between the middle of the 15th and the beginning of the 17th centuries. The former limit is dictated by the lack of information about Medieval obstetrical practices and the latter because by then physicians and surgeons had begun to make inroads into the practice of midwifery, thereby placing midwives into the inferior position relative to them in which they had only been socially, but not professionally before.

Much of our limited knowledge of midwifery in the 15th and 16th centuries is based on the regulations that were enacted to govern its practice. These, of course, convey the ideal situation as it was conceived either by the legislators or in some instances by the physicians who wrote some or all of the ordinance for the legislators, rather than the practice that actually existed. The early midwifery regulations were urban ordinances, and they began to be enacted in southern Germany a century before similar regulations were developed in other parts of Europe. The first known ordinance on this subject was enacted in Regensburg (Bavaria) in 1452. It did not specify any required training, but did require candidates to be examined publicly by "honorable, sworn women" who also were responsible for supervising them. Their authority stemmed from a higher position in the social order and probably not from the possession of particular technical knowledge. When a midwife encountered complications other midwives were to be called in consultation. Physicians were not mentioned.[7]

Physicians occasionally were involved in efforts to control the quality of the practice of midwives before formal regulations were enacted. The following brief correspondence between the mayor of Frankfurt am Main and his health officer is an example of this.

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[6] T Raynold, *The Byrth of Mankynde, otherwyse named the womans booke* (London, 1545), Prologue, pp 30-32 (modernized by present author)

[7] H W. Freund, "Die Entwickelung der deutschen Geburtshülfe aus der Hebammenkunst," *Klin Jahrbuch* (Berlin), 1891, *3* 32-80, p. 43. Fasbender, *op cit* (n. 3 above), p 80

> Dear Doctor,
>
> Would you examine this woman in the art of the midwives, whether she is skillful therein or not. My gentlemen in Sassenhausen need one.
>
> 15 April 1505.
> Conradt Schyt, Mayor.

Presumably due to an error of one of the correspondents, the physician's reply is dated two days earlier than the mayor's request:

> Dear Sir,
>
> The wife of Heinrich von Stockheim is capable and knows enough to take care of the women as a midwife, according to my examination.
>
> Tuesday, 13 April 1505,
> Henricus Geratwol, M.D.[8]

The ambivalent status of women could hardly have been revealed more succinctly. On the one hand, it is the gentlemen rather than their wives for whom a midwife is requested. On the other, there is the governmental desire for a determination that she is skillful. In some jurisdictions the mayor would have called upon a senior midwife. Requesting the opinion of the medical officer in such a medical matter was a logical alternative. However, his judgement would not have been based on personal competence in midwifery.

The development of written regulations to govern the training, ethics, and activities of midwives would permit them to be evaluated more objectively, but did not alter the lack of relevant experience of the examiners. Most of the extracts from midwifery ordinances to be cited pertain to the relationship between the midwife and the physician and/or surgeon. Such clauses became increasingly common as time went on.

Two undated ordinances from the latter part of the 15th century referred to the relationship between the midwife and the physician. The ordinance of Heilbronn, ignoring the educational level of the individuals to whom it pertained, required:

> #25. So that the midwives be better informed in all aspects they should read their professional books diligently and, when necessary, make use of the information of a physician.

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[8] K Sudhoff, "Aerztliche Hebammenbegutachtung zu Frankfurt a.M. um 1500," *Arch. f. Gesch. d Med.*, 1913, *6* 464.

The following clause of the same ordinance specifies a circumstance in which a physician should be consulted:

> #21 They shall not undertake cutting the frenum of the tongue of a newborn infant without the approval of a physician, whereby because of ignorance many serious injuries are committed.[9]

I have not found a similarly explicit admonition regarding surgery in later ordinances. Apparently, at least in this community, midwives could perform certain kinds of surgery with the approval of a physician. The preceding quotation also indicates that a physician was only to advise, rather than to personally intervene. This was also true in some of the 16th-century ordinances.

The midwifery ordinance of the far more important city of Nuremberg of the same period included the following clauses:

> Midwives, under the threat of corporal punishment, shall absolutely desist from extracting the placenta forcibly. In the event that it does not soon appear spontaneously, they must without hesitation request and seek the counsel of a licensed physician, who after learning the circumstances, can give further orders including in appropriate cases calling an experienced accoucheur.[10]

In this instance the midwife was to call a consultant physician, who, in turn, might be obliged to call a consultant as well. Such obstetrical consultants probably were surgeons, but I have found out nothing more about them in this early period.

The ordinance of Freiburg, a city south of Nuremberg, of 1510 directed:

> When, occasionally, serious problems occur, whether before or after birth, so that the delivery does not procede properly or there is other abnormality, whether of the mother or the child, when advice and assistance are needed no one shall stand on their pride, but shall readily send for other wise women or midwives to seek their counsel. Every midwife, according to her oath, shall come and assist congenially, counsel and act to the best of her ability, unless she is herself occupied by (professional) work or assisting therein and is unable to come without danger.
>
> In the event that she should have the counsel of the learned physician, she should not resist this, but should willingly take steps to prevent that injuries or illness befall either the mothers or children.[11]

---

[9] G. Burckhard, *Die deutschen Hebammenordnungen von ihren ersten Anfängen bis auf die Neuzeit* (Leipzig W Engelmann, 1912), p. 124 (Heilbronn)

[10] *Ibid* , p. 118 (Nuremberg, late 15th century).

[11] K. Baas, "Mittelalterliche Hebammenenordnungen," *Arch. f. Gesch. d. Naturwiss* , 1913, *6.* 1-7 (Freiburg, 1510).

Most of the midwifery ordinances, like this one, went into considerable detail about the midwife's responsibility to the newborn. This particularly concerned the judgement necessary to decide whether she should baptize the infant at once or whether it appeared strong enough to survive to be baptized in church.

In 1555, about a century after the Regensburg town council had enacted the first midwifery ordinance, it was replaced. The new regulation included the following clause:

> Whereas not all emergencies and all sorts of circumstances which may occur before, during, and after the delivery of pregnant and parturient women can be stated in detail in such a short ordinance, and whereas not everything lends itself to public print, and whereas well experienced midwives and other intelligent women can go astray in very dangerous situations, it should be known what they should do. One finds many and varied pertinent skills among the learned doctors of medicine whereby not only difficult labor is eased, but many other things may also easily be helped. Therefore, it also is always advisable in such cases to find knowledgeable, experienced doctors of medicine, and then to heed their advice, because God has given medicine to mankind for its benefit and will not have it disdained.[12]

Here the desire to use an ordinance as a textbook is admitted. Midwives need to consult physicians because it is impractical to draft such a document.

The most elaborate midwifery ordinance up to that time was enacted in Frankfurt am Main in 1573. It was composed by the official city physician, Adam Lonicerus (Lonitzer) (1528-1586). Included among its many paragraphs was the outline of an examination for licensure, a format which was copied by several councils (e.g., Passau, 1595). In the tenth paragraph the midwives were not only instructed when to call a physician, but also in their responsibility if he failed to respond:

> When the situation occurs that she is certain that the fruit in the womb is dead, and the mother is too weak to labor, she shall seek the counsel of a learned doctor of medicine whether the dead fruit may be expelled by medicine and the mother be helped without injury, or it must be removed from her in other ways.
>
> A particular wound-surgeon shall be assigned to the midwives, whom they can consult and use in necessary cases.
>
> When the mother finally has died and departed, and this is quite certain, and no one believes that she lies in a faint, then the assigned wound-surgeon (whom she shall always call at the right time so that he is at hand when it is necessary) shall without hesitation undertake the section and remove the child from the womb.
>
> And if the wound-surgeon should not be at hand, then the midwives themselves shall assume this responsibility.[13]

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[12] Burckhard, *op. cit* (n. 9 above), p 148 (Regensburg, 1555)

[13] *Ibid* , pp 191-192 (Frankfurt a.M., 1573).

Another Bavarian ordinance from the end of the 16th century (Passau, 1595) is unique for the period in proposing the use of the autopsy both to adjudicate possible malpractice and as an educational tool for the midwives.

> In the event that the mother and child remain together (i.e., undelivered), and both have been proved to be dead, it would not be inadvisable for such a person to be cut open by a designated wound-surgeon in the presence of physicians, requiring the attending midwives and the other licensed midwives to be present, all to see such a distressing and pitiable case. This should be desired by the authorities, since the causes and how they came about, whether they occurred due to negligence of the midwives or for other reasons, can thus be learned. Especially, however, the midwives and other women may learn much that is useful and productive therefrom.[14]

The first midwifery ordinance outside the German states was enacted in Paris in 1560.[15] It resembled its contemporaries, but contained a technical detail which I have not found in other ordinances and seems to anticipate the dispute of Semmelweiss about the cause of childbed fever three centuries later. It was required that

> . . . before they begin they must take the rings, if they have any, off their fingers and wash their hands.[16]

In regard to the midwives' need for consultation, the following was required:

> If they realize that the child presents other than head first, which is the normal delivery, or feet first, which is the next most normal delivery, and before the woman is in extremity, they must call for advice either from physicians, or from official master surgeons of the palace in Paris, or from senior mistresses and expert matrons of Paris, and not persons who are ignorant in this matter.[17]

The licensing authority in Paris was delegated in such a way that senior midwives were, at least nominally, the equals of physicians and surgeons. A candidate for the midwife's license was examined by a board which consisted of a physician, two surgeons and two senior midwives. Their decision was by majority vote. The professional oath included the clause:

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[14] *Ibid*, p 250 (Passau, 1595).

[15] R. L. Petrelli, "The regulation of French midwifery during the *Ancien Régime*," *J Hist Med*, 1971, *26* 276-292.

[16] G -J. Witkowski, Histoire des accouchements chez tous le peuples (Paris G Steinheil, 1887), pp 653-656

[17] This passage, aside from the reference to washing, closely resembles Paré's instructions to the surgeon about to prepare himself to perform a delivery: "Then must the Chirurgion, having his nailes closely pared, and his rings (if he weare any) drawne off his fingers, and his arm naked, bare, and well anointed with oyle. . " A. Paré, Of the Generation of Man, in: *The Workes of that Famous Chirurgion Ambrose Parey*, transl. Th Johnson (London, 1634), p 915

> . . . if I foresee any danger I will call Physicians or Surgeons or women who are more experienced in this matter, and would not do anything without their advice or help. . . .[18]

The licensure of midwives was first proposed in England by Dr. Andrew Boorde of London in 1547. He advised that the candidate be nominated to the bishop by a group of "honest women of great gravitie."

> Then the bishop, with the consent of a doctor of physick, ought to examine her, and to instruct her in those things of which she is ignorant. To be thus proved and admitted is a laudable thing. If this were used in England not half so many women should miscarry, nor so many children perish in England as there be. The bishop ought to look into this matter.[19]

It will be noted that ecclesiastic instruction was to receive precedence over medical instruction. Nothing came of Boorde's advice. However, for long thereafter, whatever licensing was done was ecclesiastic and was intended principally to regulate the midwife's character rather than her technical competence.

So much for the regulation of the midwives. What knowledge did the learned physician have at his disposal that would make him a valuable consultant to the midwife? Before the 16th century only a few rather brief manuscripts and small sections in more comprehensive treatises were devoted to "diseases of women." Scant attention was paid in them to the mechanics of delivery. The two best known manuscripts were (1) *Passionibus mulierum curandorum* (The Diseases of Women) attributed to Trotula of Salerno, written in the 11th century and first printed in 1544[20] and (2) *De secretis mulierum* (The Secrets of Women), probably by Henricus de Saxonia, a pupil of Albertus Magnus, written in the 13th century and first printed in 1480.[21] The two agree on one obstetrical procedure. Trotula advises:

> If the child does not come forth in the order in which it should, that is, if the legs or arms should come out first, let the midwife with her small and gentle hand moistened with a decoction of flaxseed and chick peas, put the child back in its place in the proper position. . . .[22]

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[18] Witkowski, *op cit.* (n. 16 above), p. 658

[19] A Boorde, *The Breuyary of Helthe* (London, 1547, reprint Amsterdam· Theatrum Orbis Terrarum, 1971), book II, chap. 17 T. R Forbes, *The Midwife and the Witch* (New Haven: Yale Univ. Press, 1966), pp 143-144

[20] E. F Tuttle, "The Trotula and Old Dame Trot· a note on the lady of Salerno," *Bull. Hist Med*, 1976, *50*· 61-72.

[21] C Ferckel, "Die Secreta mulierum und ihr Verfasser," *Arch. f Gesch. d. Med*, 1954, *38* 267-274.

[22] E. Mason-Hohl (transl.), *The Diseases of Women by Trotula of Salerno* (Los Angeles· Ward Ritchie Press, 1940), p 23.

Albertus gives the same advice, adding that this maneuver causes pain, and the woman may then die. Although Albertus says more about obstetrics than Trotula, he gives greater emphasis to astrologic speculations, particularly in regard to fetal development.[23] As for treatises, Guy de Chauliac in the mid-14th century in his prestigious *Chirurgia magna* wrote only about extracting the placenta and extracting a dead fetus.[24]

If we accept the premise that Trotula was a midwife, then she was unique prior to the 17th century in writing about diseases of women from her own observation. But extant writings that have been attributed to her gave no more instructions about delivery than cited above. Little if anything the male authors wrote about obstetrics came from clinical experience, but admissions of this were rare. One instance appears in Albertus' *De secretis mulierum*. In regard to the cephalic presentation being normal for delivery, the author stated: "I have learned this from certain women."[25]

The first predominantly obstetrical work of the Renaissance was *Der swangern Frauwen und Hebammen Rosegarten* (usually referred to simply as "Rosengarten") by the south German apothecary, Eucharius Rösslin (d. 1526).[26] Its first edition was published in German in 1513. It was another compilation of practices and folklore from Hippocrates to Albertus Magnus by an author who almost certainly had no personal experience as an obstetrician.[27] However, it was the most comprehensive obstetrical work to its time, was illustrated and being printed, could be disseminated relatively easily. It achieved immediate popularity throughout Europe. English was the sixth language into which it was translated. This translation was published in 1540 with the title *The Byrthe of Mankinde*. The audience for which Rösslin had intended his work is more apparent from its original title, which translates into "The Pregnant Women's and Midwives' Rosegarden."

Beside the translations, various smaller publications based on the *Rosengarten* appeared throughout the 16th century. However, the next influential obstetrical work undoubtedly was *De la Generation de l'homme* . . . , which was published in 1573 by Ambroise Paré (1510-1590) and was translated into English with the same title, "Of the Generation of Man," in 1634.[28] It differs radically from the preceding works in that it was clearly addressed by a surgeon to surgeons. Midwives were barely mentioned. For instance, the surgeon is to judge when preparations for delivery are to begin so that the woman shall not "be wearied or so weakened" prematurely to interfere with true labor. At the proper time he is to have her placed in one of the carefully described positions which will facilitate delivery. Only if everything is proceding well ". . . the business is to be committed to nature and to the Mid-wife."[29] In a later passage Paré indicated the minor role that a midwife may have in the management of intra-uterine death. He stated that

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[23] Fasbender, *op cit* (n 3 above), pp 88-92.

[24] Guy de Chauliac, *La grande chirurgie*, transl. E Nicaise (Paris, 1890), pp 548-550

[25] Fasbender, *op cit* (n. 3 above), pp 88-92

[26] G. Klein, "Zur Bio- und Bibliographie Rösslin's und seines Rosengartens," *Arch f. Gesch d Med* , 1909, *3* 304-334.

[27] E Ingerslev, "Rösslin's 'Rosengarten': Its relation to the past (the Muscio manuscripts and Soranos), particularly with regard to podalic version," *J Obst. & Gyn. Brit. Emp* , 1909, *15* · 1-92

[28] Paré had appended a short obstetrical essay to an anatomy manual in 1549 which was " . in fact, the

> Also women or Mid-wives may help the endeavour of the Chirurgion by pressing the patient's belly with their hands downwards as the infant goeth out. . . .[30]

The development of governmental regulation of the practice of midwifery coincided with the development of the widespread persecution of "witches." From the 1480s, when this persecution began to reach an extent that made it a socially significant force, through the 17th century midwives were placed in particular hazard of being accused. The intensification of the ancient, more or less desultory concern with witches began rather abruptly with the bull "Summis desiderantes affectibus" that was issued by Pope Innocent VIII in 1484, and the publication by two of his German inquisitors (Henricus Institor, also known as Heinrich Kramer, and Jacob Sprenger) of *Malleus maleficarum,* "The Hammer of Witches," in 1484 and '87. This virtually became the textbook of the Inquisition. It included two chapters in which midwives were accused of commonly practicing witchcraft, or, indeed, the converse: that witches masquerated as midwives. The viciousness of these accusations is evident from the chapter titles: I:xi "That witches who are midwives in various ways kill the child conceived in the womb, and procure an abortion; or if they do not this, offer new-born children to devils." And II:xiii "How witch midwives commit most horrid crimes when they either kill children or offer them to devils in most accursed wise."[31] The general aspects of this topic have been elucidated by T. R. Forbes.[32]

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germ of his much larger treatise on the same subject which forms Book I of his *Deux livres de Chirurgie,* 1573," *De la génération de l'homme* . . . (Paris, 1573). Its first English translation was published in 1634 by Thomas Johnson (n 17 above). See J Doe, *A bibliography of the Works of Anbroise Paré* (New York: N.Y Acad Med., 1937), pp 43-45, 166-167

[29] Paré, *op. cit.* (n 17 above), p 902

[30] *Ibid.*, p 915

[31] H Kramer and J Sprenger, *The malleus maleficarum,* transl M Summers (New York. Dover, 1971), pp. 66, 140-144.

[32] Forbes, *op cit* (n 19 above) and *idem,* "Midwifery and witchcraft," *J Hist. Med.*, 1962, *17*· 264-283.

It is surprising to how small an extent the supposed fear of the potential of midwives to cause harm mystically rather than by poor obstetrical practice became incorporated into the ordinances governing their activities. Indeed, in the midst of the witchcraft craze midwives were authorized to be expert witnesses in certain legal procedings having no relationship to superstition. Legal references tended to be to superstition and avoided witchcraft and terminology which would acknowledge its existence. Even these references occurred more frequently in oaths of licensure than in the ordinances themselves. The following is an example from the Frankfurt ordinance of 1573, cited before:

> . . . Because one finds some midwives who are so superstitious that they falsely persuade themselves that pregnant women can not deliver and that they also can not deliver the child unless they practice such trickery and monkey business. Therefore, the Lord God frequently is sorely angered by such superstition and heathen blessings so that God then lets the infant of such a marriage fall ill, or lets the mother and the infant die. . . .[33]

The Parisian midwifery oath of 1560 included the clause

> . . . I will not use any superstitious or illegal means, either in words or signs, nor any other way, and will do nothing in revenge or in personal anger. . . .[34]

The license of a London midwife, awarded in 1588, contains the admonition

> . . . that ye shall not in any wise use or exercise any manner of witchcraft, charm, Sorcery invocations or other prayers than may be seemly with god's Laws (sic) and the Queens.[35]

A possible early example of the use of midwives as expert witnesses in preference to physicians is the story of a committee of "matrones tres-expertes" which was appointed by King Charles VII of France (1403-1461) in 1431 to confirm the virginity of Jeanne d'Arc. The committee consisted of Iolande d'Aragon, the wife of King Louis II of Naples and mother-in-law of King Charles, and two ladies of the court, Dame de Concourt and Dame de Vienne.[36] How these royal and aristocratic ladies became expert midwives is unknown, and it seems plausible that their selection was based on their political rather than their medical reliability. Nevertheless, the Regensburg ordinance of 1452 may provide some confirmation of such practice, although not of the anecdote. It stated that the "honorable, sworn women" who supervised the practicing midwives may be called to give expert testimony in certain legal actions.[37]

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[33] Burckhard, *op cit.* (n 9 above), p 216

[34] Witkowski, *op cit* (n. 16 above).

[35] J Hitchcock, "A sixteenth century midwife's license," *Bull Hist Med.*, 1967, *41* 75-76.

[36] A Delacoux, *Biographie des sages-femmes célèbres, anciennes, modernes et contemporaines* (Paris, 1834), pp 81-82, 91-93

The criminal code of the Holy Roman Emperor, Charles V (1500-1588), usually called the "Carolina," enacted in 1530 and '32, is quite specific about the role of midwives in legal matters and says nothing regulatory about their practice. Only one clause (#134) pertains to physicians and this is intended to distinguish liability for death due to malpractice from murder by a physician. One clause (#133) pertains to abortion, but refers neither to midwives nor other medical practitioners. Midwives are referred to only in the following two related clauses in the role of technical experts in suspected cases of maternal infanticide.

> Sufficient evidence of mothers who bear children in secret and kill them.
> #35. When there is suspicion that a woman who is believed to be a virgin has secretly born a child and has killed it: one shall particularly inquire whether she has been seen with an unusually large abdomen, especially whether the abdomen became smaller and she then became pale and weak. If such and similar things are found, whereby this woman becomes a person who has aroused suspicion, she should be examined in her secret parts by experienced women (i.e., midwives) for further evidence so far as it is useful. If she is then also found to be suspect and still will not confess her deed, she may be questioned under torture.
> #36. When, however, the infant has been killed so recently that the mother has not yet lost the milk from her breasts, she may be milked. One in whose breasts fully developed milk is found is therefore under strong presumption to be questioned under torture. However, if several official physicians state that someone who has not carried a child may due to various natural causes have milk in her breasts, so that a woman in such cases may exonorate herself, further information shall be obtained by midwives or others.[38]

The Paris regulation of 1560 gave midwives less independent legal authority than that which seems to result from the *Carolina*. It stated that:

> The matrons or expert midwives will not make a report by themselves on the chastity, corruption, or pregnancy of girls or women without the presence of a physician, two official surgeons of the King. . . , or one of them, in the event of a sickness like those which often accompany pregnancy, since they (midwives) are not instructed in this matter, added to which is the need to write and sign those reports, and few of them know how to write.[39]

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[37] Freund, *op cit.* (n. 7 above)

[38] J. Kohler and W. Scheel, *Die peinliche Gerichtsordnung Kaiser Karls V. Constitutio criminalis Carolina* (Halle: Waisenhaus, 1900), #134, pp. 69-70; #133, p. 69; #35, pp. 26-27, #36, p. 27. See also S. Polsky and S. Beresford, "Some probative aspects of the early Germanic codes, Carolina and Bambergenesis," *Ann. Int Med* , 1943, *18:* 841-845.

[39] Witkowski, *op. cit* (n. 16 above).

Having begun this history with the courageous Jacoba Felicie in the 14th century, it is fitting to conclude with another outspoken French woman three centuries later. Louyse Bourgeois (1563-1636) began to practice midwifery in Paris in 1593. She was the wife of a surgeon named Martin Boursier who had for long been an assistant of Ambroise Paré, and was taught by her husband rather than by midwives. One indication of her unusual independence was that she persistently used her maiden name, Bourgeois. She was appointed the royal midwife in 1601, when she delivered the future King Louis XIII. Her presence in the royal household ended in 1627 soon after her patient, the Duchess d'Orléans died following delivery. The autopsy revealed peritonitis. Its report, which was signed by ten physicians and surgeons, at least some of whom were her adversaries at court, did not accuse her of malpractice or even mention her. Nevertheless, Bourgeois replied with a vituperative defense ten pages long. In this she did not stop with attacking the competence of the board, but was so bold as to inpugn Galen for (also) having written about obstetrics without having practical knowledge:

> The things are no better known to you than they were to your Master Galen who, although he never had a wife and also attended few pregnant and child bearing women, yet took the liberty to dictate to a midwife how she should conduct herself in her duties. He also wrote a book about this subject in which, however, he betrays that he never knew anything about the uterus of a pregnant woman or about her afterbirth.[40]

Louyse Bourgeois was the first woman to write a book about midwifery. Four editions were published in French between 1609 and 1634, and there were translations into German and Dutch.[41] She conceded that "the office and standing of midwives is disdained"[42] and implied that their best defense consisted in better education. For instance:

> The various errors and mistakes which the midwives commit on the parturient women, about which there often are complaints, cause me to state that there is a great need for midwives to be admitted to the dissections of bodies of women by physicians, so that they can view and learn the position and nature of the womb in the body. . . .[43]

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[40] W. H Allport, "Louise Bourgeois, an old midwife's tale," *Am J Obst* , 1912, *65* 820-850 L Bourgeois, *Schutzrede* (Apologia) (Franckfurt, 1629), p 7.

[41] L. Bourgeois, *Hebammenbuch* . . , 2nd German ed., from the French ed. of 1626 (Franckfurt, 1628), vol. 2, p. 156. Translation of the full title: Midwifery book, wherein fertility and infertility, timely and untimely delivery, the condition of the fruit within and without the mother's body, incidental diseases as well of the mother as of the child, as well as their cure, together with the duties of a midwife are extensively considered.

[43] L Bourgeois, *Hebammenbuch* . . , 1st German ed., from the French ed of 1617 (Oppenheim, 1619), vol 1, p. 152.

While she strove zealously to protect the role of midwives, she admonished her colleagues not to delay to call a physician or, preferrably, a surgeon:

> . . . If she can not accomplish anything definite with the cooperation of physicians, the help of the wound surgeon should be employed soon enough . . . . She should inform an experienced surgeon or wound surgeon that there is an emergency . . . he knows what to advise when things go badly, rather than becoming frightened and paralyzed by dangers . . . However, one finds midwives who hold themselves in such high esteem that when they are called to a woman in labor and have undertaken to assist, if they see and feel that it is impossible for them they still delay until the situation is quite hopeless. Only then is the poor wound surgeon called . . . disregarding that had he been called soon enough the mother and child could have been saved.[44]

This statement both is in keeping with the oath she had taken and shares the critical view of, among others, Jacques Guillemeau (1550-1630), surgeon to the King and, later, a signatory to the autopsy of the Duchess d'Orleans. In his textbook of obstetrics (French 1609, English 1612), although he preserved a far greater role for midwives than Paré did in his treatise, he criticized them as well as the families for commonly endangering both mother and child by refusing to call a surgeon:

> . . . if he (the fetus) be placed awry, or else be weake and faint, or else if the midwife be at the farthest of her skill; then if they will save the child, and so consequently the mother from death: they must call a Chirurgion to deliver her, and bring the child into the world: which . . . is commonly done too late eyther through the wilfulnesse of the kinsfolks, or obstinacy of the Midwives.[45]

Mme. Bourgeois-Boursier may be considered a principal link between the Renaissance development of midwifery and the pre-modern phase of obstetrics. She advocated both better education for midwives and freer consultation by them with physicians and surgeons. As it turned out, freer consultation rapidly became replacement of midwives by better trained surgeon-accoucheurs, at least in the service of the upper classes. As these practitioners gained in acceptance they furthered their position by becoming increasingly critical of and less helpful to the midwives, who consequently fell farther behind them in relative skill and prestige.

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[44] Bourgeois, *op cit* (n 42 above), pp 54-55

[45] J Guillemeau, *Child-birth or the happy deliverie of women* (London, 1612), introduction, p. 2 This was translated from. *De l'heureux accouchement des femmes, où il est traicté de leur grossesse, de leur travail* (Paris, 1609)

ENLIGHTENED HEALTH REFORM, PROFESSIONAL MEDICINE AND TRADITIONAL SOCIETY: THE CANTONAL PHYSICIANS OF THE BAS-RHIN, 1810-1870*

GEORGE D. SUSSMAN

During the eighteenth and nineteenth centuries European medicine developed from a learned profession into a consulting profession. Earlier, despite the privileged status of medicine in the university, the profession's effective control over the diagnosis and treatment of diseases was largely confined to the towns and subject even there to many challenges. Only since the late nineteenth century has the professional monopoly been universal and virtually unquestioned. Speaking socially, not scientifically, three elements contributed to the transformation of medicine from a learned profession to a consulting profession: the doctors' own ambitions for their profession, the support of political authorities for medical ambitions, and public confidence in the medical profession and its claim to provide superior health care. The first two elements in this mixture, medical ambitions and political support for those ambitions, were already coalescing in the late eighteenth century in the elaboration of projects by alliances of progressive doctors with enlightened-absolutist governments to provide comprehensive health care to society, as well as in the first efforts to regulate the practice of medicine over the entire territory of the state and not just in the towns. In the early nineteenth century the alliance of professional medicine and government was reinforced by their cooperative efforts to propagate Jennerian vaccination against smallpox and to overcome the public-health crisis created in the cities by massive immigration and overcrowding, the crisis dramatized in this period by the cholera epidemics. The third element in the development of medicine as a consulting profession, public confidence, was much slower to develop than the other two, especially in rural areas, where most of the population but very few of the doctors lived in the late eighteenth and early nineteenth centuries. The failure of public

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confidence in the medical profession to develop at the same pace as medical ambitions and political support during this period may be ascribed to the wide cultural gap between university-trained physicians and the general population before primary education became universal and compulsory, and to the failure of professional medicine before the germ revolution to demonstrate convincingly its superiority over irregular healers, especially in the area of therapeutics.[1]

The French department of the Bas-Rhin (Lower Alsace) offers an unusually well documented illustration of the development of the social role of professional medicine in the difficult transitional era from the late eighteenth to the late nineteenth centuries. There on the cultural frontier between France and Germany in the year 1810 one of Napoleon's prefects, the Baron Lezay-Marnésia, building upon the tradition of enlightened-absolutist health reform developed over the previous forty years, established a system of publicly salaried rural practitioners in each canton of the department. From 1810 until the German occupation of Alsace in 1870, the cantonal physicians served the poor rural population of the Bas-Rhin. The numerous letters and reports they sent to the departmental administration in Strasbourg over this period reveal the difficulties of professional-governmental cooperation in the liberal nineteenth century, the provincial doctors' own ambitions for medicine, and the indifference and resistance of the people of Alsace to the joint efforts of government and professional medicine in their behalf.[2]

The enlightened-absolutist program of health care has been described most systematically for the German states under the label "Medical Police," but recent scholarship has shown that a very similar program was elaborated in French administrative practice in the last decade and a half of the Ancien Régime and in legislation or draft legislation of the Revolutionary period. Under this program, if we may characterize it in a general way, the state would have assumed two kinds of tasks traditionally associated with municipal governments in Continental Europe: the regulation of medical practice and the supervision of public health, including provision of health care for the indigent. In both areas the reformers envisaged not only a state takeover of municipal functions, but the enlargement and reform of those functions. The minimal standards for medical practice would be raised and unlicensed practice vigorously prosecuted everywhere. In place of traditional public-health measures—isolation of sick individuals and communities in epidemics and custodial care for the chronically ill—the reformers emphasized the treatment of patients, generally at home and even in the country, and a comprehensive assault on the presumed environmental causes of ill health. A few characteristic measures they proposed and implemented were formal obstetrical instruction for midwives, the institution of epidemic doctors who were dispatched to communities suffering from unusual morbidity or mortality, the preparation of epidemic and climatic surveys and medical topographies to understand the causes of disease, the provision of free food and medicine to the indigent sick and the preparation of stores of common remedies for rural communities located at a distance from any pharmacy.[3]

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[1] For these generalizations I am particularly indebted to Eliot Freidson, *Profession of Medicine, A Study of the Sociology of Applied Knowledge* (New York: Dodd, Mead & Co., 1970), especially ch. 1, and Richard Harrison Shryock, *The Development of Modern Medicine, An Interpretation of the Social and Scientific Factors Involved* (New York: Alfred A. Knopf, 1947).

[2] I first became aware of the institution of cantonal physicians in the Bas-Rhin through a reference in J. Léonard, "L'exemple d'une catégorie socio-professionnelle au XIX^e siècle: les médecins français," in *Ordres et classes, Colloque d'histoire sociale, Saint-Cloud, 24-25 mai 1967* (Paris-The Hague: Mouton, 1973), p. 226. The papers concerning the cantonal physicians are deposited at the Archives départementales du Bas-Rhin in Strasbourg (hereafter abbreviated A. D. Bas-Rhin).

In France such a program for health care was born of the collaboration in the mid-1770s between the Controller-General Turgot and the physician Vicq d'Azyr, soon to be General Secretary of the Royal Society of Medicine.[4] The program developed in practice both in the Intendancies and in Paris until the Revolution, when it was first formulated in a comprehensive manner as draft legislation. In this form the program included provision for a publicly salaried physician or surgeon permanently settled in each rural canton to care for the indigent sick, inoculate the poor, and report annually on medical topography and statistics. But in the Constituent Assembly medical reform faltered, caught in the crossfire between a committee of physicians, organized by Vicq d'Azyr and primarily concerned with the regulation of medical practice, and a committee of charitably minded *grands seigneurs*, led by the Duc de Rochefoucauld-Liancourt and concerned with health care as an aspect of the even more general subject of poverty and public assistance. During the subsequent decade of Revolution and the Consulate the doctors' program, emphasizing the regulation of medical practice, was enacted and implemented, while the public-health side of the Enlightenment program was neglected because of the competing claims of war and changing political circumstances.[5] Perhaps the full health-care program of enlightened absolutism was too radical for the French Revolution, or perhaps it was only adapted to a more authoritarian and paternalistic state than emerged from that event.

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[3] The application of certain elements of this general program may be seen in two local studies Jean-Pierre Goubert, *Malades et médecins en Bretagne (1770-1790)* (Rennes: Institut armoricain de recherches historiques, 1974), and Dr. Eugène Olivier, *Médecine et santé dans le pays de Vaud au XVIII^{e} siècle, 1675-1798*, Bibliothèque historique vaudoise, vols 31-32 (Lausanne. Librairie Payot, 1962). Carlo M Cipolla, *Public Health and the Medical Profession in the Renaissance* (Cambridge: Cambridge University Press, 1976), depicts some features of the older, municipal pattern of medical care in Nothern Italy

[4] Caroline C Hannaway, "The Société Royale de Médecine and epidemics in the Ancien Régime," *Bull Hist. Med* , 1972, *46* 257-73, J Meyer, "L'enquête de l'Académie de Médecine sur les épidémies, 1774-1794," in *Médecins, climat et épidémies à la fin du XVIII^{e} siècle* (Paris-The Hague Mouton, 1972), pp 9-20 George Rosen, "Mercantilism and health policy in eighteenth-century France," *Med. Hist* , 1959, *3* 259-77, suggests some of the intellectual bases for the enlightened-absolutist health-care program in France

The enlightened-absolutist program of health care received its fullest theoretical expression in the German states during the last three decades of the eighteenth century. There Johann Peter Frank's six-volume *System einer vollständigen medicinischen Polizey*, which began to appear in 1779, was the most outstanding and comprehansive monument to an idea which also inspired other books, several journals and university courses, as well as many partial reforms and a few drafts for more comprehensive medical codes.[6] An "Organic Edict concerning the state of medicine in the Kingdom of Bavaria," enacted in 1808 under the reform ministry of Montgelas, provided that every urban and rural district in the realm was to have a *Gerichtsarzt*, whose duties included smallpox vaccination, enforcement of the laws governing the practice of medicine, supervision of the sanitary state of public institutions, and monthly reports to the government on the general state of the district's health and prevalent diseases.[7] Great Britain, despite many individual contributions to public health, generally on private initiative, took no part during the eighteenth century in the elaboration of comprehensive state programs of health care.[8]

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[5] George Rosen, "Hospitals, medical care and social policy in the French Revolution," *Bull. Hist. Med* , 1956, *30* 124-49, Dora B. Weiner, "Le droit de l'homme à la santé—une belle idée devant l'Assemblée Constituante. 1790-1791," *Clio Medica*, 1970, *5* 209-23 This article and others by George Rosen which are cited below have recently been reprinted in George Rosen, *From Medical Police to Social Medicine Essays on the History of Health Care* (New York Science History Publications, 1974).

[6] Johann Peter Frank, *A System of Complete Medical Police*, ed. Erna Lesky Trans of 3rd ed. of 1786 (Baltimore Johns Hopkins University, 1976). George Rosen, "Cameralism and the concept of medical police," *Bull Hist Med* , 1953, *27* 21-43, and "The fate of the concept of medical police, 1780-1890," *Centaurus*, 1957, *5* 97-113. Reinhold August Dorwart, *The Prussian Welfare State before 1740* (Cambridge Mass . Harvard University Press, 1971), Part V, suggests an earlier, more bureaucratic and less medical origin for some parts of the program of Medical Police

[7] Dr Walther Koerting, *Die Medizinalverfassung von 1808 fur das Konigreich Baiern*, Schriftenreihe der Bayerischen Landesarztekammer, Band 25 (Munich Bayerische Landersärztekammer [1971?])

[8] Shryock, *The Development of Modern Medicine*, ch. v, George Rosen, *A History of Public Health* (New York M.D. Publications, 1958), chs iv-v.

The decree of October 31, 1810, by which the prefect Adrien de Lezay-Marnésia organized health care and established the cantonal physicians in the department of the Bas-Rhin, falls by its origin and contents within the tradition of enlightened-absolutist health reform which I have been describing. Its author, a French noble of the Old Regime born in the same month as Napoleon (August 1769), had many opportunities to acquire the German Cameralist administrative outlook as a student at the practically oriented Collegium Carolinum at Braunschweig in 1785-87 and at the University of Göttingen in 1791-92, as a collaborator in the Germanophile circle of Madame de Staël during the late 1790s, and in a number of official posts he filled for Napoleon in the German states before he was appointed to his post in Strasbourg in the spring of 1810. Immediately before becoming prefect of the Bas-Rhin, Lezay-Marnésia had held the same office in the annexed German department of Rhin-et-Moselle. It was there in 1807-8 that he first devised his comprehensive health-care program in collaboration with Franz Gerhard Wegeler, a physician trained at Bonn and Vienna who was then professor of legal medicine and obstetrics at the University of Bonn. Their program for Rhin-et-Moselle, set forth in a decree of January 18, 1808, established eighteen district physicians with broad responsibilities for health care.[9] This was the model from which Lezay fashioned the system of cantonal medicine in the Bas-Rhin in October 1810, some eight months after his promotion from the prefecture at Koblenz to the more important one at Strasbourg farther up the Rhine.

But there was more to Lezay's political initiative in both departments than the tradition of enlightened-absolutist health reform. There was also an urgent problem which required a solution. This problem was the persistence of high levels of smallpox mortality almost a decade after Jennerian vaccination had been introduced into France with the strong support of the central government.[10] In Lower Alsace throughout the first decade of the nineteenth century smallpox cases and deaths occurred every year and major outbreaks every third or fourth year, following the classic pattern in the period before vaccination. A major outbreak in 1803 caused an estimated 500 deaths in the city of Strasbourg, whose population was a little over

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[9] Egon Graf von Westerholt, *Lezay Marnesia, Sohn der Aufklarung und Prafekt Napoleons (1769-1814)* Mainzer Abhandlungen zur mittleren und neueren Geschichte, Band 2 (Meisenheim am Glan Verlag Anton Hain KG, 1958); notice on Wegeler in *Biographisches Lexikon der hervorragenden Arzte aller Zeiten und Volker,* V (2nd edition, Berlin and Vienna. Urban & Schwarzenberg, 1934), pp. 877-78

[10] R G. Dunbar, "The introduction of the practice of vaccination into Napoleonic France, *Bull Hist Med.,* 1941, *10* 635-50

50,000. Another major outbreak in 1807 took between 300 and 400 lives in Strasbourg and over 1,200 in the entire department of the Bas-Rhin, leaving about the same number of people permanently disfigured.[11]

The departmental administration had not neglected numerous communications from Paris concerning propagation of the vaccine. Lezay-Marnésia's predecessors had established centers to provide free vaccination and appointed a commission drawn from the medical faculty in Strasbourg to supply rural practitioners with vaccine and instruct midwives and health officers in the technique of vaccination. The results were disappointing. From the first introduction of Jenner's discovery in 1801 until 1806 there were only 2,442 vaccinations reportedly performed in the entire department of the Bas-Rhin, with a population of nearly one-half million. In the next two years, 1806-7, despite the fright created by the epidemic of 1807, the department's physicians and surgeons, who were all provided by the prefect with forms to record this information, reported performing only 10,560 vaccinations. Add 6,649 reported in 1808 and the total number of vaccinations reported in the Bas-Rhin over eight years barely equaled the normal number of births in the department in a single year.[12]

Doctors argued that popular indifference and resistence were to blame for the slow adoption of the vaccine in Lower Alsace. But Lezay-Marnésia, in the preamble to the decree of October 31, 1810, insisted that the major obstacle was not indifference, but indigency and the distance which separated most of the population from any health professional. Three years before, in 1807, the prefect Shée pointed to the same difficulty when he ordered that the epidemic physicians (*médecins des épidémies*) in each arrondissement, upon the mayor's request, should vaccinate free of charge in communes "where the absence of trained health officers has prevented the inoculation of vaccine up till now." But there were only four epidemic doctors in the department and probably several hundred communes without trained health officers. The solution adopted by Lezay-Marnésia was so evidently called for by the situation that it was actually proposed to an earlier prefect in 1809 by the sub-prefect of Sélestat: "I propose . . . that one physician or health officer be designated in each canton to perform free vaccinations in the communes."[13] There was, in other words, no way to introduce universal vaccination and eliminate smallpox in the Bas-Rhin without introducing professional medicine into rural districts, and only the administration could support the establishment of doctors in the country, where there was too little money and interest in medical services to support private practice. In Rhin-et-Moselle, Lezay-Marnésia had proven that with publicly supported doctors in the rural districts, universal vaccination was feasible. For the two years following the introduction of the district physicians in January 1808 his moderately sized department (with a population of 250,000) registered the most numerous vaccinations for any of the 111 departments of Napoleon's Empire.[14]

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[11] A D. Bas-Rhin, 5M.66, Département du Bas-Rhin, "Rapport sur la vaccine, 1808", 5M.74, draft letter from the prefect to the Minister of the Interior, Dec 17, 1812

[12] A D. Bas-Rhin, 5M.66, *arrêté* of 15 Thermidor an XI, Département du Bas-Rhin, "Rapport sur la vaccine, 1808," and "Deuxième rapport sur la vaccine, vaccinations opérées dans le département pendant l'année 1808 "

[13] A. D. Bas-Rhin, 5M.74, letter from Noel to the prefect, Apr. 20, 1808 (for the doctors' views), + 3K 11,

Universal vaccination fit readily into the humanitarian vision of Enlightenment doctors and bureaucrats: professional health care for all parts of society. The preamble of the decree of October 31, 1810, by which Lezay-Marnésia organized health care in the department of the Bas-Rhin, was a forceful statement of enlightened thinking. "The indigent sick of the country," it declared, had a right to assistance; most fatal diseases were so destructive only because so much less was done for their prevention than for their treatment; poverty was often the cause of the most common diseases. In the same spirit the preamble took account of recent legal and medical developments—the need to enforce the new national laws regulating the practice of medicine and pharmacy and "to extend to all classes the benefits of vaccination"—as well as local circumstances—notably the absence of any medical professionals in several cantons of the department. In the body of the decree Lezay-Marnésia established *médecins de canton* in each of the thirty-three cantons of the Bas-Rhin outside the city of Strasbourg, these offices to be filled by the prefect and supported at a fixed annual salary (initially 500 francs) by the communes of each canton. Lezay evidently intended that these positions would be filled by university-trained medical doctors rather than by less qualified *officiers de santé,* the successors to rural surgeons authorized by the Law of 19 Ventôse year XI. In practice the appointments invariably did go to full-fledged doctors. The cantonal physicians reported to a medical committee created by Lezay-Marnésia in Strasbourg to advise the prefect.[15]

Recueil officiel des actes de la préfecture du département du Bas-Rhin, XI (Strasbourg F G Levrault, 1811), pp 399-454 (for the *arrêté* of Oct 31, 1810); 5M 74, draft *arrêté* [Dec 1807] and letter from the subprefect of Sélestat to the prefect, Nov 19, 1809 I am responsible for all translations

[14] Westerholt, *Lezay Marnesia,* p 152

[15] A D Bas-Rhin, + 3K 11, *Recueil officiel des actes de la préfecture du département du Bas-Rhin,* XI, 399-454 (the *arrêté* of Oct 31, 1810), 535 (*arrêté* of Dec 28, 1810) The latter decree contains the first mention of a specific salary, 500 francs per year By 1818 the annual salary had been raised to 600 francs, where it

The bulk of the decree outlined the cantonal physicians' diverse functions, both in ordinary times and in the event of epidemic or epizootic diseases, and the functions of cantonal physicians, mayors, clergymen, and others with respect to vaccination and smallpox. The articles on vaccination (35 of 91 articles in the decree) minutely prescribed the procedures by which cantonal physicians, assisted principally by the mayors, were to vaccinate free of charge all infants in their jurisdictions within three months of birth (in addition to all other children not yet immunized against smallpox), to verify one week later that the operations had taken, and to maintain the local supply of vaccine at all times using the arm-to-arm method. The cantonal physicians' other ordinary duties included free medical treatment of all designated indigents, first aid for victims of accidents and emergencies, legal-medical testimony when required, quarterly sanitary inspections of every commune with specific attention to the state of public buildings, markets, cemeteries, and streets, enforcement of the laws governing the practice of medicine and pharmacy, the preparation of quarterly reports on hygiene and semi-annual medical topographies. In the event of a local epidemic the cantonal physician was to organize medical assistance and controls and to make extraordinary reports to the prefect.

Once he had appointed physicians to fill the new positions in each of the cantons (mostly over the last two months of 1810), Lezay-Marnésia set about vaccinating the population of the Bas-Rhin. He conceived of this task in two stages, which he presented in circulars to the cantonal physicians and mayors of the department as goals. In the first stage, which was to occupy the year 1811, the goal was "to bring vaccination to the point that the entire living generation of this century has received its benefit." He explained further:

> During this year we must finish off whatever [children] remain to be vaccinated, both from the first half of the year, and especially from previous years, back to and including 1801, in order that on next year's tables there will appear among the number vaccinated only the births of the preceding quarter [16]

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remained for many years. (5M.33, *arrêté* of June 21, 1818.) This salary was neither sufficient nor intended to provide the full living of a university-trained professional, including the unavoidable expense in the country of keeping a horse. (See the cantonal physicians' petitions for a raise, Aug 1845, in 5M 33.) From the beginning, then, cantonal medicine was a supplement to and not a substitute for private practice The new national laws regulating the practice of medicine and pharmacy were the Laws of 19 Ventôse and 21 Germinal year XI, both cited in Article I of Lezay's decree The Law of 19 Ventôse defined the different educational requirements and other procedures for licensing doctors of medicine and surgery, health officers, and midwives, and the limits imposed on medical practice by health officers and midwives. It also prescribed penalties for unlicensed and unauthorized practice For the text of the Law, see *Gazette nationale ou le Moniteur universel*, 8 Ventôse year XI, pp 645-46

[16] A. D. Bas-Rhin, 5M 67, circular from the prefect to the mayors and cantonal physicians, Oct. 25, 1811.

In the second year, 1812, Lezay-Marnésia's goal was to establish the vaccination of all newborns on a regular basis. Until that was achieved, the task was incomplete. Thus the prefect turned aside a compliment from the secretary of the Central Committee of the Vaccine on the extraordinary number of vaccinations performed in the Bas-Rhin in 1811 with this observation:

> I do not believe that my department should be held up as a model for vaccination; for it is not their number which deserves merit, but to have brought vaccination to a par with births. My former department of Rhin-et-Moselle is more advanced in this respect; all the children there are vaccinated during the three months following their birth. The majority of cantons in the Bas-Rhin has already reached that point, but not yet all.[17]

To achieve these goals it was not enough to provide free vaccination in every commune by the cantonal physicians. Local officials had to cooperate by providing lists of infants born and vaccinated and by seeing that the right people were assembled when the doctor arrived to vaccinate, to verify earlier operations, or to take material from previously vaccinated children for subsequent use elsewhere. Lezay-Marnésia prodded and threatened the mayors in response to frequent complaints by cantonal physicians who had journeyed to villages only to find that no preparations had been made for their visit, despite their prior notification of local officials.[18] The fault was not only with the mayors. Against the reluctance of parents to allow their children to be vaccinated, Lezay-Marnésia mobilized the persuasive efforts of local officials and especially clergy.[19] When persuasion failed, he tried various coercive measures, including depriving the parents of their communal rights and even placing their homes in quarantine. Napoleon, reading about these measures in the newspaper, tersely remarked to his Minister of the Interior that the prefect had gone too far.[20]

The results of Lezay-Marnésia's efforts on behalf of vaccination in the Bas-Rhin were no less astounding than his earlier achievement in Rhin-et-Moselle. In the first year of his campaign, 1811, 38,657 vaccinations were performed in the department. Taking account of the number of infants who died shortly after birth, it was claimed that the number of vaccinations that year exceeded the number of newborns by over 100 per cent (there were 21,455 births, of which number the prefecture deducted one-sixth to determine the number of survivors). Lezay-Marnésia was satisfied that all the children missed in previous years had been vaccinated. The next year, 1812, there were 18,929 vaccinations in the department and 21,744 births—still a net excess of vaccinations if we make the adjustment of one-sixth for early infant mortality.[21] In subsequent years, even after Lezay-Marnésia's death in 1814, the parity between vaccinations and births in the Bas-Rhin was maintained. A later prefect, in 1817, gave credit for this sustained achievement to the institution which his predecessor had established in 1810:

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[17] A. D. Bas-Rhin, 5M 74, copy of a letter from the prefect to Husson, Dec 23, 1812

[18] See, for example, A D. Bas-Rhin, 5M 67, circular from the prefect to the mayors, June 6, 1811, and drafts of letters to the mayors of Engenthal and Wangenbourg, Nov 25, 1811

[19] For example, A D Bas-Rhin, 5M.67, draft of letter from prefect to the Grand Vicars of the Diocese of Strasbourg, Aug 21, 1813

[20] A D Bas-Rhin, 5M 67, draft letter from the prefect to the sub-prefect of Sélestat, Feb. 2, 1811, Fernand L'Huillier, *Recherches sur l'Alsace napoléonienne* (n p Librairie Istra, 1947), pp 559-60, letter from Napoleon to the Comte de Montalivet, May 10, 1811, in *Correspondance de Napoléon Ier publiée par ordre de l'Empereur Napoléon III,* XXII (Paris Imprimerie impériale, 1867), 176

> I have the deep-seated certainty in advance that without the assistance of the cantonal physicians, vaccination would soon fall back into the state of inertia from which concerted efforts only raised it after several years and by the means which I have just indicated.[22]

The necessity of extending smallpox vaccination to the entire population, especially the rural population, was the immediate reason for the creation of the cantonal physicians, and the success of that program was the institution's first, most striking, and most enduring achievement.

Both doctors and administrators had other ambitions for cantonal medicine than universal vaccination, but the surviving correspondence and reports leave the impression that few of these ambitions were fulfilled, at least by the cantonal physicians before 1870. I shall discuss the doctors' ambitions and popular resistance to them below. Cantonal physicians were, in general, persistent advocates of better hygiene in the streets, public buildings, and homes of the Alsatian villages, and their reports indicate a marked improvement in these areas during the 1850s and '60s. But their reports also indicate that the doctors had little direct role in this achievement, for which most of the credit should go to administrative initiative and general economic development.[23]

Vaccination was different in that it could not have been achieved without the active cooperation of doctors and administration. Vaccination embodied the partnership of medical ambition and political support which had been forming since the end of the Ancien Régime, and more. All of this was represented in the *séance de vaccination*, a commonplace scene of Alsatian village life in the nineteenth century. Every three months the cantonal physician arrived to vaccinate in the communal hall all the newborn babies assembled beforehand by the mayor. As the doctor vaccinated, the mayor or another official of the commune recorded the event in the registers he was required to keep. An even more important element in this scene than the cooperation of doctor and mayor, professional medicine and government, was the encounter between doctor and baby. With vaccination, for the first time in Western history every last person routinely saw a university-trained physician at least once in his or her life. Vaccination was the first step in the medicalization of the general population and in the medicalization of the life cycle of contemporary man.[24]

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[21] A D. Bas-Rhin, 5M 74, tables of vaccinations performed in the department of the Bas-Rhin, 1811 and 1812.

[22] A D Bas-Rhin, 5M 75, annual reports on vaccinations in the department, 1814-30, and draft letter from the prefect to the Minister of the Interior, Sep 10, 1817.

[23] A D Bas-Rhin, 5M 47-52, semi-annual reports by the cantonal physicians, 1850s and '60s

Vaccination, however, was only one part of Lezay-Marnésia's broad and enlightened vision of state-directed health care for all, the immediate but not the only reason for his establishment of cantonal physicians in the Bas-Rhin. The doctors of nineteenth-century Alsace, even those who served as cantonal physicians, did not share the prefect's vision in its entirety.[25] Local doctors appear to have had no part in framing the decree of October 31, 1810. It is true that from 1810 to 1870 most of the positions in the system of cantonal medicine were readily filled whenever they fell vacant. The cities were overcrowded with doctors in the early and mid-nineteenth century, and the small salary attached to the position of cantonal physician provided the necessary supplement to private fees that made rural practice economically viable.[26] Many doctors who were natives of the region sought the posts as a means of founding a practice near their homes, properties, and families.[27] Where there were other doctors and health officers practicing in the canton, doctors sought the title for its competitive value, because it "assures them ordinarily a numerous clientele in the country, and the inhabitants regard them as more learned than the other doctors."[28]

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[24] One cantonal physician wrote in 1867· "The vaccinations are accomplished everywhere with great facility, the infants are presented to us willingly and we can only praise the cooperation which the mayors of the various communes show us on this occasion." Another wrote the same year· "I should mention . that the custom of presenting infants at the *séances de vaccination* seems to be generally established [in his canton] " A. D Bas-Rhin, 5M.47, reports of Hartmann, July 20, 1867, and Müller, Aug. 9, 1867 The major difficulty the doctors encountered was in persuading mothers to return with their babies a week later so the doctor could verify whether the vaccination had taken and also (and this was the reason for the mothers' reluctance to return) so the doctor could recruit one infant as a source of live vaccine for operations elsewhere. On the difficulties of verification and of obtaining vaccine, see A D Bas-Rhin, 5M 30, report by G. Tourdes, Aug. 16, 1869. The problem of maintaining supplies of vaccine was general to nineteenth-century France See L. Camus, "La vaccine à l'Académie de Médecine (de 1820 à 1920)," in *Centenaire de l'Académie de Médecine, 1820-1920* (Paris. Masson, 1921), pp. 239-78 Ivan Illich develops the concept of the "medicalization" of contemporary life in *Némésis médicale. l'expropriation de la santé* (Paris Seuil, 1975), recently issued in English

[25] An exception was F E. Fodéré (1764-1835), professor of legal medicine at the Faculty of Medicine of Strasbourg, who was appointed Inspector of the Cantonal Physicians in 1820 See his "Instruction pour MM les Médecins cantonneaux," Strasbourg, July 25, 1820, in A D Bas-Rhin, 5M 33. Fodéré, who was influenced by Frank's work, was a major figure in the French hygiene movement See Rosen, "The fate of the concept of medical police," p 110, and Erwin H Ackerknecht, "Hygiene in France, 1815-1848," *Bull Hist Med.*, 1948, *22:* 120.

Once in office, the cantonal physicians gave priority to their private practice. Among the duties of their official post the one they performed most conscientiously, aside from vaccination, was that of reporting instances of illegal practice of medicine, a widespread problem which seriously reduced the potential market for their private services. The tone of some of their denunciations shows that their concern was not solely with the public's health. Dr. Neurohr, the cantonal physician of Bergzabern, declared in 1811:

> As long as barber-surgeons practice internal as well as external medicine, as long as they usurp the title of doctor, and they are mistaken for true physicians; as long as pharmacists treat their drugs like any other merchandise whatever, sell their compound drugs to anyone without a prescription from an authorized practitioner, and as long as pharmacists even write prescriptions for patients who come to them, for so long will the status of the country doctor, who is jealous of his honor, be precarious and his situation sad.

Dr. Brouillet, writing in 1849, was even more blunt:

> On all sides, midwives, blacksmiths, veterinarians, itinerant drug vendors exploit for their profit the credulity and ignorance of the inhabitants of the country and thereby deprive the licensed practitioner of the just reward to which he is entitled, both because of his education and often because of an entire patrimony expended in acquiring it.[29]

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[26] On medical overcrowding in the cities, see George D Sussman, "The glut of doctors in mid-nineteenth-century France," *Comparative Studies in Society and History*, July 1977, *19*: 287-304.

[27] I made a study of a sample of 70 men who served as cantonal physicians at some time during the years 1810-70. Of 52 for whom I could discover the place of birth, 32 were born in the department of the Bas-Rhin, 4 in the adjacent department of the Haut-Rhin, 10 abroad (generally in nearby German states), and 6 elsewhere in France Of 62 for whom I could discover the medical faculty where they obtained their M D., 53 earned their degree from Strasbourg, 4 from German faculties, 3 from Paris, and 2 from Montpellier. In addition, the papers concerning appointments indicate that many cantonal physicians had other strong ties to the place where they served, often succeeding their fathers in a local practice, holding property or public office in the canton, etc. A. D. Bas-Rhin, 5M 34-42, papers concerning appointments, 5M 13-21, lists of medical personnel authorized to practice in the department

[28] A D Bas-Rhin, 5M 33, draft letter from the prefect to the Minister of Commerce and Public Works, Oct 11, 1833

[29] A D. Bas-Rhin, 5M 46, report of Neurohr, fourth quarter 1811, 5M 45, report of Brouillet, Jan 12, 1849

Clearly, the doctors' dedication to the exposure and prosecution of illegal practice derived as much from the interests of their private practice as it did from the duties of their public office.

The principal obligation of the cantonal physicians, in Lezay-Marnésia's conception, was to care for the indigent sick. "Those to whom fortune leaves the least means," he wrote in his first circular to the new officials, "have the first rights to your solicitude." In a decree of July 30, 1835, the prefect Choppin d'Arnouville specified that in fulfillment of their obligation to provide free medical care to the indigent sick, the cantonal physicians should tour all communes of their cantons at least once a month, in addition to conducting weekly public consultations.[30] Yet in 1854, when a later prefect asked the cantonal physicians in a circular letter whether the monthly tours were sufficient, only one of the fifteen doctors whose responses I found in the archives indicated that he made these tours. The others explained that they preferred to see the indigent sick either whenever they were summoned to do so, or when they happened to visit the villages to see private patients, or in scheduled hours for consultation in their offices. One cantonal physician even argued that the monthly tour was "contrary to the professional dignity of the doctory, for it is a principle that the patient or his family takes the initiative to summon a doctor."[31]

The cantonal physicians, then, did not share Lezay-Marnésia's vision, the enlightened-absolutist program of state-supported, professional health care for all. What they objected to in this program was not the ambition it expressed for the medical art—professional health care for all—but only the central role it assigned to the state in the direction of health care. These Alsatian doctors of the nineteenth century, ordinary rural practitioners far removed from the leaders of the French medical profession, freely expressed the broadest imaginable ambitions for their profession and for their art in the reports they regularly filed on the hygienic state of their cantons. The cantonal physicians aspired first of all, on behalf of all medical doctors, to establish their authority over all other medical occupations. When the cantonal physicians made their first tours of the communes in 1810 and 1811, they generally summoned the midwives, health officers, and pharmacists practicing in each commune to the town hall to read them the recently enacted laws which defined the limits on the permissible practice of each occupation. The doctors took command of rural health care with deliberate solemnity: "This manner of usurping a kind of authority," wrote Dr. Müller, "seemed to me all the more necessary as most rural midwives customarily address doctors with a protective tone, and in fact the doctor's fate, so to speak, often did depend on them."[32] Their official salaries and the new laws on the practice of medicine offered the doctors the prospect of emancipation from this demeaning dependency.

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[30] A D Bas-Rhin, + 3K 11, *Recueil officiel des actes de la préfecture du département du Bas-Rhin*, XI (1810), 443 (circular of Oct 31, 1810), + 3K 36, *Recueil*, XXXVI (1835), 247 (*arrêté* of July 30, 1835, articles 9-11)

[31] A D Bas-Rhin, 5M 29, responses to the prefect's circular of Sep 4, 1854 The quotation is from Dr Luroth's letter of Sep 9, 1854

Within society at large the cantonal physicians aspired to extend professional care to the entire population and particularly to less serious illnesses and illnesses affecting small children and aged people, instances where the peasants rarely summoned the doctor. They also sought to supervise all public and private hygiene, including such new topics of medical interest as domestic cleanliness, diet, alcoholism, and accidents caused by industrial machinery. To the twenthieth-century reader the nineteenth-century doctors' ambitions for their art appear to reach far beyond the areas in which medicine could then claim any demonstrable efficacy. In 1816 Dr. Müller, who was so conscious of the authority a doctor should exercise over a midwife, insisted that a certain paralyzed deaf-mute, who made frightful facial expressions, should not be allowed to beg on the main road because "the sight of these deformities sometimes has a very marked influence on reproduction not only of women but also of men." Müller claimed to know this by observation; Richerand had already included similar ideas on the causation of birth defects among his "popular errors relative to medicine."[33] In 1855 another physician sought to apply the concept of the epidemic constitution to a rash of suicides in his canton. Still another cantonal physician suggested during a flu epidemic in 1857-58 that the rural populace should consult doctors more often so they could be treated to the beneficial effects of purges and bleedings.[34] The point is not to make fun of the ignorance of these nineteenth-century rural practitioners, but to emphasize that the radical expansion of the ambitions of professional medicine, which developed from the late eighteenth century with the support of the state, anticipated any significant advance in the efficacy of medical intervention that might have justified so much larger a social role for professional medicine, certainly in the eyes of an ill-educated and sceptical public.[35]

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[32] A D Bas-Rhin, 5M 45, report of Dr Muller on the commune of Brumath, Jan. 5, 1811, report of Dr. Wies on the commune of Haguenau, first quarter 1811

[33] A D. Bas-Rhin, 5M.45, Müller, report on tour of Jan 11, 1816, A Richerand *Des erreurs populaires relatives à la médicine* (2nd edition, Paris: Imprimerie de Crapelet, 1812), ch. i.

[34] A. D Bas-Rhin, 5M 47, report of Frohlich, Mar 6, 1855, on suicides. "As similar cases did not occur in the surrounding cantons, it would be difficult to attribute this kind of epidemic to atmospheric influences. I believe instead in the instinct of imitation "

On the flu, see in the same carton the report by Kummer on the first half of 1858: "In the most serious cases, about the only ones for which we are consulted in the country, we have always found it effective to use evacuants in a moderate manner. However we have never hesitated to draw blood whenever the violence of the fever and the patient's constitution suggest it and we have always found this effective and the patients did too "

For the doctors of nineteenth-century Alsace, then, cantonal medicine was not the all-embracing structure of medical care envisaged by Lezay-Marnésia, but a peripheral concern, a salary to supplement private fees and entailing limited obligations, principally in the areas of vaccination and public assistance. It is somewhat surprising, therefore, to see several would-be spokesmen of French medicine in the 1840s propose cantonal medicine as a model for the reorganization of medical care in France as a whole. There were two reasons for this unusual burst of interest in the Alsatian institution. The first was the acute overcrowding of the medical profession, which was probably at its worst in that decade. The second was the general interest on the eve of the 1848 Revolution in the social question and socialist (as opposed to liberal individualist) solutions to all manner of political and social problems. In the end, the Medical Congress of 1845, the first representative assembly of the French medical profession, rejected the idea of cantonal medicine for the nation at large. Among the reasons expressed for this decision was the fear that such a system would create "a medical aristocracy dependent upon the authorities" and the idea that the poor should have the same freedom as the rich to choose their own doctor.[36] Although the Alsatians at the Medical Congress defended their local institution, there was really little disagreement on the superiority of private medicine over public. Thus, for example, when the post of cantonal physician of Niederbronn fell vacant in 1849, the doctors and health officers of the canton petitioned (unsuccessfully) to request that the position not be filled and offered to vaccinate and care for the indigent sick free of charge.[37]

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[35] A suggestive article on this and related ideas is Thomas McKeown, "A sociological approach to the history of medicine," *Med Hist*, 1970, *14*. 342-51

[36] See Sussman, "The glut of doctors" and *Actes du Congrès médical de France, Session de 1845, Section de médicine* (Paris. 1846), pp 106-34

[37] Ch Schützenberger, "Rapport sur le programme des questions à résoudre par le Congrès médical, fait à la Société de Médecine de Strasbourg, par une commission," *Mémoires de la Société de Médecine de Strasbourg*, 1850, *I* 214-17, *Recueil des travaux du Conseil départemental d'hygiene publique et de salubrité du Bas-Rhin de 1849 à 1858* (Strasbourg Imprimerie de G Silbermann, 1858), p 2

By the middle of the nineteenth century the doctors of rural Alsace seemed confident that their ambitions for medicine could be realized without the active support of the state. Indeed, they began to perceive state-supported medicine as a competitor and unwelcome master.

As there was a burst of medical interest in the cantonal physicians of the Bas-Rhin in the 1840s under the impact of medical overcrowding, the social conscience and socialist ideas of the '48 generation, so in the next decade there was a burst of governmental interest in the institution under the patronage of the Saint-Simonian Emperor, Napoleon III. Various ministerial circulars to the prefects in the 1850s spoke of the Emperor's "special interest" in the organization (at the departments' expense) of a system of free medical care for the poor population of the country and recommended the system of "cantonal physicians, applied with success in several parts of France."[38] Compared to the enlightened-absolutist program of health care, that of the Second Empire was a very modest proposal. In the late eighteenth century a program of health care for the indigent population of the country was a program to organize the efforts of a small number of qualified professionals on behalf of the majority of the nation. By the 1850s urbanization, economic development in the country, and the expansion in the numbers of qualified medical professionals, had reduced health care for the rural poor to a minor problem of public assistance which could be managed without any fundamental reorganization of the social role of professional medicine. For the government of Napoleon III, as for the doctors of nineteenth-century Alsace, cantonal medicine was peripheral to the central structure of French medicine, which was based on private practice.

If the doctors of the Bas-Rhin were never more than acquiescent in the role of cantonal physician conceived by Lezay-Marnésia, the rural population was indifferent or positively hostile to the new relationship with professional medicine which was forced upon it. Village mayors and municipal councils contributed to the doctors' maintenance only under persistent pressure from the departmental administration, especially in the early years of the program. Despite repeated pleas by Lezay-Marnésia, the communes begrudged the cantonal physicians an allowance of free firewood from communal forests such as they provided to the priests and pastors.[39] The cantonal physicians' salaries, the burden of which was distributed among the budgets of the component communes of each canton, were the source of endless conflict between the prefect and the municipal councils for their appropriation, and between the doctors and the mayors for their payment. No municipal council ever thought to exercise the power it had to raise the cantonal physician's salary above the prescribed base. "If these communes were left to themselves to vote concerning their views of medicine," six bitter doctors surmised in 1845, "it would be in favor of the charlatans of all sorts."[40] Many doctors also complained that the communes provided no funds at all or derisory amounts to purchase medicine, food, and fuel for the indigent sick; without these expenditures, professional care was helpless.[41]

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[38] A. D Bas-Rhin, 5M 29, circular letter to the prefects from the Directeur de l'Agriculture et du Commerce, Aug. 3, 1852, circular letter to the prefects from Billault, Minister of the Interior, Aug. 22, 1855, and many letters from various prefects requesting information on the operation of the system of cantonal medicine in the Bas-Rhin

Perhaps the best evidence of popular indifference and hostility to cantonal medicine is the hearty state of illegal practice and charlatanism for many years after the introduction of professional medical care in the country. This picturesque medical *demi-monde,* which haunted the cantonal physicians' reports from 1810 to 1870, included ill-trained and completely untrained practitioners, those with limited licenses to practice medicine and those with none, midwives and old wives, health officers and barber-surgeons, urine-readers, itinerant Tyrolean drug vendors, executioners who doubled as bone-setters, and the specialists who promised to repair sprains, hernias, diseases of the eye, and whitlows.

The doctors attributed the charlatans' success to the combination of their devious showmanship with their clients' credulity and ignorance. Thus Dr. Lion, cantonal physician of Soultz-sous-forêts, described the success of Jean Volk, known as "Doctor Hanns." He was a native of Baden, a shoemaker by trade. He never saw his patients: "To give his business a certain mysterious air, he has the patients' urine brought to him, or uses several words that make no sense." Volk charged nothing for his consultations, but split fees with the pharmacists who sold the drugs he prescribed at inflated prices. These remedies, according to Lion, were "harmful and poisoned," "certainly deadly poisons for the majority of patients," but "produced nevertheless a momentary good effect on patients who still had any strength." When Volk was arrested on the complaint of an earlier cantonal physician, the people of Ober- and Niederbetschdorff collected several hundred francs for him and sent a deputation to Strasbourg to obtain his release. They were unsuccessful, but during his fifteen-day imprisonment at Wissembourg, Volk continued to dispense medical advice from his cell.[42]

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[39] A. D Bas-Rhin, 5M 33, draft letters from the prefect to the mayors, Oct 9, 1812, and Aug 24, 1814, letters to the prefect from J J Müller, Sep 20, 1812, Rieffel, no date, and Rey, Aug. 16, 1814

[40] A D Bas-Rhin, 5M.33, draft letter from the prefect to the Directeur général de l'Administration départementale et de la Police, Nov 14, 1820, letter from six cantonal physicians of the arrondissement of Schlestadt to the prefect, Aug 18, 1845

[41] See, for one, A D Bas-Rhin, 5M 46, report by Lion, Feb 20, 1825

There were other reasons for the persistence of charlatanism in nineteenth-century Alsace. The sub-prefect of Wissembourg, to whom the prefect had forwarded Dr. Lion's angry reports on illegal practice, charged that the cantonal physician was himself "one of the principal causes, for if he were not to ask a very high fee for his visits, the people would not be forced, so to speak, to have recourse to more tractable persons, even to charlatans."[43] The cantonal physicians were obliged to give free medical care only to registered indigents, a relatively small minority of the population. This undoubtedly left many other people in the country who, though not utterly destitute, still could not afford the services of a university-trained professional. The charlatans also profited from the doctors' powerlessness in certain areas; they promised recovery from the conditions which the doctors declared incurable. Perhaps some patients were frightened by the repellent treatments still widely employed by the doctors of nineteenth-century Alsace—purges, leeches, bleeding.[44]

The major reason, I suspect, for the charlatans' relative success and the doctors' relative failure in the competition for patients in rural Alsace was the doctors' cultural and personal isolation from the rural communities in which they practiced. The doctors were all university-trained, whereas most of the villagers were barely literate. The doctors came from outside the community or, if they were natives, they belonged to the small economic and cultural elite. Often in religiously divided Alsace the doctors professed a different faith than the community they served.[45] The cultural differences may be illustrated by another report from Dr. Lion. This one concerns

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[42] A. D. Bas-Rhin, 5M 46, report of Lion, June 12, 1812.

[43] A. D. Bas-Rhin, 5M 46, letter from Blanchard, sub-prefect of Wissembourg, to the Baron Malouet, prefect, Nov 16, 1820.

[44] Richerand, *Des erreurs populaires*, p 313, Marcelle Bouteiller, *Médecine populaire d'hier et d'aujourd'hui* (Paris G.-P. Maisonneuve et Larose, 1966), p. 20

[45] Some of the cantonal physicians belonged to the small Jewish minority in Alsace, and there were also tensions between Catholics and Protestants There are many references to religion in the papers concerning appointments, A D Bas-Rhin, 5M.34-42 See also 5M.33, a letter from Cazeaux, *curé* of Sufflenheim, to the prefect, Sep 15, 1854, where the priest requests that the new deputy cantonal physician profess the religion of the majority in the locality

Jean-Philippe Muntzer, a native of Soultz who spent three years in Switzerland as an apprentice barber and student of surgery. On his return Dr. Lion questioned him and found that he knew neither Latin nor French and was ignorant in medicine and surgery. But Muntzer went off to Strasbourg and returned with the diploma of a health officer from the departmental Medical Jury. Dr. Lion shuddered to report the deaths Muntzer had caused since then and noted, as a mark of the health officer's callous inhumanity, "He joins in the funeral processions of his victims and eats the meal at the home of the deceased, in accordance with the despicable custom of this region."[46] In one sentence Dr. Lion illustrates, without realizing it, why the peasants would call on Muntzer and why they would not call on him.

It was not only the cultural difference which isolated the doctors in the Alsatian villages. It was also that they were newcomers, intruders into stable, highly integrated communities. In 1860 Dr. Binder wrote of the difficulties he faced in denouncing charlatanism:

> There exists here a remarkable family spirit. To attack one single member is to rouse against one the enmity of the entire family to which he belongs. Several denunciations followed by prosecution would be the signal for a universal crusade preached and directed against the unfortunate doctor who had been imprudent enough to undertake a struggle which it is impossible to sustain.[47]

The same tightly knit community that protected the charlatans would also naturally direct patients their way rather than to the doctors. In Eliot Freidson's sociological analysis, the combination of an indigenous lay culture markedly different from the professional culture with a highly extended, cohesive lay referral system would result in the least utilization of professional services.[48]

Besides all this evidence of popular indifference and hostility to professional medicine in rural Alsace in the nineteenth century, there is also some evidence of a trend toward professional health care, even before 1870. Certainly both the number and the quality of doctors practicing in the country were rising in this period. One encounter between the doctors and the peasants, vaccination, had become well established. The medical examination of young men for conscription may have provided another routine point of contact between the two worlds.[49] Slowly, over the years, more and more patients began to call for the doctor. Dr. Luroth declared that the number of his visits and consultations with the indigent rose at an annual rate of 10 per cent between 1830 and 1853. He claimed to make, on behalf of the indigent, an average of 18 visits and 30 consultations a month during the 1830s, compared to 44 visits and 60 consultations in the years 1850-53. In the years 1856-59, when similar statistics began to be collected from all the cantonal physicians, 70-75 per cent of the registered indigents in the department called upon the public doctors at least once a year.[50] In 1866 Dr. Kummer, who had been deputy cantonal physician at La Petite Pierre for at least a decade, wrote in connection with an epidemic of typhoid fever in the commune of Reipertswiller:

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[46] A D bas-Rhin, 5M.46, report of Lion, June 12, 1812

[47] A. D Bas-Rhin, 5M 47, report of Binder, second half of 1860

[48] Freidson, *Profession of Medicine*, pp 292-95

[49] Dr Steinbrenner, cantonal physician of Saar-Union, had a reputation for leniency in certifying physical

> We noticed that the population of Reipertswiller has made progress, slow it is true, in the care given to patients. With very few exceptions everyone called for the doctor's presence, whereas, in the past, those who did so were the exception.[51]

The files on cantonal medicine in the departmental archives at Strasbourg cease with the German occupation in 1870, so I do not know what became of the institution after that. In any event cantonal medicine was, in practice if not in the conception of its founder, a transitional institution whose function was nearly obsolete by 1870. It was born of the humanitarian ambition, shared by progressive circles in medicine and government in the late eighteenth century, to provide professional health care to all citizens, and notably to the poor population of the country. The possibility of eliminating smallpox after Jenner's discovery of vaccination gave urgency to this ambition in the Bas-Rhin, and perhaps elsewhere as well. For the Alsatian doctors of the nineteenth century cantonal medicine offered a means of access into the country, a way of overcoming popular indifference and resistance to professional health care, of medicalizing the entire population. Slowly but steadily the doctors extended their influence, and as they did they found the state's direct support less necessary and less desirable. In 1870 an era of unprecedented public confidence in professional medicine was about to open, thanks to the germ revolution and the bringing under control of certain infectious diseases. Thereafter the doctors, at least, had no more need of cantonal medicine.

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disabilities to the draft board A. D Bas-Rhin, 5M 33, letter from Neveux, sub-prefect of Saverne, to the prefect, Oct. 27, 1843

[50] A. D Bas-Rhin, 5M.29, letter from Luroth to prefect, Sep. 9, 1854; 5M.30, "Renseignements sur la médecine gratuite, à S.Ex le Ministre de l'Intérieur," n d. (circa 1858), and draft of a table on cantonal medicine in the Bas-Rhin, 1858-59, sent to the Minister of the Interior in April 1861. There were approximately 31,000 registered indigents in the department in the years 1856-59.

[51] A D Bas-Rhin, 5M.47, report of Kummer, 1866

DEPOPULATION IN POLYNESIA*

CHARLES S. JUDD, JR.

This essay begins in the small town of Dijon, in France, in 1749, when the academy of that community offered a prize for an essay dealing with the question: "Has the progress of science and art contributed to the corruption or to the improvement of morals?" A young Bohemian of the day, J. J. Rousseau, was visiting his friend, Denis Diderot, who was in prison in Vincennes. Diderot suggested the paradox of the superiority of the savage state. Rousseau picked this up and introduced his "noble savage" concept in his *Discourses on the Sciences and Arts,* which, incidentally, won the prize of the Academy of Dijon. He stated that natural man is indeed corrupted by the development of the arts and sciences and is better off in his natural state.

Rousseau's knowledge regarding his noble savages probably came in part from the writings of explorers who had returned from the Pacific. The Spaniards, Mendana and Quiros, who reached the Marquesas as early as 1595, and subsequently the Dutchmen, Roggeveen and Tasman, and finally Lord Anson of Britain in 1740, were a handful of men who brought back news of this great ocean. Rousseau learned of isolated, attractive people living on beautiful islands, who needed really nothing from the Western World. Eventually French explorers made voyages to the Pacific, Bougainville reaching Tahiti in 1868. When the latter returned to France and published an account of this "island of love," Diderot wrote an essay entitled *Supplément au Voyage de Bougainville*, in which he looked ahead and foresaw the far-reaching effects of future voyages to Polynesia. He addressed himself to the Tahitians with reference to their tearful friendly farewell to the explorer, Bougainville, as follows:

> Weep, wretched people of Tahiti, weep! But let it be for the coming and not for the going of these wicked and ambitious men. One day you will know them better. One day they will come back, that piece of wood you see hanging from that man's belt in one hand and the steel hanging by that man's side in the other, preparing to enslave you, massacre you, or subject you to all their own extravagances and vices. The day

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will come when you will be their slaves, as corrupted, as base, and as wretched as they themselves.[1]

Now in the two hundred years sinces Diderot wrote this, much of what he predicted has come true. The great turning point came in 1769, two years after Bougainville's voyage, with Captain James Cook's far flung explorations in the Pacific.

One of the most striking things that happened to the Polynesian islands after the arrival of Westerners was a decrease in population. Cook estimated the Tahitians at 204,000 in 1769.[2] Herman Melville seventy years later described them as numbering only 9,000 people.[3] From the records of Cook's lieutenants, the estimates for Hawaii were 300,000 people in 1778.[4] A formal census 100 years later tallied 57,000 Hawaiians.[5] In Samoa, the population dropped from 56,000 in 1839,[6] to 34,000 in 1853.[7] The Marquesans numbered 50,000 in 1772;[8] by 1936, there were 1,300 left.[9] Cook's figure for the Maoris in New Zealand was 100,000;[10] by 1900, these had dwindled to 40,000.[11] Many of these figures quoted are subject to speculation, but they do indicate a trend.

Prior to the arrival of the Westerners, natural phenomena caused fluctuations in population. Storms were frequent and were often followed by famines. A population might outgrow the resources of an atoll, resulting in migration, by canoe, of weaker and yet courageous members seeking new land. Indeed, this probably explains the great migrations of the Polynesians through the Pacific. Then there were the killing of enemies, human sacrifice, cannibalism, sorcery, infanticide, abortion, and suicide. Most of these were constant factors, so that great changes in numbers of people did not occur.

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[1] D Diderot, "Supplément au Voyage de Bougainville," in *Selected Writings*, ed L G Crocker; transl D. Cottman (New York MacMillan, 1966), p. 226.

[2] J Cook, *The Voyage of the "Resolution" and "Adventure" 1772-1775. The Journals of Captain James Cook on His Voyages of Discovery*, ed. J C Beaglehole (Cambridge· Cambridge Univ Press for the Hakluyt Soc, 1961), p 409

[3] H. Melville, *Omoo* (New York· Dodd, Mead, 1924), pp. 117-118.

[4] R. C Schmitt, *Demographic Statistics of Hawaii, 1778-1965* (Honolulu Univ. of Hawaii Press, 1968), p. 22

[5] "Census of the Hawaiian Islands, taken Dec. 27, 1872," *Hawaiian Gazette*, Honolulu, Apr 9, 1873

[6] C. Wilkes, *Narrative of the United States Exploring Expedition during the Years 1838, 1839, 1840, 1841, 1842* (Philadelphia· Lea and Blanchard, 1845), vol 2, p 130

[7] *Samoan Reporter* (London Missionary Society), no 15, Jan 1854.

[8] G Forster, *A Voyage Round the World in His Brittanic Majesty's Sloop "Resolution," Commanded by Captain James Cook, during the Years 1772, 1773, 1774, and 1775* (London White, Robson Elmsley, and Robinson, 1777), vol. 2, pp 33-34

[9] W C Handy, *Forever the Land of Men* (New York Dodd, Mead, 1965), p 224.

[10] K. Sinclair, *A History of New Zealand* (Middlesex Penguin Books, 1959), p 25

[11] *Ibid* , p 25

The depopulation that occured after the arrival of Europeans was dramatic. The first encounter between a voyaging ship and a Polynesian island involved the Westerners, on the one hand, with a superiority complex and a rather fixed attitude about any dark-skinned people. Cook's men called the Polynesians "Indians," on the basis of experience of encounters with natives on other continents. Sailors were starved, sometimes scurvy-ridden, and anxious to obtain water, fresh food, and wood for their fires. The Polynesians, on the other hand, having never seen white men before, or large ships, were in great awe of them, believing them, in some cases, to be gods. At the same time, the Polynesian curiosity, and their adventuresome spirit, prompted them to pilfer nails and other bits of iron, and personal effects from the foreigners very swiftly. Cook had his socks stolen from under his pillow, without knowing it, while sleeping ashore at Tahiti. The difference in concepts of property can perhaps best be expressed by Willowdean Handy, in recounting early visitations to the islands of the Marquesas:

> Each one felt obliged to "punish" the natives for theft, neither side knowing what the other considered theft to be. To the outsiders it was a betrayal and a sin. To the Marquesans it was play that tested the skill of the perpetrator, a popular game among themselves. How they laughed when forced to return Marchand's handerchief and snuffbox which they had filched from his person without discovery! Could two points of view be further apart?[12]

Sometimes minor thefts led to greater ones, and serious misunderstandings arose. These often led to conflict, resulting in the Europeans using musket and gunfire to great advantage against the hand weapons of the Polynesians. This is well described in an account from Wallis' ship in Tahiti in 1767:

> When the great guns began to fire, there were not less than 300 canoes about the ship, having on board at least 2,000 men; many thousands were also upon the shore, and more were coming from every quarter: the firing, however, soon drove away the canoes that were about the ship, and put a stop to the coming off of others. As soon as I saw some of them retreating, and the rest quiet, I ordered the firing to cease, hoping that they were sufficiently convinced of our superiority, not to renew the contest. In this, however, I was unhappily mistaken; a great number of the canoes that had been dispersed, soon drew together again, and lay sometime on their paddles, looking at the ship from a distance of about a quarter of a mile, and then suddenly hoisting white streamers, pulled toward the ship's stem, and began again to

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[12] Handy, *op cit* (n 9 above), p 96-97

throw stones, with great force and dexterity, by the help of slings, from a considerable distance: each of these stones weighing about two pounds, and many of them wounded people on board.[13]

This description by Wallis of an encounter is similar to others made many times by Europeans. In Cook's final meeting with the Hawaiians, he was at a point where he had worn out his welcome at Kealakekua Bay in 1779. He departed after weeks of misunderstandings and strained relationships. He was forced to return again because of storms, however. The natives were tired of the novelty of the British. When Cook went ashore to seize a hostage after the ship's cutter was stolen, angry natives set upon him and killed him by stabbing him in the back.[14]

The superiority of gunpowder and cannon fire invariably gave the Westerners an advantage over the Polynesians. Mendana killed 200 natives in the Marquesas and Society Islands in 1595. Some of the native kings, upon obtaining the tool of gunpowder from the Europeans, used it against their own kind. This was particularly true of King Finau of Tonga and King Kamehameha the First of Hawaii.

The French explorer, La Perouse, called the Samoan "a more malignant being than the wildest beast." His description of the massacre of his lieutenant, M. de Langle, and eleven other Frenchmen by Samoans makes the natives appear especially barbarous. Only later was it revealed that these same natives had been acting in revenge for a Samoan who, for some real or supposed pilfering, had been fired at from La Perouse's ship and mortally wounded. A keen sense of justice of these Polynesians was simply manifesting itself. The guilty party did not always receive the vengeance; sometimes it was showered upon some unsuspecting visitor.[15]

Few Europeans tried to understand the true nature of the Pacific islanders; most did not care. Only a small minority have bridged the huge cultural abyss between natives and Caucasians. One such man was Robert Dean Frisbie, who died as recently as 1948. He once said, " . . . it is of the utmost importance that a white man never ridicule the natives; never sneer at them or in any way humiliate them. If he does this he is lost."[16]

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[13] S. Wallis, *An Account of the Voyages Undertaken by the Order of His Present Majesty for Making Discoveries in the Southern Hemisphere*, ed. J. Hawkesworth, 3 vols (London. W Strahan, 1773), vol 1, pp. 224-225.

[14] John Ledyard, *A Journal of Captain Cook's Last Voyage to the Pacific Ocean . . Performed in the Years 1776, 1777, 1778, and 1779* (Hartford· Nathaniel Patten, 1783), p 146

[15] G Turner, *Samoa, a Hundred Years Ago and Long before* (London MacMillan. 1884), pp 196-197

[16] J N Hall, "Frisbie of Danger Island," in *The Forgotten One and Other True Tales of the South Seas* (Boston Little, Brown, 1963), p 210

Now the second encounter between Westerners and Polynesians was of a very different nature than the one just described, but it nonetheless heralded a second key cause of depopulation. It was an amicable encounter. The European sailors, upon finally reaching shore, found many native maidens with their arms outstretched to embrace them. Some girls were even thrust forward by their fathers and brothers. Being typical seafaring men eager to avail themselves of the fairer sex, especially after months-long voyages in heavy seas, their responses to these overtures were prompt and enthusiastic. If veneral disease was present, no better opportunity could have been devised for its dissemination.

When Bougainville arrived in Tahiti in 1768, several of his men contracted veneral disease. Wallis, the English explorer, had been in Tahiti six months previously, and was blamed by Bougainville for introducing the disease. When Cook came to Tahiti two years later, the disease appeared rapidly among his men. When Cook returned to England, his botanist, Joseph Banks, later president of the Royal Society, conferred with John Hunter about the veneral disease as it manifested itself in Tahiti. Hunter believed that the disease was probably gonorrhea, and that it was probably Bougainville who had introduced it because his ship had stopped at several places on the way to Tahiti, and it was the only disease that could have persisted in active contagious form for several months on a long voyage.[17] Gonorrhea's obstructing adhesions of the Fallopian tubes had a significant effect on fertility.

As more Westerners came to the islands, syphilis, with its involvement of many parts of the body, eventually became very apparent. Chapin, in 1850, cited syphilis as the most prominent cause of the decrease in population in Hawaii.[18]

In some of the more humid areas, the natives exhibited a disease called yaws. Curiously, this is caused by an organism essentially indistinguishable from that which causes syphilis. The serological tests and modern treatment for the two diseases are similar. One might speculate that in areas where yaws was endemic, such as Samoa, Tahiti, and Tonga, the disease conferred upon the people a partial immunity to syphilis, and made the devastation of the latter less pronounced. On the other hand, in areas where yaws did not exist, such as Hawaii, the people were more vulnerable to syphilis.

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[17] J Hunter, *The Works of John Hunter, F R S., with Notes*, ed J F Palmer (London. Longman, Rees, Orme, Brown, Green, and Longman, 1835-1837), p 31

[18] A Chapin, "Remarks on the venereal disease at the Sandwich Islands," *Boston Med & Surg J* , 1959, *42* 80

This brings us to other epidemic diseases which dramatically reduced the numbers of Polynesians. Their resistance, because of long isolation, was negligible to these fulminating contagions. In Hawaii in 1804, a disease with a high mortality rate spread rapidly and killed over 5,000 people. It was called the "squatting disease," and was referred to in the writings of Lisiansky[19] and Mariner[20] and others. They described it as having a rapid course of one or two days, accompanied by diarrhea and wasting. There was no skin eruption. Modern epidemiologists believe it was Asiatic cholera, presumably introduced from a ship coming from the Orient.

Another disease, often dramatic with its sudden onset, headache, high fever, and cough, was influenza. A single case might be difficult to diagnose, but if it was in epidemic proportions, it could be recognized easily. The Maoris called it the "head-splitting disease," because of the intense headache, and they suffered severe epidemics in 1790 and 1836. It came to Samoa in 1837 and to Hawaii in 1848. In the great world pandemic of influenza of 1918, thirty percent of the Tahitians died, twenty-five percent of the Samoans, and ten percent of the Tongans.[21]

Smallpox first appeared in Eastern Polynesia in 1835 at Mangareva in the Gambier Islands. It came to Tahiti in 1841 when an American ship arrived from Chile on its way to Hawaii. Knowledge of cases of the disease resulted in an attempt at quarantine of the ship, but several passengers went ashore nonetheless. One of these died of smallpox in a few days, and then the pestilence spread rapidly among the natives. Fortunately another ship soon brought some vaccine, and subsequent inoculation stemmed further spread.

Hawaii's experience with smallpox in 1853 was the result of the docking of an infested ship from California. An epidemic of 9,000 cases occurred, with 7,000 deaths, about eight percent of the population. This resulted in compulsory vaccination, which was especially effective on the island of Maui due to the vigorous efforts of Dr. Dwight Baldwin of Lahaina.

The Marquesas Islands suffered from smallpox in 1862 when two Peruvian ships returned thirty islanders, some of whom were infected. An epidemic spread quickly and killed one-quarter of the population on two islands in a period of six months.[22]

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[19] Ur Lisiansky, *A Voyage Round the World in the Years 1803, 4, 5, & 6 . in the Ship Neva* (London John Booth, 1814), pp. 111-112.

[20] J. Martin, *An Account of the Natives of the Tonga Islands*, 2 vols (London: Johns Murray, 1818), vol 1, p 96.

[21] R Boyd and D R. Steward, *Fiji Census 1921 Report of the Census Taken 24 4 1921*. Council Paper No. 2, Legislative Council (Fiji, 1922), p xii S. M. Lambert, *The Depopulation of Pacific Races* B P. Bishop Museum Spec Pub. No. 23 (Honolulu: Bishop Mus. Press, 1934), p. 16

Rapa, a tiny island south of Tahiti, had an original complement of 2000 people, but it was hard hit by epidemics. Its final decimation occurred in 1863 when a Peruvian ship dumped on it several previously-kidnapped Polynesians who were suffering from both smallpox and dysentery. The epidemic which ensued killed many of the remaining people on the island. The population estimate a year later revealed only 132 people.[23]

The story of the introduction of epidemic diseases to isolated islands was repeated many times, and it is even possible that the missionaries spread sicknesses in their zeal to take the Word to island communities. An example of the introduction of a communicable disease to an atoll can be demonstrated today in the Tokelau Islands, 200 miles north of Samoa. These little groups each have about 500 to 700 people on them, and a boat from Samoa brings them supplies every three months. A few days after the visit of the boat, quite a number of the people come down with a severe cold.

Probably the disease of next importance is measles. Classically one thinks of it as a disease with a rash, when, in reality, it is a severe respiratory disease in which a rash is incidental. It was the involvement of the lungs with secondary pneumonia that caused a high mortality amongst Polynesians. Fiji, a group of islands bordering Polynesia, suffered an epidemic of measles in 1875, in which 40,000 out of a total population of 150,000, died. It had been introduced by a prince who was returning to his homeland on a ship from Australia. Unfortunately thousands of Fijians were holding a conclave in the ancient capital of Levuka at the time. When they dispersed and went to their various districts, they took the disease with them. The epidemic spread with great rapidity for a period of four months, and then burned itself out. The appalling mortality resulted from the ominous acuteness of the disease in people of no resistance. Whole villages were so affected that there was no one left to provide food and water for the sick. Many died of neglect and starvation, lack of knowledge of nursing care, and of despair and apathy. Some believed that the white man had introduced the disease deliberately to exterminate the Fijians in order to obtain their lands.[24]

In a plague epidemic which came from California to Honolulu in 1900, attempts were made to seal the city off from the rest of the island, so that the disease would not be spread. Civilian guards were conscripted to maintain posts on various paths and roads to the city, in order to prevent ingress or egress of potentially diseased persons. The plague was well controlled by these measures and the mortality was less than 100 persons.

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[22] L Rollin, *Les Îles Marquises* (Paris Société d'éditions géographiques, maritimes, et coloniales, 1929), p 269

[23] P. H Buck, *Vikings of the Sunrise* (New York F A Stokes, 1938), p 176

[24] B G Corney, "Epidemic disease in Polynesia," *Brit Med J* , 1884, i. 1000

Other diseases with relatively high mortality were dysentery, mumps, dengue, and tuberculosis. Leprosy was allegedly introduced to Hawaii from the Orient sometime between 1820 and 1848. The native Hawaiians were very susceptible to it and it spread rapidly. By 1883, there were 1,600 cases.[25] The patients were isolated in a settlement on the island of Molokai, on a peninsula at the base of a steep cliff. It was here that the Belgian priest, Father Damien, laboured for years and finally died of the disease himself.

So much for the epidemic diseases. After the various islands were invaded by outsiders, many upheavals occurred which resulted in further depopulation. The French settled in Tahiti in 1842, and many wars between the French settlers and natives resulted in loss of life, pillaging of plantations, and neglect of the planting of crops. There was avarice of natives for foreign goods, and hoarding ensued. Prostitution and alcoholism were common. In Hawaii, the cutting of sandalwood, as ordered by kings and chiefs, disrupted the daily lives of natives. Many young men were taken for crews for ships seeking whales, furs, mother-of-pearl, and guano. Blackbirding, or kidnapping natives from islands, became a regular practice. The Peruvian and Chilean blackbirders took island men to collect guano from the islands off their coasts. In 1862, a thousand natives from Easter Island were kidnapped. Ninety percent died. European governments were able to effect the return to Easter Island of the 100 survivors. Unfortunately, smallpox came with them and killed fifty percent of the rest of the population. Sir Peter Buck estimated that there were 3,000 to 4,000 Easter Islanders originally; in 1875, there were 111 left.[26]

Changes in the daily life of natives, sometimes as the result of the imposition by missionaries of clothing and walled-in houses, had a physical effect. Restrictions on native practices like headhunting and ancestor worship were not replaced by any activities appealing to the natives, and this caused them to lose interest in life. It diminished the powers of the chiefs, and it broke down the family structure. Death rates from apparently trivial illnesses were high. In addition, dislocation of young men caused reduction in birth rates.

The Maoris of New Zealand were particularly victimized. After the Treaty

[25] A. C Heffenger, "Letter from Honolulu, H. I" *Boston Med & Surg J.*, 1883, *109*· 44

[26] Buck, *op. cit.* (n 23 above), p 223

of Waitangi in 1840, in which the Maoris ceded their sovereignty to the British Crown, land became the prime object of the white settlers, who sought its rich stores of timber and flax, and its meadows for sheep raising, and they obtained it in one way or another. A few laws made to protect the natives were never enforced. The Maoris' resistance to an act of purchase, imposed unjustly by the government in 1860, resulted in a five-year war in which the British used 20,000 troops, and artillery and vessels. Three million acres of native land were confiscated. The Maoris saw the white man growing in power in the land while they felt themselves to be passing away. A mood of profound depression resulted. Lack of interest in life led to disregard of provisions for the future. Through subtle interrelations of mind and body, in a people more than usually imaginative and suggestible, it led to physical deterioration, and affected birth rates. The Maori population continued to decline until the end of the century. Reform did not come until after 1895.[27]

The accounts of the Maoris impress upon one that despair and bewilderment prevailed among them, and prominently affected their outlook and attitudes. Some of their responses may have resembled those of American soldiers captured by communists in the Korean War, who gave up and died for no apparent physical reason.[28]

The greatest depopulation in Polynesia occurred probably in the first fifty years after contact with foreigners. The low point was in the pandemic of influenza in 1918, in island groups that are relatively isolated. In Hawaii, however, the "sliding way of death," as O. A. Bushnell describes it, has continued unabated, until now there are practically no pure Hawaiians left. Considerable intermarriage with other races has also diminished the number of pure natives.

In communities a little off the beaten track, like Tonga and Samoa, repopulation is occurring rapidly. In Tonga, the numbers have doubled since 1919, and in Western Samoa, the birth rate is one of the highest in the world, over fifty per thousand. One half of the people living in Western Samoa are under fifteen years of age. Indeed, the prospects of raising food to feed these new generations, or acquiring the purchasing power to buy food to feed them, for these struggling small independent nations, is bleak at best. Fortunately some efforts at population control are meeting with success in places like Fiji, where a vigorous campaign by the health authorities is in progress. Eventually, progress will probably be made in Polynesian communities as well.

[27] H Belshaw (ed), *New Zealand* (Berkeley Univ of Calif Press, 1947), pp 60-61

[28] W Mayer, *U S News and World Report*, 1956, *40* 56-62, F24

ANATOMIST OF INFAMY: AUGUST HIRT*

ERNEST LACHMAN

The crimes of the Nazis, which began with "euthanasia" of institutionalized mentally retarded or deranged German patients and then extended to the killing of more than ten million civilians, are now a part of history and may well be remembered beyond the thousand years Hitler prophesied for the Nazi Reich. Less well known is the involvement of German physicians and scientists in ruthless experiments on the inmates of concentration camps in the name of "medical science." Telford Taylor, chief counsel for war crimes, confirmed that the victims were "in the hundreds of thousands"; and only a small number survived. Most, as attested by Taylor and other witnesses, were either slaughtered outright or died in the course of inhuman medical experiments.[1] Of the 90,000 physicians in Germany, approximately 350 were involved in "medical crimes."[2] Human experiments of unimaginable brutality, in contravention of every international and national law, were practiced: exposure in pressure chambers with sudden compression and decompression; immersion in ice water until body temperature reached 28° C, resulting in immediate death; investigations with poisoned bullets; forced drinking of salt water; transmission of contagious diseases (virulent typhus, tuberculosis, gas gangrene); exposure to war gases (mustard and phosgene); sterilization of both sexes; bone operations such as shortening of legs; and other crimes too numerous to mention.[3] There was also the development of the new science

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* This narrative of the later life and crimes of August Hirt is based on the protocols of the *Trials of the Major War Criminals* (T M.W C.) before the Nuremberg International Tribunal and the record of the *Trials of War Criminals* (T.W C.), known as the "Medical Case," vols 1 and 2, before the American Tribunal, also in Nuremberg, as well as the scant book and journal literature on these crimes and numerous personal communications from institutions and medical friends Particularly helpful was the information furnished by Dr. Connor Johnson, Professor of Biochemistry, University of Oklahoma Health Sciences Center, who, in 1971, spent his sabbatical leave in Strassburg and had been supplied with a list of questions by the author, the major part of which he was able to answer. All these contributions are gratefully acknowledged.

[1] *Trial of the War Criminals (T W C.) Before the Nuremberg Military Tribunals,* "The Medical Case," Nuremberg, October 1946-April 1949, vol. 1, p. 27

[2] Alexander Mitscherlich and Fred Mielke, *Medizin ohne Menschlichkeit,* 2d ed. (Frankfurt a M and Hamburg: Fischer Bücherei, 1960), p 13.

[3] *Ibid.*, pp. 20-236 See also Alexander Mitscherlich and Fred Mielke, *Doctors of Infamy The Story of the Nazi Medical Crimes,* transl from the German (New York: Schuman, 1949); Editorial, *JAMA*, 1946, *132* 714-15; Andrew C. Ivy, "Nazi war crimes of a medical nature," *Federation Bull.*, 1947, *33*. 129-31 and 132-46,

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of "thanatology," which sought to find the quickest, cheapest method to exterminate the largest number of people.[4]

These extreme barbarities are exemplified in the inhuman experiments by the German anatomist, Professor August Hirt (1898-1946). He sacrificed more than a hundred innocent people, eighty-six of whom were killed to augment his anatomy museum in Strassburg.

August Hirt was born in Mannheim, Germany, on April 29, 1898, studied medicine at the University of Heidelberg, receiving his M.D. degree in January 1922. He served as an assistant, then as second prosector, in the Department of Anatomy at Heidelberg, under Professors Braus, Kallius and Hoepke. He became a member of the faculty of this department in 1925, and was appointed assistant professor in 1930. During the Nazi regime Hirt rose to the following posts: in 1936, Director of the Department of Anatomy at the University of Greifswald; in 1939, Professor (Ordinarius) and Director of the Anatomy Department in Frankfurt a.M.; and around 1940, Professor and Director of the Anatomy Department at the University of Strassburg, a politically expedient appointment to a predominantly French School.[5]

Hirt's research before 1936 concerned the comparative anatomy of the autonomic nerve supply of organs and culminated in the chapter, "Comparative Anatomy of the Sympathetic Nervous System," in the *Handbook of Comparative Anatomy of the Vertebrates* (edited by Bolk, Goeppert, Kallius and Lubosch), published in 1934. His later field of research dealt with fluorescent microscopic observations under vital staining, a procedure that he called "intravital microscopy under fluorescent light." He published a number of papers on this topic and planned to write a "completely new anatomy of living organs under fluorescent light."[6]

Politically inactive before the Hitler era, Hirt joined the Nazi party,[7] and later became a member of the S.S. (his serial number in the S.S. was

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Eugen Kogon, *Der S S.-Staat Das System der Deutschen Konzentrationslager* (Europäische Verlagsanstalt, 1946), pp 170-90

[4] Kenneth Mellanby, "Medical experiments on human beings in concentration camps in Nazi Germany," *Brit Med J.*, 1947, *I* 148-50, Mitscherlich and Mielke, *op. cit* (n 2 above), p 169, Editorial, *op cit* (n 3 above), p. 715;

[5] Letter from Dr Weisert, Archivist of the University of Heidelberg, to Lachman, 25 April 1974, Letter from E. Brand, representing the Martyrs' and Heroes' Remembrance Authority (Yad Vashem), Jerusalem (Israel), to Lachman, 10 April 1974, letter from Dr K Holldack, Neukölln-Berlin, who searched the German literature on Hirt's scientific investigations, to Lachman, 5 June 1974 Also Kürschner's Deutscher Gelehrten-Kalender, 1940-41, p 740

[6] Mitscherlich and Mielke, *op cit.* (n 3 above), p 83

[7] Letters from Professor H. Hoepke, Professor and Chairman Emeritus, Department of Anatomy, Heidelberg, to Lachman, 30 April 1965, 23 June 1965

100414).[8] He ingratiated himself with Nazi authorities, was a personal friend of Himmler, and corresponded with Hitler.[9] He was rapidly promoted in the Nazi hierarchy and achieved the rank in the S.S. of Sturmbannfuehrer, equivalent to Major.[10]

In communications from various sources and in the political play by Rolf Hochhuth (*The Deputy*),[11] Hirt is characterized as a loud-mouthed, jovial backslapper. Before the Nazi period he was sociable and friendly with medical students,[12] but in Strassburg he lectured in uniform and conspicuously displayed his revolver. French speaking students called him "délirant" (delirious) and were terrified by his rude, boorish behavior. He favored German students and discriminated against Alsatians.[13] Hochhuth, in *The Deputy*,[14] has him appear under his true name in a bowling scene with prominent Nazis such as Eichmann. Hochhuth states that Hirt cultivated a Swabian accent to ingratiate himself with S.S. superiors and describes him as barrel-chested, gargantuan, a hard drinker, and a repulsive cynic with the head of a vulture and a bullet-scarred mandible. In the play Hirt brags to his friends about a planned skeletal collection (the basis of his crimes), and in alcoholic rapture reveals his cherished wish to hear, on the day of final victory, Bach's B-minor Mass in the Strassburg cathedral and to show the Führer his skull collection.

In 1942, Hirt became associated with the "Ahnenerbe," a Himmler creation founded in 1933 for the study of race and heredity and to promote self-glorification of the northern race of Indo-Germans.[15] Hirt was appointed director of one of its sub-branches, the "Institute for Military Scientific Research." Secretary-general of the "Ahnenerbe" was Wolfgang Sievers who was later brought to trial as a war criminal and executed. Hirt escaped and was never legally prosecuted. At the trial of Sievers facts about Hirt's crimes were brought out.[16] The protocols of the trial of war criminals in the so-called "Medical Case" revealed a letter, submitted by Hirt in 1941 to

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[8] Letter from Brand to Lachman, *op. cit.* (n. 5 above).

[9] Personal communication from Johnson to Lachman (see footnote to title).

[10] Letter from Brand to Lachman, *op. cit.* (n. 5 above)

[11] Rolf Hochhuth, *Der Stellvertreter* (Hamburg: Rowohlt, 1963); English title, *The Deputy*, transl. by Richard and Clara Winson (New York: Grove Press, 1964)

[12] Letter from Hoepke to Lachman, *op. cit.* (n. 7 above).

[13] Personal communication from Johnson to Lachman, *op. cit.* (see footnote to title).

[14] Hochhuth, *op. cit.* (n. 11 above), German ed., pp. 28-54

[15] Mitscherlich and Mielke, *op. cit.* (n. 2 and n. 3 above).

[16] *Trial of the Major War Criminals (T.M.W.C.) Before the International Military Tribunal*, Nuremberg, November 1945-October 1946, vol. 20, pp. 517-26. T.W.C., *op. cit.* (n. 1 above), vol. 1, pp. 54-56, 738-58, and vol. 2, pp. 179, 261-62.

Himmler's adjutant, declaring the desirability of procuring a collection of Jewish skeletons, particularly those of "Jewish Bolshevik Commissars." The letter in the following official translation read:

> We have a nearly complete collection of skulls of all races and peoples at our disposal. Only a very few specimens of skulls of the Jewish race, however, are available with the result that it is impossible to arrive at precise conclusions from examining them. The war in the East now presents us with the opportunity to overcome this deficiency. By procuring the skulls of the Jewish Bolshevik Commissars, who represent the prototype of the repulsive, but characteristic subhuman, we have the chance now to obtain a palpable, scientific document.
>
> The best, practical method for obtaining and collecting this skull material could be handled by directing the Wehrmacht to turn over alive all captured Jewish Bolshevik Commissars to the Field Police. They in turn are to be given special directives to inform a certain office at regular intervals of the number and place of detention of these captured Jews and to give them special close attention and care until a special delegate arrives. This special delegate, who will be in charge of securing the "material" has the job of taking a series of previously established photographs, anthropological measurements, and in addition has to determine, as far as possible, the background, date of birth, and other personal data of the prisoner. Following the subsequently induced death of the Jew, whose head should not be damaged, the delegate will separate the head from the body and will forward it to its proper point of destination in a hermetically sealed tin can, especially produced for this purpose and filled with a conserving fluid.
>
> Having arrived at the laboratory, the comparison tests and anatomical research on the skull, as well as determination of the race membership, of pathological features of the skull form, the form and size of the brain, etc., can proceed. The basis of these studies will be photos, measurements, and other data supplied on the head, and finally the tests of the skull itself.[17]

Hirt's proposals, which had the complete support of Himmler who put his full authority behind them, were executed with minor modifications. The preliminary work of anthropological measurements, casts, and photographs was carried out in Auschwitz by S.S. Captain Dr. Bruno Beger, a representative of the "Ahnenerbe," on selected inmates of the concentration. He examined 115: 79 male Jews, 30 Jewesses, 2 Poles, and 4 Asiatics. In the scene from Hochhuth's play mentioned above, Hirt suggests to a prominent member of the German heavy industry, on his way to Auschwitz in search of slave labor, that he might visit Dr. Beger to observe inmates being prepared for shipment to Dr. Hirt and interesting (presumably Jewish) skeletons.

Due to the danger of spreading epidemics, the program was limited to 86 prisoners transferred from Poland across Germany to the Alsatian concentration campt Natzweiler-Struthof, near Strassburg, where Hirt took over. All the prisoners were Jews, but there is no record that their political activities as "Bolshevik Commissars" were taken into account in their selection. Joseph Kramer who commanded the concentration camp and was former commander of the Belsen camp—where he was known as the "Beast of Belsen"—testified that he received instructions from Dr. Hirt and a half-pint of cyanide salts with which to kill the prisoners in the newly built gas chambers. Hirt gave particular instructions that the heads of the prisoners should not be damaged. The gruesome details of the murder of the 86 male and female victims are in the records of the trials. Their bodies were transported to the anatomy department in Strassburg and embalmed.[18] A French embalmer at the Anatomical Institute, Henry Henripierre, confirmed Kramer's testimony and the date, August 1943, of the delivery of the bodies. He noted that they were warm on arrival, there was absence of rigor mortis, and

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[17] T W. C., *op cit.* (n 1 above), vol. 1, pp. 54-55

> their eyes were wide-open and shining. They were red and bloodshot and were popping from their sockets with traces of blood around the nose and mouth.

Warned by Hirt to keep his mouth shut or be killed, Henripierre nevertheless copied and hid their tatooed prison numbers. He realized that they were freshly killed and, in the males, observed the absence of the left testicles which had been removed and sent to the laboratory of Dr. Hirt. He testified that there were three shipments of bodies: 30 females and 56 males.[19]

When the Allied Armies were approaching Strassburg a year later, Sievers and other Nazi authorities, worried about the discovery of the bodies, approached Himmler's adjutant for instructions regarding those still stored in tanks in the basement of the Anatomical Institute. Sievers informed the adjutant that Hirt would be able to dismember and deflesh the bodies, making them unrecognizable.[20] It was first decided to destroy all evidence, but with temporary improvement in the military situation, the decision was rescinded. Erroneously, Sievers informed his superiors on October 21, 1944, that in compliance with orders received the collection had been completely dissolved. There exists in the protocols a very cynical letter from Sievers to Hirt, dated January 20, 1945, in which he describes the interest of the French and English authorities in Hirt's skeletal collection and his tremendous satisfaction that all clues to this project have been destroyed so that the enemy has been deprived of evidence.[21] Although Hirt had ordered his embalmers to dismember the bodies and cremate them, they were unable to complete the task. Henripierre revealed that many cadavers in the tanks remained undissected or partly dissected to give the impression that they were routine dissecting material. To quote the protocols:

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[18] *Ibid*, p. 740.

[19] Mitscherlich and Mielke, *op. cit.* (n. 3 above), pp 86-88.

[20] T.M W.C, *op cit.* (n. 16 above), vol. 20, pp. 517-526.

> The pictures of these corpses and of the gas chambers in the Natzweiler concentration camp, taken by the French authorities after the liberation of Strassburg, tell the grim story of this mass-murder more vividly than witnesses or documents ever could.[22]

Hirt fled across the Rhine two weeks before the Allied troops arrived in Strassburg.[23] For two weeks he tried to run the department by telephone. While other departmental records were found intact, the records of Hirt's victims were not discovered. The remains of many of the prisoners left by Hirt in the storage tanks were, unknowingly, subsequently dissected by French medical students. Other bodies were buried by the French, after identification had been tried. The rumor in Strassburg in the early 1970s was that Hirt may have gone to South America. Shirer quotes sources that Hirt, before he left Strassburg, boasted that he would never be caught alive. Shirer was uncertain at the time of his writing whether Hirt was alive or not.[24] According to one personal communication he was last seen in Tübingen around 1945 or 1946.[25] But court authorities in Frankfurt a.M. stated in 1963 that he died in 1964 in a village in the Black Forest,[26] Whether of natural causes, suicide, or at the hand of arresting authorities is not known.

The attorney general in Frankfurt a.M. informed me that Hirt's skeletal collection was still sub judice as recently as 1970, in the trial of Dr. Beger and his associates;[27] and in a letter of June 1974 he stated that the case is now closed and the files (15 volumes) deposited at the "Institut fuer Zeitgeschichte in Munich.[28]

Leaving aside the question of Hirt's unprecedented inhumanity, of what scientific value were his pseudo-scientific "anthropological" studies? Evidently he believed and wished to demonstrate to Nazi rulers that the

[21] Mitscherlich and Mielke, *op. cit.* (n. 2 above), p. 181

[22] T.W.C., *op. cit.* (n. 1 above), vol. 1, p. 741.

[23] Personal communication from Johnson to Lachman, *op. cit.* (see footnote to title)

[24] W. L. Shirer, *The Rise and Fall of the Third Reich* (New York Simon and Schuster, 1960), pp 1274-79

[25] Letter from Hoepke to Lachman, *op. cit* (n 7 above)

[26] Letter from Brand to Lachman, *op. cit.* (n 5 above)

[27] Letter from Attorney General Metzner, Frankfurt a M. to Lachman, 2 May 1974.

[28] Letter from Mr. Wiese, Prosecuting Attorney, Frankfurt a M, to Lachman, 26 June 1974

skulls of "Jewish Bolshevik Commissars" would expose the criminality of "subhumans" by their external characteristics. He was doing a study in phrenology.

Phrenology is, and was in Hirt's day, regarded as a completely discredited pseudo-science. It was introduced around 1800 by Franz Joseph Gall and developed by his followers. Cesare Lombroso (1838-1909) applied it to criminal anthropology, claiming that people of a criminal nature display physical characteristics, particularly of the skull, which are partly due to atavism, i.e. reversal to a primitive evolutionary stage of development, or to degeneration. These characteristics are supposed to allow an expert to detect criminal tendencies.

Hirt was not only a heartless killer; he also participated in cruel live experiments at the Natzweiler concentration camp. From 1942 to 1944, he supervised and actively took part in experiments with mustard gas which were ordered by Himmler and the Luftwaffe. The involuntary subjects were political prisoners: Russians, Poles, Czechs, and some Germans. The liquid poison was applied to the forearm, and ten hours later burns appeared and spread over the whole body. Some of the experimental subjects became partially blind, others suffered unbearable pain, and eight of the 30 subjects died with lung and other organ destruction. Experiments were also carried out in gas chambers where prisoners inhaled the poison for a set period. Of 150 about one-third died. Other mustard gas experiments were carried out by injection of the poison, resulting in death of all victims. About 220 prisoners were exposed to these various experiments with mustard gas ("lost gas," as the Germans called it), resulting in fatal outcome in about 50 cases. None of the subjects had volunteered.[29] Apparently Hirt also participated in virulent typhus inoculations of non-immunized prisoners.[30]

A statement in an editorial of the *Journal of the American Medical Association* pointed out that no scientific benefit of any significance accrued to mankind from these experiments which were conducted by concentration camp physicians with such brutality, torment, and the murder of thousands of innocent human beings.[31]

How was it possible that a man who had been absolutely non-political,[32] and seemed to follow the pattern of a typical German medical academician, could be transformed into a ruthless, brutal mass murderer? Lacking personal acquaintance with Hirt and a family history, we can only reach some conclusions by analogy with like-minded Nazis.[33]

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[29] T.W C., *op cit* (n. 1 above), vol. 1, pp 318-32, Mitscherlich and Mielke, *op. cit* (n 2 above), pp. 166-73.

[30] Mitscherlich and Mielke, *op cit* (n. 2 above), pp. 166-73

[31] Editorial, *op cit*. (n. 3 above), p. 715.

[32] Letter from Hoepke to Lachman, *op. cit*. (n 7 above)

Mitscherlich and Mielke, in a second German edition of their original work, emphasized that these crimes were committed by people of normal endowment who had acquired scientific expertise and desirable positions in German society. With the advent of the Nazi cult, they discarded humanitarianism for world-destroying and demonic action.[34] In Hirt's case there is the added incriminating fact that as director of an academic institution and a member of the Nazi party, he could not have been under any life or position endangering pressure forcing him to become a cog in the wheel of Nazi atrocities. It may be assumed that it was unmitigated ambition that turned him from legitimate anatomical studies to unscientific anthropology, i.e. phrenology. Thus he ingratiated himself with the Nazi hierarchy and accepted their ideas, rewards, recognition, promotions, and an increase in socio-economic standing.

Was Hirt insane at the height of his criminal career? He was certainly not insane in the legal sense. Eichmann and other Nazi criminals who had been examined by American and Allied psychiatrists were pronounced sane. The concept of insanity in a society where moral and spiritual values have lost their meaning is itself meaningless.[35] Hirt revealed symptoms of paranoia when he conducted classes dressed in a Nazi uniform, armed with a revolver which he displayed ostentatiously before his students. Was he sick? A secret letter exists in the files of the S.S. (German elite corps) which records his hospitalization for cystic lung disease, pulmonary hemorrhage, and circulatory asthenia.[36]

The writer Elie Wiesel, who had himself been confined to a concentration camp and subjected to Nazi brutality, finds it impossible to understand how leading members of the Nazi elite corps were able to go on with monstrous experiments and killings day in and day out and then go home to their families, tend their gardens, water their flowers, or be overwhelmed by the beauty of a landscape. Had the conscienceless killers been savages the shock of their deeds might be less. But they were well educated members of upper or middle class families: doctors, lawyers, philosophers, sociologists, and even theologians.[37]

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[33] Kogon, *op cit* (n 3 above), pp. 344-61, Shirer, *op cit* (n 24 above); Henry V Dicks, *Licensed Mass Murder. A Socio-Psychological Study of Some S.S Killers* (New York. Basic Books, 1972), pp 230-32, Eugen Davidson, *The Trials of the Germans* (New York: Macmillan, 1966), pp 580-86.

[34] Mitscherlich and Mielke, *op cit* (n 2 above).

[35] Thomas Morton, "A devout meditation in memory of Adolf Eichmann," *Ramparts*, 1966, *5*· 8-10; Dicks, *op cit*. (n 33 above), pp 230-32

[36] T.W C., *op. cit* (n 1 above), vol. 1, p 748

At the first meeting of the revived German Anatomical Society in 1950, no regretful word was said about the vicious deeds of August Hirt and his co-workers. However, when the president of the Society read the role of the 49 anatomists who had died since the last meeting in 1939, Hirt's name was omitted, as it was from the roster of members of the Society. Hirt had become a non-person.[38]

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[37] Elie Wiesel, *One Generation After*, English transl. by L. Edelman and Elie Wiesel (New York: Random House, 1965).

[38] *Anatomischer Anzeiger* (Suppl.), 1950, *97*. 1-5; 246-52.

NEW LIGHT ON THE FLEXNER REPORT: NOTES ON THE AMA–CARNEGIE FOUNDATION BACKGROUND

HOWARD S. BERLINER

In the course of conducting research for a dissertation dealing with the role of philanthropic foundations in the promotion of scientific medicine in the United States, I have come across some material relevant to the Flexner report of 1910, which sheds new light on several aspects of the dynamics of the report. Sources for this new material include the Minutes of the Council on Medical Education of the AMA meetings; the newly opened Pritchett papers at the Library of Congress; the archives of the Carnegie Foundation; and the Abraham Flexner papers at the Library of Congress.

The first mention of the Carnegie Foundation found in the records of the AMA Council on Medical Education is contained in a speech by James H. Kirkland, Chancellor of Vanderbilt University, entitled "Controlling General and Medical Education in the South." In this speech, presented at the Third Annual Conference of the Council on Medical Education held in Chicago, April 29, 1907, Kirkland says:

> Twenty years ago, Vanderbilt University adopted the policy of . . . placing conditions for entrance that were practically identical with those maintained in the first class institutions of the East. We did that in 1887, and reduced our students with one blow . . . by about one-half. We have sacrificed something for our faith in educational work; and yet, gentlemen, perhaps no one fact can bring before you more forcefully the general condition of things in the southern states than this, that in this years, 1907, twenty years after we have made our fight and twenty years after we have established our standard, the Carnegie Foundation for the Advancement of Teaching issued a bulletin defining the preliminary educational requirements necessary for a high grade college in fourteen units, and the Carnegie Foundation cited our course alone in all the South as meeting those requirements . . . . The Carnegie Foundation for the Advancement of Teaching has caused a shudder to run through all these institutions in the South by the publication of that report that I alluded to a while ago. If the council would inspect and tell the truth about all our institutions, point out their defects, point to their equipment and give the facts as regards them, and as regards their manner of teaching, the moral force that would be exerted by such an inspection would be a tremendous power and would have an uplift that could not be calculated or realized. That, sir, would be more efficient in the long run than an attempt by too drastic legislation to secure results that might work disaster to the cause we all have so dearly at heart. (Loud applause.)[1]

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[1] James H Kirkland, "Controlling General and Medical Education in the South," Third Annual Conference of the Council on Medical Education of the AMA, Chicago, April 29, 1907

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At this same meeting in 1907 the results of the AMA's first inspection of medical schools were presented. Arthur Dean Bevan, chairman of the Council on Medical Education, reported later:

> As a result of the report of this first inspection a great wave of improvement in medical education swept over the country. Fifty schools agreed to require by 1910 or before, at least one year of university physics chemistry and biology and one modern language as a preliminary education before matriculating in medicine. Immediately a number of schools as a result of state boards refusing examinations to their graduates, went out of business and it became evident that the 160 schools would be reduced within a short time to probably less than 100. By the time of the report of the second inspection in 1910, the number of schools had been reduced from its high water mark of 166 to 126, a reduction of forty, almost 25 per cent.[2]

Within a year of this meeting Bevan contacted Henry S. Pritchett, President of the Carnegie Foundation, regarding cooperation between the two organizations in the reform of medical education.

It is clear from the above that the reform of medical education was proceeding rapidly and that there was a good deal of self-reform within the profession.[3] Reminiscing in 1928, Bevan noted the Council's agreement that if "we could obtain the publication and general approval of our work by the Carnegie Foundation for the Advancement of Teaching it would assist materially in securing the results we were attempting to bring about."[4]

The Carnegie Foundation for the Advancement of Teaching was set up in 1905, ostensibly to grant retirement pensions to college professors.[5] The Foundation decided, however, that since it did not have the funds to grant pensions to everyone who was teaching in what was then called a college, it would have a define exactly what a college was. Included in its definition of a college were certain prerequisites for students who entered (Carnegie units); elimination of religious restrictions in the college charter; faculty in at least six departments, each headed by a chairman with a doctoral level degree; and income-producing endowment of $200,000. By granting pensions only to professors from colleges which met these criteria the Foundation was able to encourage "uplift" within higher education, as colleges tried to pattern themselves in such a way that they would meet the requirements set for

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[2] Arthur A Bevan, "Cooperation in medical education and medical service," *JAMA*, April 14, 1928, *90:* 1175

[3] See for example the most recent studies in medical self-reform: Robert P. Hudson, "Abraham Flexner in perspective American medical education 1865-1910," *Bull. Hist Med*, 1972, *46*· 545-561.

[4] Bevan, *op. cit* (n 2 above), p 1175

[5] Howard J. Savage, *Fruit of an Impulse Forty-five Years of the Carnegie Foundation* (New York: Harcourt & Brace, 1953).

Carnegie pensions. In 1909, the President of the Foundation, Henry Pritchett, persuaded its trustees to conduct studies of professional education in hopes of drastically reforming its nature. Since this was a major departure from the Foundation's original mission, it deserves some analysis.

The larger purpose of foundations, in general, has been to foster social stability and ameliorate class antagonisms. The increasing shift to industrial capitalism from rural agrarianism, the growth of the proletariat as a class in the U.S., and the increasingly large wave of immigration, all posed problems that traditional hierarchical modes of social control could not handle. One attempt to overcome these problems was the spread of a higher education based on science and technology. The new college graduates, along with skilled craftsmen and artisans, former merchants and a new managerial stratum, in effect formed a new class in American society—a middle class—acting as a buffer between the growing proletariat and the capitalist class. In an annual report devoted in part to professional education in the U.S., Pritchett said that "a hundred years ago ministers were the educated men of their communities and their power was in proportion," and he extended this characterization to the other professions as well.[6] The crown of the new middle class would be the professionals—doctors, lawyers, engineers, ministers, etc. if they could be restored to their former prominence. This former influential status would be regained by drastically reducing the number of professionals to include only the new breed, trained and versed in science, who would naturally fill positions as civic and community leaders and spokesmen. Pritchett's attitude toward professional education thus fit in very well with that of the leadership of the AMA: medical education had to be made scientific (which would regain for the professional public respect and status) and the numbers of physicians had to be reduced (so there would be no petty competition and so that physicians could deal with the larger problems of the community). While the AMA saw these moves resulting in professional prestige and increased income, the Carnegie Foundation saw them resulting in increased social stability through the amelioration of class antagonisms.

This being the case, Pritchett impressed on his board the need for reform of the professions. He wanted to start off with the legal profession, but was apparently rebuffed by the legal professional organization. When an unsolicited invitation to survey medical education came from the AMA, Pritchett immediately accepted. That the two organizations worked very closely with each other can be gleaned from the following extract from the Council on Medical Education minutes of a meeting on December 28, 1908, at which Pritchett and Flexner were present:

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[6] David N. Smith, *Who Rules the Universitites An Essay in Class Analysis* (New York: Monthly Review, 1974); Henry S. Pritchett, "Standards of professional education in the United States," *Third Annual Report of the Carnegie Foundation for the Advancement of Teaching* (New York: The Foundation, October 1908), p 161.

> He [Pritchett] agreed with the opinion previously expressed by members of the Council that while the foundation would be guided very largely by the Council's investigations, to avoid the usual claims of impartiality no more mention should be made in the report of the Council than any other source of information. The report would therefore be, and have the weight of an independent report of a disinterested body, which would then be published far and wide. It would do much to develop public opinion.[7]

and from this letter of Pritchett to Bevan dated November 4, 1909:

> In all this work of the examination of medical schools we have been hand in glove with you and your committee. In fact, we have only taken up the matter and gone on with the examination very much as you were doing, except that as an independent agency disconnected from actual practice, we may do certain things which you perhaps may not. When our report comes out, it is going to be ammunition in your hands. It is desirable therefore, to maintain in the meantime a position which does not intimate an immediate connection between our two efforts.[8]

Although there was mutual recognition and acceptance of this relationship, when the report finally came out its major criticism from the medical press was that it ignored the work that the Council on Medical Education had done previous to the report—obviously the editors of the other medical journals did not know of the relationship between the two organizations.[9] Some have noticed a less than enthusiastic review of the report in the pages of JAMA, which may be due to a little pique at not being mentioned.[10]

The relationship between the two organizations was, if anything, strengthened in the wake of the report. Thus shortly after the report's publication in 1910, Pritchett wrote to Bevan that:

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[7] Council on Medical Education, AMA Minutes of Meeting, December 28, 1908, in Minutes of the Business Meetings of the Council on Medical Education and Hospitals: 1907-1917, AMA Library, Chicago.

[8] Henry S. Pritchett to Arthur D Bevan, November 4, 1909, Carnegie Foundation Archives; also cited in E. Richard Brown, "Physicians and Foundations The Early Rockefeller Medical Philanthropies, 1901-1925," Ph D Dissertation, University of California, Berkeley, December 1975;

[9] See for example: "The Carnegie Foundation," *New York State J. Med.*, November 1910, *10* 483-484; "The Report of the Carnegie Foundation on Medical Education," *Med. Record*, June 25, 1910, *77*· 1097.

[10] Carleton B Chapman, "The Flexner Report by Abraham Flexner," *Daedalus*, Winter 1974, *103* 106 The editorial is in *JAMA*, 1910, *43* 1948-1949

> I have been a little amused at the effort which has been made in a good many quarters to identify the Foundation in this matter with the so-called medical trust, which I understand to be mainly the American Medical Association and your Committee on Education. Of course, this sort of cry does not disturb me at all.[11]

It apparently did not disturb Bevan either, who said in a later response:

> You say that you are amused that the effort has been made to identify the Foundation with the so-called "medical trust". I can only say that as a part of the American Medical Association that we feel very much flattered by such an association. I think that both the foundation and the medical association can only be strengthened by attacks of this kind.[12]

The cozy relationship began to wear thin in the later part of that decade when Pritchett began to disagree with the policies of the AMA regarding their ranking of medical schools.[13]

Given this relationship between the two organizations, the seemingly oddest part is the position of Abraham Flexner. It is not well known that in order to get his foundations approval to hire Flexner, Pritchett had to put up a strong fight. This is casually mentioned in Flexner's biography of Pritchett and mentioned in somewhat more detail in Savage's history of the Carnegie Foundation.[14] A reasonable question to ask is why was Flexner chosen to do the study, and the most likely answer to this question, in the absence of direct evidence, is that Abraham Flexner was a graduate of Johns Hopkins and the brother of Simon Flexner, at that time President of the Rockefeller Institute. Flexner would therefore have good relations with the model institutions for the report. In addition William H. Welch was elected to the Presidency of the AMA in 1909 thus further cementing the relationship between Hopkins, the Rockefeller Institute and the AMA. It should also be noted that Simon Flexner and Welch had put together a draft of what medical education should become, for the Rockefeller organization in 1907, and the report that emerged under the title of the Flexner report is very similar to that draft in many respects.[15]

While in almost all of the recent literature on the Flexner report, Abraham

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[11] Henry S Pritchett to Arthur D Bevan, June 18, 1910 (Carnegie Foundation Archives).

[12] Arthur D. Bevan to Henry S. Pritchett, December 17, 1910 (Carnegie Foundation Archives)

[13] See Brown, *op cit*. (n. 8 above)

[14] Abraham Flexner, *Henry S Pritchett A Biography* (New York: Columbia Univ Press, 1943), pp 109-110, Savage, *op cit* (n 5 above), pp 105-108

[15] Howard S. Berliner, "A larger perspective on the Flexner Report," *Internat J Health Services*, Fall 1975, 5 573-592 It should be noted that this 1975 paper was submitted before the research presented in the present paper was completed Therefore, some conclusions reached in the 1975 paper, especially on pp 588-589, are re-evaluated in the present paper

Flexner is portrayed as something of a genius and expert at putting together a medical educational system in the United States, this was not the impression of people at the time he was working. Flexner had started his inquiries in January 1909 and it was not much later that Pritchett had to write in the following vein to several people:

> Before asking Mr. Abraham Flexner to begin work here in connection with the study of professional education, I consulted with a number of persons concerning his ability and character. Since he has begun work, however a number of criticisms have come to me about him, generally in the way of casting some doubt upon his judgement and indicating somewhat erratic tendencies. There appears also to have been something at Harvard which caused friction. I shall appreciate it if you will give me confidentially a statement with regard to these matters, which shall go no further than myself.[16]

and similarly:

> . . . He [Flexner] impressed me as an able fellow, and on the testimony of his friends I had no question that he was a man of high character. Within the past few days, however, a good many criticisms have come to me concerning him, something to the effect that he is erratic and hard to get along with and somewhat uncertain in judgement.[17]

Conclusion and New Directions

After carefully reviewing all of the recent literature on Flexner and the Flexner report one thing becomes immediately apparent—the Flexner report is fast becoming a cul de sac for historians of medical education. The report has received attention far out of proportion to its actual contribution to medical education and the constant dwelling on the report serves only to mask the real dynamics of the period and the inner reasons for the changes occurring in medical education. A far more important line of study is in the area of how the recommendations of the Flexner report were implemented in medical schools, and how the nature of the medical profession was changed. It should not be forgotten that the Flexner report did not, by itself, create changes in medical education, rather it was the money poured into medical education by the large foundations. Hollis estimates that between 1910 and 1932 the large foundations poured over $150,000,000 into medical education and that this money probably attracted an amount three or four times as large as itself from local sources and smaller foundations.[18] A start is being made by trying to locate the basic reforms of medical education within the context of events occurring in the larger society during the "progressive era."[19] This work is only in its initial stages and must be continued.[20]

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[16] Henry S. Pritchett to William T Councilman, January 22, 1909 (Henry S. Pritchett papers, Box 1 Correspondence, 1908-1913. Library of Congress).

[17] Henry S. Pritchett to Cyrus Adler, January 22, 1909 (Pritchett papers, *op. cit.*)

[18] Ernest V Hollis, *Philanthropic Foundations and Higher Education* (New York: Columbia Univ. Press, 1938), p. 217.

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[19] For a revealing view of the changes occuring in the legal profession at this time, remarkably in tune with the changes in medicine, see Jerold S. Auerbach, *Unequal Justice Lawyers and Social Change in Modern America* (New York Oxford Univ Press, 1976).

[20] Examples of this type of study include the dissertation of E. Richard Brown (n 8 above), Stephen J Kunitz, "Professionalism and social control in the Progressive Era. the case of the Flexner Report," *Social Problems*, October 1974, *22*. 16-27; Gerald E. Markowitz and David K Rossner, "Doctors in crisis: a study of the use of medical education reform to establish modern professional elitism in medicine," *American Quarterly*, March 1973, *25*· 83-107, as well as my own article cited earlier (n. 15 above) and the forthcoming book by E. Richard Brown, *The Rockefeller Medicine Men Medicine and Capitalism in the Progressive Era* (Berkeley: University of California Press [in production])

THE CONTRIBUTIONS OF ALEXANDER GRAHAM BELL AND THOMAS ALVA EDISON TO MEDICINE*

DAVID A. E SHEPHARD

The U.S. bicentennial year—1976—was a peculiarly appropriate year in which to consider the contributions to medicine of two great Americans, Alexander Graham Bell and Thomas Alva Edison. This year was the centennial of Bell's great invention, the telephone; it was also the centennial of the opening of Edison's industrial research laboratory at Menlo Park, New Jersey—the first of its kind in North America; and 1876 marked the beginning of the period when technological advances, particularly the telephone and the incandescent electric light, began to change the pattern of social activity quite radically. In the last quarter of the 19th century there were many great advances in medicine, and, during this time, when Bell and Edison were at the zenith of their powers—they both became 50 in 1897—these two master technologists made their own important contributions to medicine. Their contributions were both specific and general: specific, in that these contributions were discrete and tangible; general, in that, if medicine be concerned essentially with the improvement of health in the widest sense, these contributions may be said to have greatly and inestimably benefited the health and happiness of mankind.

Bell and medicine.[1] Bell, two of whose forbears had practised medicine, at various times in his life had many interests in medicine.[2] He was interested in neonatal asphyxia and resuscitation (he invented a "vacuum jacket" for artifical ventilation);[3] the treatment of neuralgia; the diagnosis of diabetes (he himself had diabetes and probably died from it in 1922, ironically 1 year after Banting and his associates showed that diabetes could be treated with insulin); devices to help the blind detect obstacles; breathing under water; prevention

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* Read at the annual meeting of the American Association for the History of Medicine, Galveston, Texas, May 13, 1976.

[1] Many of Bell's interests were recorded in his notebooks, held in the Library of Congress Much of the material for this essay is derived from the Bell archives

[2] D A E. Shephard, "Alexander Graham Bell, Doctor of Medicine," *New Engl. J Med*, 1973, *288* 1166-1169

[3] A R Henderson, "Resuscitation experiments and breathing apparatus of Alexander Graham Bell," *Chest*, 1972, *62* 331-316

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of thirst; and eugenics and heredity. Remarkable as these interests were, they did not directly influence medical practice—but four other interests certainly did. These were his work on audiometry, his great understanding of the problems of the deaf, his invention of the telephone probe, and his recommendation that radium enclosed in vials be used in the treatment of deep-seated cancer.

The audiometer. Bell perfected the telephone in 1876. It soon had numerous applications. One was audiometry. In 1878 Hartmann[4] in Germany, and Hughes[5] in England, applied the principles of the telephone to the testing of hearing. Bell, not surprisingly, also worked on the audiometer and in his notebook for December 21, 1879, he drew a "Plan of Electrometer—and Audiometer" and noted that his instrument provided "a measure of the power of the ear applied to the telephone." The importance of the audiometer was that it enabled Bell to demonstrate for the first time that there were variations in degree of deafness: that some of those considered simply "deaf" were, in fact, hard of hearing rather than stone-deaf and that they could and should be educated accordingly.

The problems of the deaf. Bell's work with the deaf may well have been his greatest contribution to medicine. In Bell's day most people considered it a waste of time to teach deaf children to speak: all deaf persons were grouped together, and the ancient view that muteness inevitably accompanied deafness largely prevailed. Bell showed that this view was false. He showed, both through his own teaching and through his forming the American Instructors of the Deaf (later the American Association to Promote the Teachings of Speech to the Deaf), that there was an alternative to the common, silent lip-reading method. Bell advocated the oral method.

This method helped de-isolate the deaf; he worked hard to integrate them into society. He succeeded. One need but mention what Bell did for Helen Keller, his pupil. In her autobiography Keller[6] wrote of her first meeting with Bell that it was "the door through which I should pass from darkness into light, from isolation to friendship, companionship, knowledge, love"; it was appropriate that she should dedicate the book to Bell as one "who has taught the deaf to speak and enabled the deaf to hear speech from the Atlantic to the Rockies."

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[4] A Hartmann, "Eine neue Methode des Horprüfung mit Hülfe elektrischer Ströme," *Arch. f Anat u Physiol* (Physiol Abteil), 1878.

[5] D. E Hughes, "On an induction-currents balance, and experimental researches made therewith," *Proc Roy Soc* , 1879, *29* 56-65

[6] H Keller, *The Story of My Life* (Garden City, N Y Doubleday, 1954), p. 34

The telephone probe. Before the discovery of x-rays in 1896, it was extremely difficult to locate foreign bodies that lay at all deep to the skin. Bell achieved fame by developing the telephone probe for this purpose. In 1881 he used its prototype, the induction balance, in an attempt to locate the bullet that had wounded President Garfield. The bullet, which lay near the spleen, was, however, too deep for detection and Bell was unable to help the President's physicians. Soon after Garfield died, though, Bell perfected the telephone probe and this was used frequently in the period 1881 to 1896, and also thereafter, to detect metallic foreign bodies. The principle of the telephone probe was explained in a monograph published by Bell[7] in 1882:

> . . . A fine needle . . . is connected to one terminal of a telephone, and the other terminal makes contact with a plate . . . preferably of the same material as that composing the needle. Place this metallic plate . . . against the surface of the patient's skin and thrust the needle into that portion of the body where the bullet is believed to be lodged. When the point of the needle makes contact with the surface of the bullet . . . a galvanic battery will be formed naturally within the body, the two poles of which are respectively the leaden bullet . . . and the metallic plate. Under these circumstances a click will be heard from the telephone each time the bullet is touched by the needle.

It was in recognition of Bell's work on the telephone and on its applications in medicine, particularly the telephone probe, that Heidelberg University in 1885 conferred on Bell an honorary doctorate of medicine.

Use of radium in the treatment of cancer. Soon after the discovery of radium in 1898, radium was used for the treatment of cancer, particularly superficial lesions. The treatment of deeper lesions was difficult with techniques of the day, because of damage to healthy tissues nearby. Bell recognized that some of the effects of radium were similar to those of x-rays, which were administered by using Crookes' tubes. Bell made a useful suggestion to his own physician, in a letter written in 1903.

> The Crookes' tube from which the Röntgen rays are emitted is . . . too bulky to be inserted into the middle of a mass of cancer; but there is no reason why a tiny fragment of Radium sealed up in a fine glass tube should not be inserted into the very heart of a cancer, thus acting directly upon the diseased material. Would it not be worth while making experiments along this line?

Bell's physician, Dr. Z. T. Somers, recognized the originality of this suggestion and recommended publication of the letter. Publication in *American Medicine,*[8] among other journals, influenced at least three physicians who treated cancer with radium. One of these was Dr. J. Morton, considered the first American radiologist.[9]

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[7] A G Bell, *Upon the Electrical Experiments to Determine the Location of the Bullet in the Body of the late President Garfield and upon a Successful Form of Induction Balance for the Painless Detection of Metallic Masses in the Human Body* (Washington. Gibson Brothers, 1882).

[8] Z T Somers, "The uses of radium," *Am Med*, 1903, *6*. 261

Edison and medicine.[10] Edison, like Bell, had numerous medical interests. These ranged from the homespun and trivial polypharmaceutical liniment, Polyform, through a practical and useful safety lamp for miners, to his greatest contribution: the vast array of medical and surgical equipment that came out of his laboratory and the factories of the various Edison companies. Of Edison's many medical contributions three are of particular interest: ionotherapy, fluoroscopy and practical aspects of the application of x-rays, and manufacture of medical and surgical equipment.

Ionotherapy for gout. In 1890 Edison[11] published "An account of some experiments upon the application of electrical endosmose to the treatment of gouty concretion." This interesting paper almost amounts to a Victorian vignette of Edison, scientist-technologist supreme. It illustrates three facets of Edison's genius.

The 1890 paper is an example of Edison's ability to tackle almost any problem and his delight in doing so where others—in this case, physicians—had failed. In the 1880s lithium salts were given orally for gout, but transport of lithium to sites of gouty concretions was uncertain. Edison introduced a new form of treatment, ionotherapy; by means of this technique lithium ions were "driven" electrically through the skin and, by appropriate placement of the electrified chemical solutions, reached the tophi. It was an original suggestion[12] and one that was referred to by physicians interested in ionotherapy, particularly the Frenchman, Leduc,[13] who introduced it clinically in 1900.

Edison's paper on gout illustrates also his intelligent application of principles established in other disciplines—in this case, in electricity and chemistry—to a discipline in which he was not trained—in this case, medicine; it also illustrates his familiarity with the scientific method as applied to clinical investigation: initial experiments using sheep bladder were followed by experiments on a healthy subject and, once the efficacy of the technique had been demonstrated, by treatment of a patient with gout.

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[9] E. R. N. Grigg, *The Trail of the Invisible Light* (Springfield, Ill. Charles C Thomas, 1965), p. 778.

[10] Much of the material for this essay is taken from the Edison archives, Edison National Historic Site, West Orange, N.J.

[11] T. A. Edison, *An Account of Some Experiments upon the Application of Electrical Endosmose to the Treatment of Gouty Concretion* (Berlin. L. Schumacher, 1890)

[12] F. H. Garrison, *History of Medicine*, 4th ed. (Philadelphia W. B. Saunders, 1929), p. 656

[13] S. Leduc, *Electric Ions and Their Use in Medicine*, transl. R. W. MacKenna (London· Rebman, 1908), p. 55

Fluoroscopy and researches into the application of x-rays. When Edison died in 1931—he survived Bell by 9 years—an obituary notice in the journal *Radiology* made clear how profoundly Edison had influenced the development of the young specialty of radiology. "His example of service and self-sacrifice," the notice ran, "has been inspiring to all professions and none appreciate it more than the Radiological Society of North America."[14] Just as Bell had fully earned his honorary MD so Edison had earned the plaudits of his medical contemporaries.

Edison made three important contributions to radiology.

1. *Research into substances that fluoresce under the influence of x-rays.* Röntgen's discovery of x-rays was made in November 1895. The news of this discovery reached North America by cable on January 7, 1896. According to W. H. Meadowcroft, and one time Edison's assistant and secretary, the master was the first to recognize the importance of the cable and he started research into x-rays immediately.[15] One of his first tasks was to find a substance that would, with great reliability, fluoresce under the influence of x-rays. Röntgen had used barium platinocyanide; Edison, after investigating upward of 1800 substances, found that calcium tungstate was superior to any other compound.[16] Its use became widespread in radiologic work.

2. *Invention of the fluoroscope and manufacture of x-ray equipment.* Like many others, Edison wanted to develop an instrument that would make visible the effects of x-rays. Independently three men—Salvioni in Italy, Magie in the United States, and Edison—developed a similar piece of apparatus. Salvioni's[17] was known as the cryptoscope; Magie's,[18] the skiascope, and Edison's,[19] the fluoroscope. The fluoroscope differed from the other two in making use of calcium tungstate in preference to barium platinocyanide. The fluoroscope became an established piece of equipment and gave rise to the term *fluoroscopy.* The fluoroscope was an origional piece of x-ray apparatus, invented by Edison himself. It was one of many pieces of apparatus that Edison's industrial research laboratory and several manufacturing companies put on the market. X-ray apparatus noted in catalogues of Edison's day included mention of such devices as the Edison

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[14] Obituary: "Thomas Alva Edison," *Radiology*, 1931, *17*, facing p 1060

[15] Meadowcroft's statement was made in answer to an inquiry from P. M. Hickey. Grigg, *op. cit.* (n 9 above), p 13.

[16] T A Edison, "Experiments with Roentgen rays," *Electrical Engineering*, 1896, *21* · 305.

[17] E Salvioni, "Investigations on Röntgen rays," *Nature*, 1896, *53*. 424-425.

[18] W F Magie, "The apparatus and its use," *Amer J. Med Sci* , 1896, *111* 251-246

[19] T A Edison, "Further experiments in fluorescence under the Cathode Ray," *Electrical Engineer*, 1896, *21*. 340.

Focus Tube, the Edison Ruhmkorff Coil, the Edison Instantaneous Air-Break-Wheel Apparatus, the Edison-Lalande Battery in addition to the Edison Fluoroscope. As a manufacturer Edison was efficient: when the U.S. Navy ordered an x-ray apparatus for the U.S.S. *Solace* during the Santiago campaign of 1898, one was made available within 48 hours.[20]

3. *Influence of physicians interested in radiology.* Edison was soon recognized as an expert in the field of x-rays. He received many requests for help from physicians. One was W. J. Morton, who wrote to Edison on February 20, 1896, as follows:

> My dear Mr. Edison:
>
> Would you think it too great an imposition if I asked you to let me have one of your Crookes' tubes? . . . I saw it reported that you would be willing to let some few responsible workers have one of your tubes and assure you if you can let me have one that I shall be most grateful as I much desire to get down to work in surgery and away from any other form of experimentation.
>
> Yours most sincerely,
> Wm. J. Morton

Edison, the inventor, was in great demand to render assistance in many fields of endeavour, not the least of which was medicine.

The electric light and medicine. Edison's greatest achievement was the invention of a practical electrical incandescent lamp in 1879 and the development of an associated power supply. This achievement changed medical practice. Hospitals and physicians' offices were well illuminated for the first time; routine medical and nursing procedures could be conducted under a steady and nonpolluting light; delicate operative procedures could be performed with greater safety; and some of the body's recesses could now be inspected with the newer, safer endoscopy. The benefits of Edison's electric lighting system were thus endless, in medicine and in all other areas of social activity. After 1879 countless patients would benefit immeasurably from Edison's great invention.

Conclusion: Bell, Edison and the effects of technology on medicine. The introduction of the electric light changed the pattern of social life immeasurably. So too did the introduction of the telephone. Technology determined the way people lived, in a manner that would become characteristic of the 20th century.

Bell and Edison made specific contributions to medicine but their contributions had a wider, more general influence too. Derry and Williams have said that "the greatest indisputable benefits of modern technology are perhaps . . . the revolutionary changes in the practice of medicine and surgery"—changes that are too general to discuss here. Derry and Williams[21] made another observation, though, which I would like to quote:

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[20] Anon., "The Edison portable cabinet x-ray outfit," *ibid.*, 1898, *26:* 29-296

> Health and happiness are closely related, and it would be difficult to deny that a more generous and varied diet, better protection against cold and damp indoors and out, a higher standard of cleanliness, and other changes in living conditions that have been facilitated by modern technology, are directly conducive to happiness as well as health. Moreover, invention has widened immeasurably the use that can be made of leisure: how would Bacon, who believed "that reading maketh a full man," have regarded the potential benefits that nineteenth-century revolution in illuminants which brought efficient artifical light for the long winter evenings into the homes of the masses?

The contributions of Alexander Graham Bell and Thomas Alva Edison to medicine were remarkable. How remarkable is most certainly a matter for historians to consider.

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[21] T. K. Derry and T. I. Williams, *A Short History of Technology. from the Earliest Times to A D 1900* (Oxford, Oxford Univ. Press, 1961), p. 710.

NOTES AND COMMENTS

INFLUENZA 1918-1919: A FURTHER STUDY IN MORTALITY

ROBERT S. KATZ

The possible reappearance of the 1918-1919 influenza virus produced considerable interest in the history of the great pandemic. An earlier paper by the author attempted to provide an explanation of the curious pattern of young adult mortality from influenza during this period. Subsequent research has unearthed additional material which seems to substantiate the hypotheses which were presented.

It was suggested that the excessive mortality in the twenty-to-forty age group could be explained by an analysis of the social and political trends of that era in American history. The massive immigration of the late nineteenth century, coming sometime after the prior epidemic of influenza in America, and composed mainly of Italians, Russians, and Poles, was a critical factor. The bulk of these immigrants were shown to have been in the twenty-to-forty age group and came from rural environments. They did not have earlier exposure to epidemic influenza, a disease encouraged by crowding, and, upon arrival in America, lived in congested tenement conditions. Lacking any sort of immune protection, they were affected by and died in greater numbers from this particularly virulent strain of influenza.[1]

The problem in the initial study was that the only evidence discovered which definitely linked immigrant backgrounds with high mortality was the work of Winslow and Rogers, in Connecticut.[2] They demonstrated lower than expected mortality in persons of native, Irish, English, and German stock, and higher than expected mortality, particularly of young adults, in Russian, Austrian, and Polish stock, with an extraordinarily high death rate among Italians.[3]

In order to document more fully the theory outlined above, it would be useful to have studies in addition to that of Winslow and Rogers and data from

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[1] Robert S. Katz, "Influenza 1918-1919 a study in mortality," *Bull Hist Med.*, 1974, *48* 416-422.

[2] C. E. Winslow and J F Rogers, "Statistics of the 1918 epidemic of influenza in Connecticut," *J Infect Dis.*, 1920, *26:* 175-216.

[3] *Ibid*

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states other than Connecticut. Certainly, evidence from other eastern urbanized states would be of considerable significance in this regard.

In relation to the state of Connecticut, newly-found information in the records of the Visiting Nurse Association provides more evidence on the incidence of the epidemic. Greenberg's statistics show the comparatively low incidence of influenza among persons of English, Irish, and German stock (the products of the so-called "old immigration") and the very high incidence of disease in those of Italian and Russian stock.[4] Berman, in an anecdotal report of his extensive private practice experience in Hartford during the epidemic, notes that the fatalities were generally among the twenty-to-thirty-five age group and that Jewish, Russian, and Italian patients were the hardest hit by the disease.[5] Thus, other studies from Connecticut also show that mortality was greatest among the new immigrants.

In regard to states other than Connecticut, the work of Armstrong, in Framingham, Massachusetts, showed that there was about four times as much influenza among the Italians as among the Irish. He noted that the mortality of Italians was far out of proportion to their percentage of the population.[6]

Vaughan studied influenza in Boston, by districts. He found that the lowest incidence was in the Irish tenement district and the highest was in a Jewish population. An Italian tenement district was also severely affected.[7] This is particularly interesting as it shows that the Irish, who presumably were descended from the immigrants of the mid-nineteenth century, were not as severely affected even though they lived in tenement conditions. Perhaps, as suggested, their previous exposure was the key factor in limiting morbidity.

Finally, Eichel's compilation of mortality statistics for New York State, excluding New York City, indicates the disproportionate mortality of urban Italians, Poles, Austro-Hungarians, and Russians, as compared to English, Irish, and Germans.[8] Again, the bulk of the mortality in the new immigrant groups was in the twenty-to-forty age group.[9]

[4] D Greenberg, "Influenza statistics of the Visiting Nurse Association of New Haven," *Publ Hlth Nurse*, 1920, *12* 209-217

[5] H Berman, "Epidemic influenza in private practice," *J A M A* , 1918, *71* 1934-1935

[6] D B Armstrong, "Influenza observations in Framingham, Massachusetts," *Am J Publ Hlth*, 1919, *9*, 960-965

[7] W T Vaughan, *Influenza—An Epidemiologic Study* (Baltimore American Journal of Hygiene Monograph Series, 1921), pp 129-132, 142-143

[8] O R Eichel, *A Special Report on the Mortality from Influenza in New York State (Exclusive of New York City) During the Epidemic of 1918-1919* (Albany Division of Vital Statistics, 1923), pp 56-65

[9] *Ibid*

After examination of considerable additional statistical evidence, the unusual mortality from influenza in 1918-1919 still appears to be a product of the sociological, political, and geographical considerations of the era. The Italians, Russians, and Poles came to this country after the last wave of influenza. They moved from isolated, rural areas where they were not exposed to influenza, to congested urban tenements where, lacking in immunity, they were easy targets for the disease. The ethnic groups that had been in this country for some time, such as the Irish, had the opportunity to develop immunity and were not affected nearly as severely as the new immigrants. Since most of the vulnerable ethnic groups were composed of persons in the twenty-to-forty age bracket, the mortality was naturally highest in that group. Thus, the unusual mortality from influenza would appear to be related to the immigration patterns of various ethnic groups in early twentieth-century America.[10]

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[10] Katz, *op. cit.* (n. 1 above).

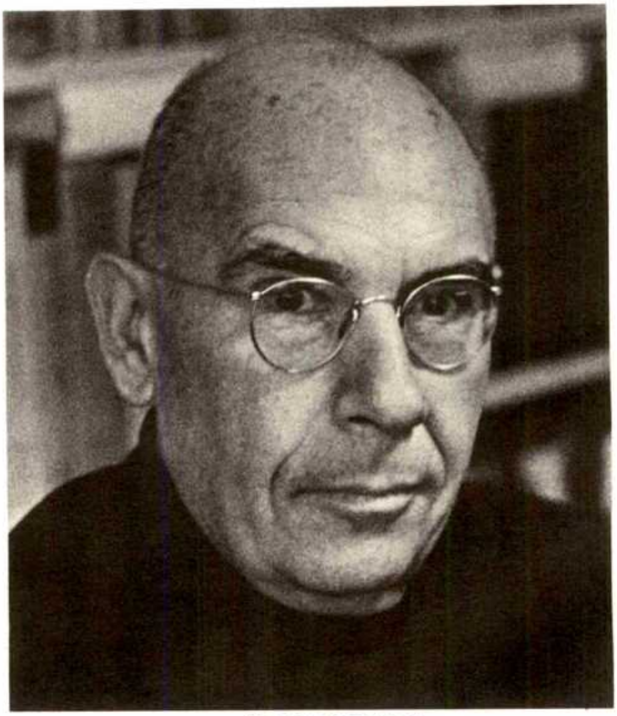
MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

GEORGE ROSEN, 1910-1977

The sudden death of Professor George Rosen, in Oxford, on July 27, 1977, has removed from the American scene a distinguished professor and a past president of the American Association for the History of Medicine (1964 and 1965), from the international scene one who was current president of the International Academy of the History of Medicine, and from the world of public health, national and international, one of its most influential leaders. In 1961 the American Association for the History of Medicine awarded him its William H. Welch Medal "for contributions to the social history of medicine."

Born in Brooklyn 23 June 1910, Rosen was educated in New York, winning his B.S. from the College of the City of New York in 1930. It was surely a monumental irony,



GEORGE ROSEN
Photo by Grace Goldin

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and a grinding and hazardous experience, that he should have gone thence to the Faculty of Medicine of the University of Berlin; and yet, because he received his M.D. in 1935, he studied in what had been one of the most distinguished medical faculties in the world before its sad deterioration in the Nazi era had proceeded too disparately far; difficult as the experience was, it was also rewarding. It was a pleasanter irony that carried him back to a two-year internship in the Beth-El Hospital in Brooklyn (1935-1937) and this was followed by a brief excursion into practice (1937-1942). Concurrently (1938-1942) he was an Assistant on the E.N.T. staff at Beth-El, and for three years (1939-1942) a Clinical Assistant in Otolaryngology at Lutheran Hospital, Brooklyn. (Thirty years later he published a book on the development of specialization in medicine.) Three of the commitments just mentioned are dated as having continued until 1942; to these a fourth and fifth must be added, for he was also Clinic Physician in the Bureau of Tuberculosis, New York Department of Health, 1941-1942, and Junior Health Officer in the Department, 1942-1943. In 1944 he was awarded the Ph.D., Faculty of Political Science, Columbia University. He had already commenced his military service (1943-1946) which was to be chiefly in the Medical Intelligence Division, Preventive Medicine Service, Surgeon General's Office and the European Theatre of Operations. A considerable portion of this work was concerned with the collection and analysis of medical and sanitary data on countries all over the world and the analysis of public health problems.

It is hard to comprehend how George Rosen could have published a good book in 1942, another good book (and this time he did not have his M.D. thesis to translate) in 1943, a third one in 1944 (a book which was eagerly reprinted in 1972) and yet another (on a theme which to this day remains uniquely his) in 1946. His capacity for work was prodigious. Winding up his service in the Medical Corps, he served for a short time (April, 1946-October, 1946) as Health Officer, New York City Department of Health, charged with the duty of preparing a "Manual of District Health Administration." In 1947 he won from the School of Public Health at Columbia his M.P.H. He was District Health Officer, N.Y.C. Department of Health, 1947-1949, and Director, Bureau of Health Education in the Department, 1949-1950. Beginning in 1950 he was for seven years Director, Division of Health Education and Preventive Services, Health Insurance Plan of Greater New York.

Besides appointments at various times as a Visiting Lecturer at Harvard and a Visiting Professor at Yale, Rosen held chairs at Columbia and Yale for a total of twenty-six years. He was Professor of Public Health Education, School of Public Health and Administrative Medicine, Columbia University, 1951-1969; Professor of the History of Medicine, and Professor of Epidemiology and Public Health, Yale University Medical School, 1969-1977. He was within one year of retirement at the time of his death.

In this imposing list of Professor Rosen's activities, we have thus far neglected the one for which he was probably more widely known than for any other—his editorship of the *American Journal of Public Health*. This was not his first editorial chore. He was also, earlier on, editor of *Ciba Symposia*, 1938-1944. In 1933 he had married Beate Caspari, who also received an M.D. degree from the University of Berlin in 1935, and she now began to assist him in his first editorship. Mrs. Rosen's skill, energy and devotion go far to account, one feels, for the incredible number and range of her husband's pursuits, to which her learning and hard work have contributed very substantially. Two children, Paul Peter and Susan Joan, completed the family. In 1946 George Rosen undertook, for the publisher, Henry Schuman, the editorship of a new periodical, the *Journal of the History of Medicine and Allied Sciences*, and continued until 1952, afterwards remaining a member of the editorial board. It was in 1948, then, that he joined the editorial board of the *American Journal of Public Health*, and after nine years became editor. This was an arduous and demanding job and he did much to maintain, and even to elevate, the *Journal*'s standard for sixteen years (1957-1973).

There was hardly a broad aspect of public health which George Rosen did not know profoundly—its history, whether long or short, its present status, and its apparent prospects for the future; and about an enormous number of lesser issues his information was extensive. He was an erudite medical historian. As a commentator on a paper read at a meeting he was formidable; there was always the possibility that he would ad lib a more thorough account of the subject (whatever the subject might be) than the original speaker had been able to muster. Offhand, he was always well prepared, and when fully prepared he was like an army with banners. I think there were times when an experienced audience, recognizing the signs that such a commentary was about to begin, would emit the faintest sigh; but if truly knowledgeable the audience would listen attentively, always to be rewarded. And more often than not the comment would be sharply relevant and brief.

There were few, I think, among his generation of American doctors who could speak with some measure of authority and acceptance to a wider range of audiences than could he. To epidemiologists, to public health people in general (whether in the public sector, in industry or in academia) to physicians of many kinds, to health insurance experts, to psychiatrists, to medical sociologists, to medical journalists, to educators, and to historians of almost every breed (although essentially modern) he was well and favorably known as editor and author: all were ready to read or to listen; most were hesitant to debate. He could not have been described as eloquent—not at least in the usual sense of the word, because rhetoric did not much interest him—but he was splendidly articulate and like Macaulay he had read everything and forgotten nothing.

Rosen was an extraordinarily productive author. He wrote or edited ten books, besides the half a dozen he translated, and at the time of his death his bibliography listed 215 articles; but the list was not complete, for in addition to whatever there may be in press elsewhere, a good paper was then in proof at the *Bulletin of the History of Medicine*—a thirty-seventh to add to the thirty-six of his essays already published by the *Bulletin*, an impressive series beginning in the era of its founder, his good friend, Professor Sigerist, and extending for more than forty years to 1977. Twenty of the *Bulletin* papers appeared in Rosen's first decade as essayist after his return from Europe (1936-1946) but although the pace then slowed a trifle, the articles continued to come out at regular intervals and became, more and more often, weighty and memorable. But of the first three papers, two were already substantial. One of them, "Social Aspects of Jacob Henle's Medical Thought," clearly announces what was to be a characteristic theme, as do several others among the first dozen; four, as further examples, deal with occupational disease, two or three with medical historiography. The very first paper, a short one published in 1936 is entitled, "John Elliotson—Physician and Hypnotist." So that even at the outset, George Rosen's consuming interests—social medicine (he was later to publish a number of studies to define it), sociology, politics, public health, psychology and psychiatry, as well as medical historiography (chiefly from social and sociological viewpoints)—were already apparent. We need, then, to recall that his first book, which had been his dissertation in Berlin (1935), had been a contribution to the history of physiology in the nineteenth century, afterwards published in translation as *The Reception of William Beaumont's Discovery in Europe* (New York, Henry Schuman, 1942).

There followed in quick succession three remarkable short books—remarkable in originality of theme and in the quality of the execution, but remarkable most of all, perhaps, for their undoubted and seemingly perdurable "relevance." If we have witnessed the somber and moving documentary film, "Harlan County," now or recently in the movie theaters, we hardly need to have pointed out to us the undimmed significance for the later twentieth century of *The History of Miner's Diseases: A Medical and Social Interpretation* (New York, Henry Schuman, 1943). *The Specialization of Medicine* (New York, Froben Press, 1944) sprang to life again in an Arno Press reprint in 1972. *Fees and Feebills: Some Economic Aspects of Medical Practice in 19th Century America* (Baltimore, Johns Hopkins Press, 1946) opened yet another vein of historic ore, which has been little worked even yet.

More widely known than any of these has been *A History of Public Health* (New York, MD Publication, 1953) which appeared in Japanese translation as recently as 1973, and *Madness in Society: Chapters in the Historical Sociology of Mental Illness* (London, Routledge and Kegan Paul; Chicago, University of Chicago Press, 1968) which was also a Harper Torchbook in the same year, and which was translated into Spanish in 1974.

Almost immediately after *Fees and Feebills* and before *A History of Public Health* Rosen edited, with Beate Caspari Rosen, *400 Years of a Doctor's Life* (New York, Henry Schuman, 1947) and again a translation was called for, this time into German, in 1956.

It was only recently that Science History Publications of New York gave to George Rosen the opportunity to bring together, in organized array, much of his most valuable work as historian, work which only now has revealed its overall pattern and coherence: *From Medical Police to Social Medicine: Essays on the History of Health Care* appeared in 1974, *Preventive Medicine in the United States, 1900-1975: Trends and Interpretations*, in 1975.

George Rosen was wonderfully efficient. The great diversity of his work concealed, although not very effectively, its fundamental unity of interest and aim. To the very last, his powers quite unweakened, he was alert for what opportunity offered, as shown, for example, by the urban studies which occupied many of his working hours in the last few years. Now and again, but very rarely indeed, he disappointed his friends by planning large projects which failed to come off. Of this the most notable example was his major project for a comprehensive study of the development of government expenditures in health-related areas since World War II: it never materialized. But the real wonder, if we look at him cooly among his compeers and among his juniors, is not what failed of achievement (a trifling proportion of the whole, after all) but what was splendidly completed, on time or ahead of time, what surpassed expectation, and what is showing itself today as work of such lasting value as to demand reprinting. More predictable, it seems to me, that some of the finest work of his peers, I think that Rosen's books will nevertheless continue to have strong appeal to genuine students as genuine repositories of the kind of knowledge they need and value.

Despite a number of interesting programatic essays, I cannot with assurance say of him that he raised as many questions as he answered; it seems to me that he seldom hesitated to supply answers. This is one of the ways in which he resembled Sigerist, whose work he always admired and many of whose preferences and convictions he shared. Sigerist's range was much the wider but his definition of "sociological" was both vaguer and narrower. Both men looked to history, it is clear, to chart paths leading into the future and both (but Rosen much more so) were attracted by themes that had practical implications. From two or three conversations with George Rosen in recent years I believe that he had plans for research and writing in his retirement that would have extended his range and might have opened new vistas for all of us. We shall miss him sorely.

LLOYD G. STEVENSON

GEORGE ROSEN, 1910-1977

Memorical Remarks at the 205th Meeting of the Beaumont Club, Yale University, October 6, 1977

In my brief comments I will not repeat what is so well known about Dr. Rosen's distinguished career, except to acknowledge what is evident to all, his deserved reputation as medical historian and authority in public health. My acquaintanceship with George Rosen extended over only a few short years. It was through a mutual interest in the History of Surgery Lectures that we became friends, and it is through this connection I grew to appreciate some of his remarkable talents.

Typical of his approach to any new venture, he immediately supported the Surgery Lectures with enthusiasm, attending nearly all and giving a number himself, including the 17th annual Samuel C. Harvey Lecture. His presentations established a bench mark of excellence. And when he discussed a paper by a less experienced historian he was warm and considerate, complimenting the presentation and leaving the author with a feeling of gratitude. This was the nature of this superb teacher.

Dr. Rosen had a remarkable way of bringing the past to bear on the present, skillfully putting contemporary issues in historical perspective. The title of his initial editorial on becoming the first editor of the *Journal of the History of Medicine and Allied Sciences*, "What is Past is Prologue," embodied a theme that surfaced time and again in his writings as he related current events to historical antecedents, a reflection of his dual professional interests.

Take his article in which he sought to explain the rebellious youth of the 1960s. "Other times and places have produced rebellious youth, social drop-outs and alienated generations," Rosen wrote, and compared these youths to such groups as the wandering scholars and Goliard poets of the later middle ages, the generation of the German *Sturm und Drang* in the latter half of the 18th century, and later groups. Read his account of the behavior of the physicians under the Third Reich: "The historian of recent German medicine who approaches his subject under the impression that he is dealing with a radically new and unique phenomenon soon finds it difficult to draw any sharp line distinguishing the influence of National Socialism from deep-seated tendencies in the broader tradition of German culture." And surely those who would ground the Concorde would find comfort in his paper on noise pollution. Again he reaches back into history to Pliny the Elder who in the first century A.D. noted that the people who lived close to the roaring cataracts of the Nile suffered from deafness.

Rosen had an uncommon knack for spotting historical gems. For example, more than 100 years after it was written but unpublished, he unearthed a most important first-hand account of French medicine as it was at the beginning of the second quarter of the 19th century. The account was the diary of Dr. Peter Townsend, an American physician visiting Paris in 1828. No second-hand biography could ever match the candid description by that articulate physician of the personalities and manners of the time.

Everything relating to medical history seemed to catch George Rosen's attention. Artistic himself, he was quick to recognize the historical significance of a sketch of the surgeon Pean by Toulouse-Lautrec. Typical of his orderly mind he researched thoroughly the manner in which Toulouse-Lautrec gained entrance to the surgeon's clinic.

George Rosen has left a written record rarely matched by other medical historians. Future historians will judge his record as he judged that of others, and nothing would please this honest man, this first-class scholar, more than a careful survey of his work. There is little doubt that history will accord him a place as one of the foremost medical historians of the 20th century.

Finally, no tribute to George Rosen would be complete without mention of his devotion to his family and friends, particularly Beate, his wife and co-worker and a distinguished physician in her own right. Contentment and happiness at home were reflected in his joyous pursuit of his work.

WILLIAM W. L. GLENN

IN MEMORIAM: EMERSON CROSBY KELLY

The Association has lost another old friend, Emerson Crosby Kelly of Albany, N.Y. who died on May 26, 1977 at the age of 77. From 1950 to 1954 he served as Treasurer of the Association, assisted by his wife Sydney who survives him.

During a lifetime of bookcollecing and activities in medical history, Dr. Kelly produced numerous publications including his *Encyclopedia of Medical Sources* (Baltimore, 1948) and articles in the *Bulletin of the History of Medicine* and the *New York Journal of Medicine*. Between 1936 and 1941 he issued five volumes of *Medical Classics*, reprinting fifty classic contributions to medicine from the time of Hippocrates to the 20th century.

Born in Griffins Corner, New York, Dr. Kelly graduated from Cornell University and the Albany Medical College, where he became a faculty member in 1929. As Clinical Associate Professor of Surgery he turned students' attention to historical and bibliographical as well as surgical topics. Between 1937 and 1960 he offered an introductory course in medical history for medical students. During his lifetime he donated his personal collection of medico-historical books to the Albany Medical

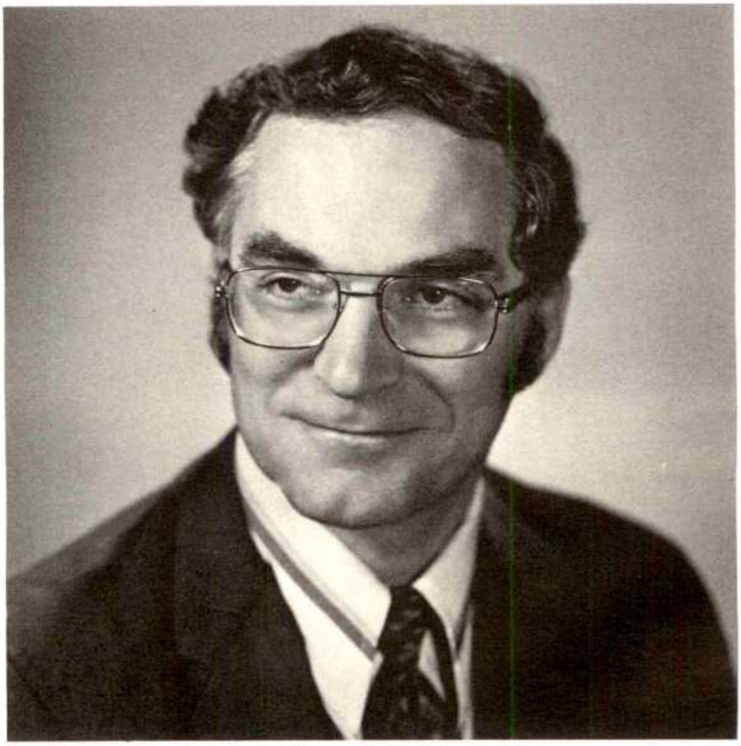
College Library of which he was Director from 1963 to 1965. When the College Library moved into a new facility in 1972, the History of Medicine Room was named for him.

Other honors included election to Alpha Omega Alpha Medical Honor Society, appointment as honorary Vice-President of the Medical Library Association, and being named Professor Emeritus of Medical Library Science. He also served as Historian of both the Albany County Medical Society and the Albany Medical College Alumni Association. In addition he edited surgical terms for Stedman's *Medical Dictionary*. Dr. Kelly's deep interest in and devotion to medical history while carrying on an active surgical practice represents the finest in the non-academic historical tradition. We are grateful for his activities which have enriched us all.

GENEVIEVE MILLER

APPOINTMENT IN THE HISTORY OF MEDICINE

Dr. Charles Gordon Roland has been appointed to the Jason A. Hannah Chair for the History of Medical and Related Sciences in the Faculty of Health Sciences, McMaster University, Hamilton, Ontario. Well-known internationally in the medical field as a medical historian and biomedical communicator, Dr. Roland is the first encumbent of the newly created McMaster position.



For five years (1964-69) one of the senior editors of the *Journal of the American Medical Association,* Dr. Roland has for the last eight years been Professor and Chairman of the Department of Biomedical Communications, Mayo Foundation and Mayo Medical School, Rochester, Minnesota. At the Mayo Clinic he was also Associate Professor and Head of the Section of the History of Medicine, Executive Editor of the *Mayo Clinic Proceedings,* and Editor of the Medical History Department for the journal *Chest.* Dr. Roland is also Secretary-Treasurer of the American Association for the History of Medicine and of the American Osler Society. At McMaster his appointment includes a professorship in the Department of Family Medicine.

Dr. Roland was born in Winnipeg and graduated from the University of Manitoba with a B.Sc. (Med.) and M.D. in 1958. He was in private practice in Ontario at Tillsonburg, 1959-60, and at Grimsby, 1960-64, in family medicine and anesthesiology. He attended the University of Toronto during 1952-54.

There is no precedent at McMaster for the formal teaching of the history of medicine, Dr. Roland said. He hopes initially to introduce some informal teaching of the subject to McMaster medical students, but says it will take some time to establish a fully fledged teaching program. His special field is nineteenth-century North American medical history, particularly Canadian.

ANNOUNCEMENTS

NATIONAL NEWS

Cheiron

The tenth annual meeting of Cheiron: The International Society for the History of the Behavioral and Social Sciences, will be held at Wellesley College, Wellesley, Mass., June 2-4, 1978. Information concerning the program and the submission of papers may be obtained from Dr. Paul T. Mountjoy, Department of Psychology, Western Michigan University, Kalamazoo, Mich. 49008

Galveston

The Moody Library of the University of Texas Medical Branch has established a department of archives and manuscripts. According to Larry J. Wygant, university archivist, the department hopes to assemble non-current records regarding the Medical Branch, private and professional papers of its faculty, alumni, and others, and records of private and professional medical societies. The records eventually will, subject to necessary permission, be available to scholars interested in the history of medicine and the contributions of the Medical Branch to the field.

Johns Hopkins University

The twentieth in the Hideyo Noguchi Lectureship series, which was established in 1929, was delivered on October 20, 1977, by Ilza Veith, Ph.D., D.M.S., Professor and Vice Chairman, Department of the History of Health Sciences, and Professor of Psychiatry, University of California, San Francisco. Dr. Veith, one of whose special interests is the history of medicine in the Far East, spoke on "The Mutual Indebtedness of Japanese and Western Medicine."

Rockefeller Archive Center

The Rockefeller University has made eight grants-in-aid to support projects requiring substantial research in the holdings of the Rockefeller Archive Center for 1977. Grants ranging from $500 to $1,000 are available to gradute students or advanced scholars engaged in similar projects. Inquiries about the program and the open collections at the Center should be addressed to the Director, Rockefeller Archive Center, Hillcrest, Pocantico Hills, N. Tarrytown, N.Y. 10591

St. Louis

On October 4, 1977, ceremonies were held at the Washington University School of Medicine to commemorate the transfer from the Central Institute for the Deaf of the book collection in speech and hearing of Dr. Max A. Goldstein (1870-1941), former faculty member of Washington University and founder of the C.I.D. A program chaired by Dr. Estelle Brodman, Director of the Medical Library, included papers on Dr. Goldstein's biography and his collection, and an exhibit prepared by Marck F. Weimer illustrated the wide range of Dr. Goldstein's interests.

Symposium on Philosophy and Medicine

The Seventh Symposium on Philosophy and Medicine, "Moral Use of New Knowledge in the Biomedical Sciences," will be held at the University of Missouri at Columbia, March 2, 3, and 4, 1978. Major speakers will include Nicholas Rescher, Alasdair MacIntyre, John Duffy and Samuel Gorovitz. For further information, please contact: Prof. William Bondeson, Director, College of General Studies, University of Missouri-Columbia, 420 General Classroom Building, Columbia, Mo. 65201 (314-882-3857); Prof. H. Tristram Engelhardt, Jr., Kennedy Institute, Center for Bioethics, Georgetown University, Washington, D.C. 20057 (202-625-2371), or Professor Stuart F. Spicker, Derpartment of Community Medicine and Health Care, University of Connecticut Health Center, Farmington, Conn. 06032 (203-674-2354).

Appointment

The White House has announced the appointment of Dr. David Musto to the Cabinet-level Strategy Council which was created in 1972 by the Drug Abuse Office and Treatment Act to develop a comprehensive strategy for all drug abuse prevention functions of the Federal Government. Dr. Musto, Associate Professor of Psychiatry and of History, and Program Director of the National Humanities Institute at Yale University, has produced valuable studies of the social history of drug use and persuasively demonstrated the importance of a historical perspective in the formation of public policy.

Award

Dr. Robert P. Hudson, Professor of History of Medicine, University of Kansas Medical School, is recipient of an Amoco Award for Excellence in Teaching.

Job Opportunity

Applications are invited by Indiana University to fill a newly created position in the history of medicine. The position entails a joint appointment in the Departments of History and of the History and Philosophy of Science, as well as an adjunct association with the I.U. School of Medicine. The primary need is for someone to teach undergraduate and graduate courses in the development of the medical sciences, but the candidate should also be attuned to the social and cultural dimensions of medical history and have some interest in cultivating regional history of medicine. Correspondence and applications should be sent to Prof. Frederick B. Churchill, Department of History and Philosophy of Science, 130 Goodbody Hall, Indiana University, Bloomington, Ind. 47401

PROGRAMS OF MEETINGS

College of Physicians of Philadelphia

September 28, 1977. " 'A little of what you fancy may do you good' Opiate Use in 19th Century Britain" (Preliminary Report), Karen Kerner; Discussor, Richard Limoges. "The Man Who Bested the Rotunda and Maternite: A Study of Dr. William Goodell (1929-1894) and His Famous Friends," W. Robert Penman; Discussors, Luigi Mastroianni, Jr., and William Goodell.

November 16, 1977: The Second Richard H. Shryock Lecture, "Physicians and the

Development of Scientific Agriculture—Empiricism to Science, 1731 to 1862," Eugene H. Conner.

Handerson Medical History Society, Cleveland

October 4, 1977: "The History of Homeopathy in Oberlin," A. Clair Siddall.

November 22, 1977: "Some Dimensions of the Battle for Licensure in 19th Century Pennsylvania," Melvin R. Strausbaugh.

Josiah C. Trent Society for the History of Medicine, Durham, N.C.

September 22, 1977: "The History of Medical Politics: The Education and Government of Doctors," Andrew Nadell.

November 3, 1977: "The Virus: A History of the Concept," Sally Smith Hughes.

Mayo Foundation History of Medicine Society, Rochester, Minn.

October 12, 1977: "Early History of Endocrinology," Alexander Albert.

Society of Medical History of Chicago

October 12, 1977: "Murder in the Medical School," John B. Stetson.

November 16, 1977: "Medical Care of the French in Illinois Country in the 18th Century," Emmet F. Pearson.

Washington Society for the History of Medicine, Washington, D.C.

September 29, 1977: "Another Boswell's Diary: Edinburgh's Medical School in the Early 18th Century," Jon Eklund. "Surgeons of Violence: John C. Handy and George E. Goodfellow of the Arizona Territory," Peter D. Olch.

November 17, 1977: "Pity the Poor Surgeons Mate: Medical Environment of the Early U.S. Navy, 1798-1815," Harold Langley. "Fertility by the Numbers: The Motivations of the 19th Century American Birth Statistics," James H. Cassedy.

Washington University History of Medicine Club, St. Louis, Mo.

September 27, 1977: "Early History of Rocky Mountain Spotted Fever," John M. Kissane.

November 15, 1977: "Osler and American Neurology," Joseph Hanaway.

NEWS FROM ABROAD

Germany

The Sixth Interdisciplinary Symposium for Philosophy and Medicine, "Science, Technology and the Art of Medicine," was held in Hamburg, September 1-3, 1977, under the auspices of Dr. Peter Fischer-Appelt. Among the co-directors was Dr. H. Tristram Engelhardt, Professor of the Philosophy of Medicine, Georgetown University.

International Academy of the History of Medicine

New members elected to the Academy in 1977 include Genevieve Miller, Ilza Veith, Guenter B Risse, Dora B. Weiner and William Coleman (U.S.A.); Frederick L. Holmes (Canada); Jean Starobinski and Esther Fischer-Homberger (Switzerland);

Marie Jose Imbault-Huart (France); Bengt Lindskog (Sweden); and Marielene Putscher and Richard Toellner (Germany).

International Congress of the History of Medicine

The 24th International Congress will take place in Plovdiv, Bulgaria, August 20-25, 1978. Congress themes include "History of Medicine in Bulgaria," "History of Military Medicine," "Folklore and Popular Medicine," "Methodologic and Historiographic Aspects of the History of Medicine." Requests for further information about attendance and the presentation of papers should be addressed to Prof. Dr. Vera Pavlova, Organizing Committee, XXVI International Congress of the History of Medicine, Medical Academy II, Belo More St. 8, 1527 Sofia, Bulgaria.

Montreal

McGill University is considering applications for the position of Assistant Professor in History of Health and Medicine. A Ph. D. in history is essential, and an M.D. useful. Applicant should have a working knowledge of French, teaching ability, and be interested in the 18th-20th century social history of health and medicine in Canada, France, U.S., or Great Britain. Duties include teaching and research in an inter-disciplinary department responsible for the social sciences and humanities in the Faculty of Medicine, and a possible cross appointment in the Department of History. For further information write to Dr. Donald G. Bates, Chairman, Department of the History of Medicine, McIntyre Medical Sciences Building, 1655 Drummond St., McGill University, Montreal, P.Q., Canada H3G 1Y6

Zurich

In his annual report for 1976-77, Dr. Huldrych M. Koelbing, Director of the Medical Historical Institute of the University of Zurich, noted that the Institute marked its 25th anniversary in 1976. Among the events commemorating the occasion was a lecture on "Spitalinfektion einst und jetzt" by Dr. Erna Lesky, Director of the Institute of the History of Medicine, University of Vienna.

Urs Boschung, M.D., has been appointed to the newly created post of conservator of the Historical Medical Museum at the University of Zurich. Dr. Borshung, a young Swiss medical historian, is now working as an assistant to Prof. N. Mani at the Institute of History of Medicine of the University of Bonn. He will assume his new post in January 1978.

Award

Dr. Erwin H. Ackerknecht, Director of the Medical Historical Institute of the University of Zurich from 1957 to 1971, was awarded an honorary doctorate by the Medical Faculty of the University of Bern.

BOOK REVIEWS

The Journals and Letters of Fanny Burney (Madame D'Arblay). Volume VI: France 1803-1812, Letters 550-631. Edited by Joyce Hemlow and others. Oxford: at the Clarendon Press, 1975. pp. 469-892. Ill. $33.75

Fanny Burney (1752-1840) was born into the illustrious household of Charles Burney, who did extensive writing on the history of music and moved in London's famous literary circle of the period. At age 26, Miss Burney published *Evelina* anonymously, her first novel, the story of an inexperienced young girl's emergence into society. It immediately caught the public's fancy. Edmund Burke and Sir Joshua Reynolds are said to have sat up all night reading the story. Miss Burney soon acknowledged the authorship of *Evelina* (1778) and she came to be greatly admired in the literary circle of Samuel Johnson. In 1782 she published *Cecilia,* in 1795 *Camilla,* and in 1814, *The Wanderer;* the theme of all Fanny Burney's books was entry into society of young, virtuous female novitiates and their mistakes in the process of acquiring sophistication and maturity. None of her subsequent novels excited as much public interest as did *Evelina*.

At the urging of her family, and perhaps with the belief that attendance upon the Court of the Royal Family would lend her additional distinction, Miss Burney spent the five-year period 1786-91 as a member of Queen Charlotte's household. Though the Queen and King George III received her very cordially, she missed very much her own circle of friends and the opportunity to continue her own productive literary career. She was able to withdraw from the arduous and confining duties of a Lady-In-Waiting in the Royal Service because of failing health. This story in itself should serve to advise all youngsters with important work of their own that its sacrifice and attachment to the service of well-known names is only conducive to unhappiness.

Fanny Burney married General D'Arblay, a French émigré, in 1793. Shortly thereafter, Fanny and her husband moved to France where she, as a foreign alien, was interned by Napoleon from 1802 to 1812, and prevented from returning to England. They had a son, Alexander Charles Louis (1794-1837). Madame D'Arblay survived both husband (1754-1818) and son, and outlived most of her generation.

The letters of Fanny Burney and exchanges with husband, family, and friends in this period have been carefully noted. Fanny Burney apparently was in the habit of reediting some of her correspondence and that circumstance too, item by item, has been carefully notated by the author and her associates. Queen Charlotte apparently continued her pension to Fanny Burney during her isolation in France, a period of financial stringency.

Fanny Burney, very obviously, recognized that her diary and letters would be of interest to posterity. The letter exchanges during this period contain a lot of family, domestic chit-chat obviously of little historic interest, though they do document an interesting period in the social life of literary society in England and France of that era.

It was also a critical period in European history when Napoleon was striving to make

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all of Europe a French state. Nelson's stunning victory over the French fleet at Aboukir on August 1, 1798, established the supremacy of the English Navy in the Mediterranean. On October 1, 1805, Nelson annihilated the French fleet at Trafalgar though he was killed in the action by a sharp-shooter. Napoleon had a string of successive military victories on land, but after Wagram 6 July 1809, Napoleon had few victories to record. His greatest blunder was invasion of Russia in the Summer and Fall of 1812. His ultimate defeat after escape from Elba (1815) to which he had been exiled in 1814, came at Waterloo in June 1815. Six years later, Napoleon succumbed to gastric cancer at St. Helena, and Europe was at peace for a few decades again. Thoughts of this long continued conflict undoubtedly occupied and clouded the minds of all English and French sympathizers of the period, but Madame D'Arblay preeminently a historian of her social set had little to say in her letters of the prevailing issues of the day.

The special justification of the appearance of this review in a medical historical journal concerns the circumstance that Madame D'Arblay was operated upon by Dominique Jean Larrey, Napoleon's famous military surgeon for a "cancer" of the breast September 30, 1811. Larrey had striven to soften the hard mass in her right breast by prophlogistic measures but without success. Dubois, Larrey's associate looked upon the lesion as hopeless, believing there already were internal metastases and extensions of the tumor. The courageous Larrey felt that an effort should be made to save the famous lady who was now 59 years of age. The operation apparently was performed without anesthetic agents of any kind.

Larrey may not have known that in Edinburgh the lectures of Alexander Monro II (1733-1817) to medical students and visitors proposed extensive phlebotomy as an effective antidote to pain. He had first urged this resource in 1779, probably repeated in subsequent lectures throughout the years. There is apparently no evidence extant that Monro II actually implemented the suggestion. Philip Syng Physick of Philadelphia, who was in attendance at Monro II's lectures in the early 1790s, initiated the suggestion upon his return to Philadelphia, and with great success in the reduction of chronic dislocation of joints, not susceptible to repair by any other method. Physick was in the habit of performing phlebotomy up to 2 liters of blood on the patient in the standing posture on both antecubital veins at the bend of the elbow. The patient immediately sank to the floor in syncope, oblivious of any pain during the manipulative procedure. Physick's technique had the advantage over ether or chloroform anesthesia, yet more than four decades away, that it was not only an analgesic agent but that syncope also induced complete muscle relaxation, not obtainable by anesthesia alone unless reinforced with local procaine anesthesia at the site of operation, as designed by George W. Crile, Sr. (1913) which combination of agents he termed anoci-association. Larrey must have known of the anesthetic qualities of whisky. In fact, the American surgeon-sanitarian Stephen Smith, long after effective anesthetic agents were available, elected to use whisky in his operations on substandard risk patients, giving an ounce or more every two hours from 8 in the morning until 3 in the afternoon when his operative schedule commenced, observing that only 10 percent as much ether was then necessary for operative procedures. Surgeons of Larrey's day were more inclined to use phlebotomy as an antiphlogistic device after operation rather than before, "to reduce inflammation."

Joyce Hemlow records the harrowing experience, torment, and agony of Madame D'Arblay in graphic language. The operation was conducted in complete silence because the patient was fully conscious; Larrey's assistant, Dubois, indicated in sign language the progress of the operation which Fanny observed and subsequently described in great detail. She survived the operative procedure 29 years, dying at age 88, raising serious doubts concerning the validity of the diagnosis of malignancy. Hemlow, in fact, relates that Dr. Rocke Robertson, well-known Canadian surgeon, was skeptical of the diagnosis inasmuch as the patient had suffered severe pain for more than a year prior to operation; he pointed out pain of this character is distinctly unusual in cancer of the breast. This reviewer also is inclined to have reservations over the diagnosis in light of the long survival of the patient. If the specimen was preserved, a histologic study, even now 165 years later, would be in order. Pertinent to this suggestion is that histologic studies 106 years after the death of John Paul Jones, famous American Naval Hero, revealed he died of pyelonephritis and not of tuberculosis as often stated.

Steinfeld (1975) has described a mastectomy performed by Zabdiel Boylston of Boston, 28 July 1718, who cut his patient's "whole breast off." The patient survived the ordeal for 39 years, dying at age 75, again raising legitimate doubts over the authenticity of the diagnosis. Another famous breast operation was performed in preanesthetic days by James Syme of Edinburgh (1830) on the Scottish peasant woman Ailie. On completion of the operation she arose from the table, curtsied to the surgeon and the large number of student-spectators, saying she hoped she had not behaved unseemly. John Brown, who participated in the operation as a medical student, related in *Rab and His Friends* 28 years later that the students who witnessed the procedure all cried like children. Ailie succumbed to sepsis a few days later.

Before anesthesia it was customary for the surgeon to employ strong attendants to restrain patients, forcibly retaining them on the operating table. Apparently this was unnecessary in the instance of Fanny Burney and in the Scottish lady, Ailie, who with great fortitude restrained themselves. It is related that when the gruff and ready surgeon Lawson Tait, of Birmingham, read John Brown's notes on Ailie's operation to his medical class that the surgeon's eyes dimmed with tears.*

When Madame D'Arblay returned to England with her family, after long detention in France during the Napoleonic wars, she is said to have kissed her hallowed English soil on disembarking at Dover. Unlike her early years spent in the midst of a lustrous literary circle of friends, all of whom she outlived, Madame D'Arblay's terminal years were spent in relative isolation and neglect. Jane Austen, with a far more modest background, had come on the scene and replaced her in the public fancy. Fanny Burney died almost forgotten, revived by studies pointing out her important contribution to English literature.

It is the fate of many in advanced years who outlast their contemporaries to be relieved of the infirmities of old age only by the permanent anesthesia of death, and so it also came to be with Fanny Burney.

Reviewed by Owen H. Wangensteen, M.D., Regents' Professor of Surgery, University of Minnesota, Minneapolis, Minn. 55455

* The earlier five volumes of Joyce Hemlow's *Diaries and Letters of Fanny Burney D'Arblay* were not accessible to this reviewer although he has more than a nodding acquaintance with some of Fanny Burney's writings as well as Arthur Bernon Tourtel-

lot's *Life and Environment of Fanny Burney, Be Loved No More* (1938); and with the volumes by Austin Dobson (1905) *Diaries and Letters of Madame D'Arblay.*

NANCY STEPAN. *Beginnings of Brazilian Science: Oswaldo Cruz, Medical Research and Policy, 1890-1920.* New York: Science History Publications, 1976. xi + 225 pp. Ill. $12.95.

The institutionalization of science in eighteenth-century England and France, the emergence in the nineteenth century of organized science centered on the German university, and the professionalization of research (exemplified by the conspicuously successful clinical research in medicine at The Johns Hopkins University) in the United States have been well described by Joseph Ben-David in his study of the role of the scientist in society. By the end of the nineteenth century Sir Francis Bacon's dream of scientists and technologists collaborating for the betterment of mankind was being realized in the so-called developed nations. The importance of Nancy Stepan's contribution to the growing literature on the sociology of science lies precisely in her choice of a country whose name is not usually associated with scientific innovation, leadership, or basic research. At the turn of the twentieth century Brazil was a developing nation, and even today falls within the catch-all category of a "third world" power.

In her four centuries as colony and independent nation, Brazil had not been untouched by science or scientists. Portugal's cultural imperialism had stifled the intellectual life of her richest colony. Brazil had proved a fertile research field for scholars, especially in the natural sciences (George Marcgraff, Wilhelm Piso), but few Brazilians had themselves contributed to scientific knowledge. The *Erario Mineral* (Lisbon, 1735) of Luís Gomes Ferreira, based on twenty years residence in Minas Gerais, had been written after the doctor-surgeon's return to his native Portugal. Portugal herself, largely isolated from scientific advances in Europe, provided no encouragement for fledgling scientists. The distinguished international career in medicine of Jacob de Castro Sarmento (*Materia Medica,* London, 1731) was exceptional.

The opening of the ports (1808) and the declaration of independence (1822) heralded in a new era in Brazil which was to witness the establishment of medical schools, public health facilities, medical societies, natural history societies, a military academy, a royal garden, and a plethora of well-publicized scientific expeditions to Brazil. Despite this flurry of activity, the hard fact remained that indifference on the part of Brazilians contrasted with the interest shown by the European scientific community in the potential of Brazil as a field for scientific research. This was exacerbated by inadequate training facilities, the absence of political or financial support for science, and a patriarchal agriculturally-oriented society whose elites had yet to grant social acceptance to the professional scientist. Scientific patronage and the industrial development which had provided the incentive for research in Europe and the United States, culminating in the institutionalization of scientific research, was totally absent in South America. At the collapse of the monarchy (1889), Brazil may be categorized as "colonial," having reached an intermediary position in George Basalla's model for the diffusion of European science to Asia and the Americas.

The unravelling of the warp and woof of social, political, economic, and medical events which saw the transition of Brazil from a position of scientific dependence into the era of independent scientific research epitomized by the Serum Therapy Institute at Manguinhos is the theme of Nancy Stepan's far-reaching monograph. Bubonic plague and yellow fever, together with a gamut of tropical diseases, demanded research and remedies which the microbiological sciences could provide. The declaration of the Republic (1889) led Brazilians to reassess the strengths and weaknesses of their nation. The election of Rodrigues Alves (1902), whose governorship of the state of São Paulo had been distinguished by his concern for public health, to the Presidency ensured support for applied research. Above all, there was the charismatic presence of a then unknown trainee of the Pasteur Institute in Paris, by name Oswaldo Cruz. Within six years Cruz emerged from obscurity as a skilled microbiologist to gain national and international acclaim for the successful sanitation campaign against yellow fever and bubonic plague in the Brazilian capital of Rio de Janeiro during his term as director of the federal department of public health. His direction of the Serum Therapy Instiute (founded, 1900) was to provide a model for scientific institution building in developing nations.

The successful operation of the most innovative and efficient medical laboratory of its day in South America was largely attributable to the role of Cruz as scientific entrepreneur. Without prejudicing the independence of his institute, Cruz gained political support and guaranteed financial backing. Albeit created for a specific purpose, viz preparation of sera and vaccines against bubonic plague, the institute succeeded in expanding its research activities to embrace protozoology, entomology, mycology, helminthology, biochemistry, and physiology. This success was built on three pillars: recruitment of Brazilian students and training them in microbiology; the establishment of a "client relationship" with national and state governments and private agencies; continuation of basic research and applied medicine, and the gaining of "visibility" in the scientific community and a place in the nation's order of priorities. The finest example of this happy marriage between scientific research and national needs was Carlos Chagas' discovery of *Trypanosomiasis americana* in 1908. At the time of Cruz' death in 1917 the Institute had achieved internal stability, had substituted "scientific dependency" by the export of ideas and discoveries, counted a core of researchers, and had contributed to the process of modernization in Brazil. It had nationalized experimental medicine related to Brazilian needs and problems, provided a pool of scientific manpower, stimulated state and federal interest in public health, and had enabled Brazil to benefit from foreign technical aid programs. This success of the Oswaldo Cruz Institute contrasted dramatically with the Bacteriological Institute of São Paulo, a victim of what Stepan refers to as the "applied science trap" and the unfortunate choice as director of Adolfo Lutz, a brilliant pioneer scientist but lacking the administrative flair of Cruz.

From this model the authoress draws lessons for the formulation of policies toward science in developing nations. She rejects the widely-held view that developing nations should pursue the policy of "technology transfer" borrowed from industrial powers and limit themselves to a "service" orientation in the implementation of scientific programs. Stepan advocates the building of a core of research scientists, The format of the catalogue is, in many ways, preferable to that of the incipit list. Here the reader finds a richer bibliographic approach in three separate indices, arranged respectively by incipit, by manuscript listing, and by author of the work. Lindberg has divided the works into two major subject categories: optics and ophthalmology. Within these two sections, entries are listed alphabetically by author, followed by the incipit, and, subsequently, by citations of the manuscripts themselves. Particularly gratifying to the working scholar are four types of generous assistance. Alternative forms and spellings of the authors' names simplify the task of finding and identifying works, an especially difficult problem in the cases of Arab names. In many cases, printed editions of the works follow the manuscript listing, thereby maximizing the availability of texts. Secondary works about the manuscripts are cited, supplying useful research aids. Last, Lindberg's descriptions and commentaries on the entries are informative, lucid, and humanizing.

Lindberg has carefully described the necessary limitations of the work which omits encyclopedias, Aristotelian works and the commentaries on them, as well as most works on artistic perspective, perception, and meteorology. Admitting their importance in the history of optics, he asserts that his catalogue is only a beginning and an effort to stimulate further research. Since his omissions evoke a desire to know the compilations which transmitted both learned and popular theories of optics, it is apparent that he has achieved his goal. A further bibliography citing such major encyclopedic works as Vincent of Beauvais' *Speculum Maius*, available in both early printed and facsimile form, and such intriguing *therapeutica* as the Huntingdon Library's fifteenth-century English leechbook (HM 19079), with its massive section on the eye, should be among the offshoots of this invaluable bibliographic resource.

Reviewed by Judith S. Neaman, Ph.D., New York, N.Y.

Alfred W. Crosby, Jr. *Epidemic and Peace, 1918.* Westport, Conn. & London, Eng.: Greenwood Press, 1976. 337 pp. Ill. $17.50.

Alfred W. Crosby's new book on the influenza pandemic of 1918-1919 has certainly arrived at an appropriate time. In a year when the debate over mass inoculation for "Swine Flu" rages across the country, perhaps it is a good idea to review the events of America's own "black death."

This exhaustively researched work, which relies largely upon contemporary sources, deals primarily with influenza in America. The author rightly devotes much time to a discussion of the effects of World War I on the dissemination of the disease. The role of the massed War Bond rallies and the busy military camps is defined and described. The unceasing demands of the Western Front for more and more soldiers produced the overcrowding of troop ships, and these in turn became perfect breeding grounds for the disease. Crosby gives a harrowing picture of the enormity of these ship-board epidemics and conveys a sense of the helplessness of those who attempted to combat the menace.

and the development of techniques and specialist areas of research appropriate to the individual needs of each nation. Because of the permeability of barriers between basic and applied science in the biomedical field, its emphasis on problem-oriented research, and the interaction of national and international medical groups, this is an area of research and application particularly appropriate to developing nations in search of the establishment of national scientific capability. The successful pursuit of this policy demands the adoption of a system of inter-connecting forward and backward linkages between research, development, and application. Especially within Latin America, the model of the research institute may prove a more appropriate context for scientific research than the university.

Based on extensive research in archival depositories and a thorough reading of the pertinent literature in English and Portuguese, *Beginnings of Brazilian Science* provides an excellent survey of the emergence of the microbiological sciences in Brazil and the circumstances surrounding the foundation of the Oswaldo Cruz Institute. The choice of a historical and comparative perspective is illuminating. On the more general themes of scientific institution building in developing nations and scientific policy formulation and implementation, Stepan has suggested avenues of further enquiry without herself venturing down them. The in-depth study of such critical and complex issues would have been impossible in a book counting less than 150 pages of text. Unfortunately, the introduction of these broader implications intrudes on the central theme and makes for a disjointed narrative and unnecessary repetition and recapitulation. The structure of the developing thesis is excellent and the writing style direct. The result is a thoughtful study in an area unfamiliar to many in the English-speaking world who will no longer be able to gainsay the admittance of Brazil into the "republic of science."

Reviewed by A. J. R. Russell-Wood, D.Phil., Professor of History, The Johns Hopkins University, Baltimore, Md.

David C. Lindberg *A Catalogue of Medieval and Renaissance Optical Manuscripts.* Toronto: The Pontifical Institute of Mediaeval Studies, 1975 (Subsidia Mediaevalia, IV), 142 pp. $7.50.

A growing fascination with optics, stimulated in no small measure by David Lindberg's work in the field, has intensified the need for this first catalogue devoted exclusively to optical and ophthalmological manuscripts of the Middle Ages and the Renaissance. Lindberg has modelled his work on Thorndike and Kibre's *A Catalogue of Incipits of Mediaeval Scientific Writings in Latin*, but his entries extend to manuscripts written during the period from the twelfth through the seventeenth centuries in both Latin and the vernacular languages of western Europe. Like his distinguished predecessors, Lindberg has avoided the pitfall of relying solely upon catalogues for the ascription and dating of the manuscripts. He has, as he notes, consulted the manuscripts themselves whenever possible and, in the process, he has succeeded in correcting a number of long-standing errors in ascription and dating.

Chapters on the response of cities such as New York and San Francisco are included and are particularly intriguing. The San Francisco account is a complete, and at times almost humorous, description of the controversy over the wearing of masks as a means of halting the spread of the infection. A chapter devoted to isolated Alaska contains the book's most chilling paragraphs. Entire native villages were totally wiped out in a few hours by the malady, and the Federal Government did virtually nothing to alleviate the human suffering. A similar, and equally frightening, situation existed in British Samoa, with a death-toll of numbing proportions.

An interesting, but perhaps less successful, attempt is made to place the blame for much of the failure of the Versailles Peace Conference upon the effects of the epidemic. Colonel House, Woodrow Wilson's key assistant, and Wilson himself, were both apparently affected by influenza during the proceedings. Crosby attempts to show that their infirmities led to unsatisfactory negotiating which sowed the seeds of the eventual failure of the Treaty. However, Crosby specifically rejects the work of Edwin Weinstein, who so carefully described Wilson's long-standing neurological problems, in favor of his own views on the effects of influenza. While Crosby does add new dimension to the subject, his evidence is often circumstantial, and his theories are not always as convincing as those supported by Weinstein's carefully marshalled evidence.

In spite of this one flaw, the book is still a success. It undoubtedly is superior to Richard Collier's *The Plague of the Spanish Lady* in presenting the facts of the epidemic in a coherent and organized fashion. While Crosby does not answer any of the old questions about the epidemic, especially those relating to the unusual young adult mortality, the chief strength of the book lies in its evocation of the true horror and desolation produced by this visitation of influenza.

It should be noted that *Epidemic and Peace, 1918,* earned the 1976 American Medical Writers' Association award.

Reviewed by ROBERT S. KATZ, M.D., Department of Pathology, Hospital of the University of Pennsylvania, Philadelphia, Pa.

THE BULLETIN RECENTLY RECEIVED*

Acta Congressus Internationalis XXIV Historiae Artis Medicinae, 25-31 Augusti 1974, Budapestini. Ed. J. Antall, G. Bozinhay and F. Nemethy. Budapest: Museum Bibliotheca et Archivum Historiae Artis Medicinae de I. Ph. Semmelweis Nominata, 1976. vols. Ill. 395 Ft.

American History and Life. Part D, Annual Index, vol. 13. Santa Barbara: American Bibliographical Center-Clio Press, 1976.

Rosa Ballester Añon. *La historia clínica pediátrica durante el siglo XIX.* Analisis de la ciencia y de la práctica pediátrica a traves de las patografías infantiles. Zaragosa: Catedra de Historia de la Medicina, 1977 (Cuadernos Aragoneses de Historia de la Medicina y de la Cinencia, Serie A (Monografías, 1). 272 pp.

Henry K. Beecher and Mark D. Altschule. *Medicine at Harvard: The First Three Hundred Years.* Hanover, N.H.: University Press of New England, 1977. xiv + 587 pp. Ill. $27.50.

W. E. B. Beveridge. *Influenzia The Last Great Plague. An Unfinished Story of Discovery.* New York: PRODIST, 1977. vii + 124 pp. $6.95.

Ernst Philipp Eduard Bischoff. Microscopic Analysis of the Anastomoses between the Cranial Nerves. Translated and edited by Ernest Sachs, Jr., and Eva W. Valtia. Hanover, N.H.: University Press of New England, 1977. ix + 138 pp. Ill.

Michael E. Burke. *The Royal College of San Carlos. Surgery and Spanish Medical Reform in the Late Eighteenth Century.* Durham, N.D.: Duke University Press, 1977. xv + 215 pp. $10.75.

James C. Burrow. *Organized Medicine in the Progessive Era: The Move toward Monopoly.* Baltimore and London: The Johns Hopkins University Press, 1977. ix + 218 pp. $12.95.

Frederick F. Cartwright. *A Social History of Medicine.* London and New York: Longman, 1977. 209 pp. $12.95 (cloth), $7.50 (paper).

A Catalogue of the Rare Book Collection in the Northwestern University Dental School Library. Chicago: Benet Lake Press, 1976. 136 pp. Ill.

Harry F. Dowling. *Fighting Infection: Conquests of the Twentieth Century.* Cambridge, Mass., and London: Harvard University Press, 1977. vii + 339 pp. Ill. $15.00.

John H. Ellis. *Medicine in Kentucky.* Lexington, Ken.: University Press of Kentucky, 1977. vii + 95 pp. $5.95.

The Evan Bedford Library of Cardiology. *Catalogue of Books, Pamphlets and Journals.* London: Royal College of Physicians, 1977. xi + 245 pp. Ill.

John Farley. *The Spontaneous Generation Controversy: From Descartes to Oparin.* Baltimore: The Johns Hopkins University Press, 1977. x + 225 pp. $13.50.

Stephen Jay Gould. *Ontogeny and Phylogeny.* Cambridge, Mass., and London: The Belknap Press of Harvard University Press, 1977. 501 pp. Ill. $18.50.

Henry Guerlac. *Essays and Papers in the History of Modern Science.* Baltimore and London: The Johns Hopkins University Press, 1977. xix + 540 pp. $20.00.

William Harvey *An Antomical Disputation Concerning the Movement of the Heart* *and Blood in Living Creatures.* Translated with introduction and notes by Gweneth Whitteridge. Oxford: Blackwell Scientific Publications; Philadelphia: J. B. Lippincott, 1976. lxii + 142 pp. Ill. $14.00 (soft cover); $29.00 (cloth).

Milton Helpern, with Bernard Knight. *Autopsy: The Memoirs of Milton Helpern, the World's Greatest Medical Detective.* New York: St. Martin's Press, 1977. 273 pp. Ill. $10.00.

Charles de Mertens. *An Account of the Plague Which Raged at Moscow, 1771* (London, 1799). With a long introduction and annotated bibliography by John Alexander. Newtonville, Mass.: Oriental Research Partners, 1977. v.p.

Earl F. Nation, Charles G. Roland, and John P. McGovern. *An Annotated Checklist of Osleriana.* Kent, Ohio: The Kent State University Press, 1976. xii + 289 pp. Ill. $27.50.

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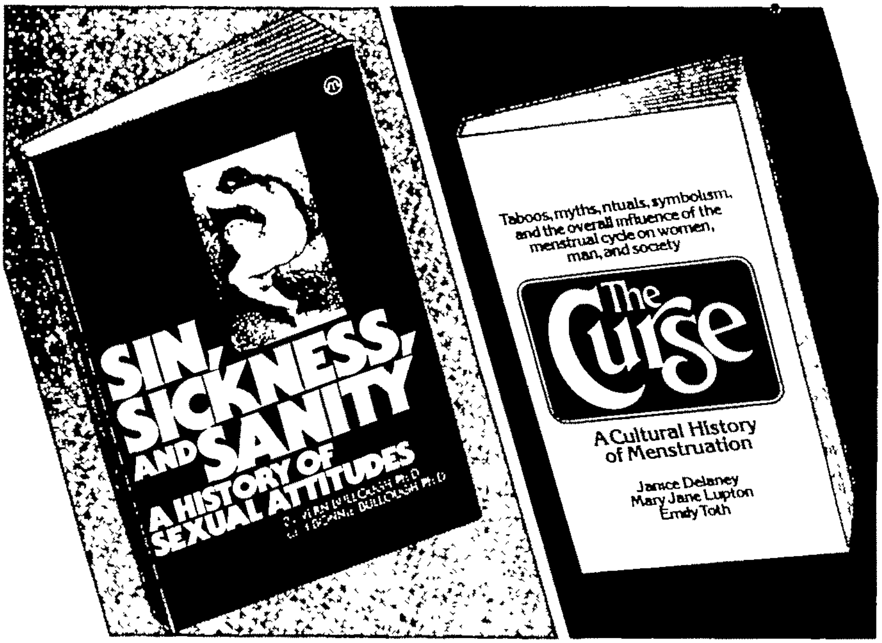
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